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Stimulus Material Format Effects on Juror Sensitivity to Eyewitness Accuracy Factors

A Dissertation submitted in partial satisfaction
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by

J. Marie Hicks

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Dissertation Committee:
Dr. Steven E. Clark, Chairperson
Dr. Carolyn B. Murray
Dr. Robert Rosenthal

The Dissertation of J. Marie Hicks is approved.

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Committee Chairperson

University of California, Riverside

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ABSTRACT OF THE DISSERTATION

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by

J. Marie Hicks

Doctor of Philosophy, Graduate Program in Psychology
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Dr. Steven E. Clark, Chairperson

Eyewitness testimony is a compelling form of evidence, and mistaken identification is a factor in many wrongful convictions (Gross & Schafer, 2012). It is crucial to understand whether jurors can judge the accuracy of the eyewitness identification from the facts of the testimony, and subsequently factor that information into their verdicts. Some research suggests that jurors are good at this task, whereas other research disagrees. The current dissertation includes a meta-analysis of this conflicting literature, followed by an experimental test of juror understanding using video, audio-only, written transcript, and written summary testimony. The meta-analysis data suggested that using written testimony resulted in greater juror sensitivity to factors relating to potential eyewitness accuracy and greater application of that knowledge to verdicts, than was seen using videotaped testimony. Based on these data, it was hypothesized that reading the testimony allows jurors greater opportunity to discern these factors, remember them better, and apply them more efficiently to their verdicts, compared to seeing or hearing the witness provide the actual testimony. This prediction was, in part, supported by the experimental data. Whether the juror saw, heard, or read the testimony, they noted the

key information relating to eyewitness accuracy. They also retained that information, and understood its relationship to the probable accuracy of the identification. However, across stimulus formats, they did not equally apply that information to their verdicts. This dissertation examines those differences and suggests potential explanations for the discrepancies.

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Chapter 1 - Introduction

Overview

Eyewitness identification provides compelling evidence in criminal trials. Gross and Schafer (2012) compiled a report discussing 873 exonerations occurring between January 1989 and February 2012. Focusing specifically on crimes that were very likely to have been committed by strangers, specifically robbery and rape, they noted that of 47 robberies and 203 rapes approximately 80.4% of these cases involved mistaken eyewitness identification (Gross & Shaffer, 2012). They pointed out that, although there are far more robbery convictions than rape convictions overall, there are more rape exonerations in their data. These false convictions, which arise (in part) through mistaken identification, raise two questions: Why did these eyewitnesses misidentify an innocent person? And why did no one see that these identifications were incorrect?

This dissertation focuses on the second question.

There is a large social science research literature that examines the abilities of ordinary people, i.e., those who make up the jury pool, to understand and analyze eyewitness testimony, and render verdicts in criminal and civil cases (Weiten & Diamond, 1979; Diamond, 1997). Even more specifically, researchers have questioned jury eligible participants to examine whether people possess sufficient knowledge to correctly assess the reliability of eyewitness identification testimony (Deffenbacher & Loftus, 1982; Desmarais & Read, 2011). Based on this research, many researchers have argued that jurors do not understand human memory or the identification task well enough to evaluate evidence or render informed verdicts. So why do jurors have

difficulty judging the validity of the identification, after hearing the eyewitness' testimony? At the core, this is a question about humans making judgments about the behavior of other humans, which is something that humans do every day.

This dissertation focuses on the research methodology that is used to assess the abilities of ordinary people to make judgments and render verdicts based on eyewitness identification evidence. The results this dissertation suggest that the conclusions depend on the methodology of the study.

This dissertation is organized as follows. Chapter 1 sets out the problem, discusses a theoretical framework for examining juror decision-making, describes different research methodologies and discusses their strengths and weaknesses. Chapter 1 also includes a discussion of previous research used to assess people's ability to evaluate eyewitness identification evidence. Chapter 2 presents the methods and results of Study 1, which is a meta-analysis of eyewitness testimony studies conducted using either videotaped or written testimony formats. Chapter 3 presents Study 2, which is an experimental examination of the effects of presenting eyewitness evidence to participants in one of four different formats, either videotaped, audio-only, full written transcript, or summarized written transcript, to examine participant sensitivity to eyewitness testimony detail across those formats. Finally, chapter 4 presents a discussion of conclusions drawn from Studies 1 and 2, including possible limitations and applications of this research, and future research suggestions based on these findings.

Theoretical Framework

A criminal case presents a simple decision for a juror. The defendant is either

truly guilty or truly innocent, and the juror's decision is to find that defendant guilty or not guilty. The decision matrix is illustrated in Table 1. There are four possible outcomes, defined by the two possible states of the world, often referred to as the Ground Truth, and two possible decisions.

		Verdict	
		Guilty	Not Guilty
Ground Truth	Truly Guilty Defendant	True Conviction (TC)	False Acquittal (FA)
	Truly Innocent Defendant	False Conviction (FC)	True Acquittal (TA)

Figure 1. Potential Trail Outcomes, Based on Actual Guilt versus Verdict

As shown in Table 1, if the defendant is truly guilty and the juror's verdict is guilty, that is a true conviction, and if the defendant is innocent and the juror's verdict is not guilty, that is a true acquittal. These are both correct judgments. However, if the defendant is innocent but the verdict is guilty, that is a false conviction error. Likewise, if the defendant is truly guilty but the verdict is not guilty, that is a false acquittal error.

For these types of situations, where there are two possible states of the world (i.e., true guilt or true innocence) and two possible judgments, or decisions (i.e., guilty or not guilty), signal detection theory (SDT) provides a statistical description of the underlying processes that give rise to those individual juror judgments. Signal detection theory has a long history, dating back to the 1950's (Green & Swets, 1966; MacMillan & Creelman, 2005). The use of SDT as a framework to describe theories of individual decision-making, eyewitness accuracy, or juror decision-making also has a long-established tradition (Clark, Benjamin, Wixted, Mickes, & Gronlund, 2015; Coombs, Dawes, & Tversky, 1970; DeKay, 1996; Ewart, Hogue, & Hogue, 1976; Kerr, 1993; Sorkin & Dai,

1994; Underwood, 1977). The basic outline of the signal detection framework for juror decision-making is illustrated in *Figure 1*. In Figure 1, the horizontal axis represents the evidence of the defendant's guilt. For some cases, the evidence against the defendant may be overwhelming (right end of the scale) whereas for other cases the evidence against the defendant may be quite thin (left end of the scale). The distribution on the left represents defendants who are truly innocent and the distribution on the right represents defendants who are truly guilty.

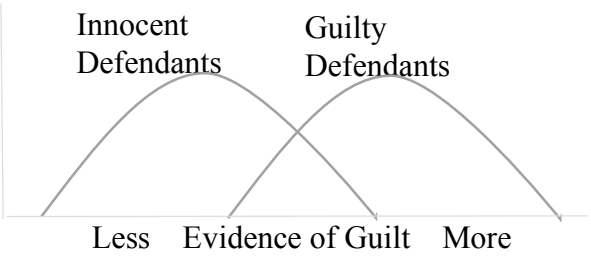

Figure 1. Distributions of evidence of guilt for innocent versus guilty defendants.

A critical feature of this description is that the two distributions overlap. Thus, some innocent defendants may have more evidence linking them to the crime than some guilty defendants. This area of overlap of the guilty and innocent distributions is the source of mistakes in juror judgments. So far, the theory only considers the amount of inculpatory evidence, but does not consider any interpretation, understanding, or application on the part of the juror.

If jurors are not able to pick out the relevant information relating to the probable guilt or innocence of the defendant, then they will have difficulty in differentiating between guilty and innocent defendants. As illustrated in Figure 2a, when jurors are less sensitive to the evidence, and cannot pick the important details from the irrelevant embedding talk, or noise, there is greater overlap of the distributions. However, as shown

in Figure 2b, when jurors possess greater sensitivity they are better able to pick the relevant evidence from the noise, and understand what that evidence indicates about guilt

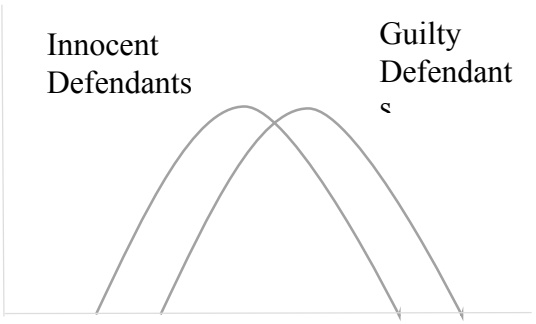

Figure 2a. The apparent evidence as viewed by jurors possessing less sensitivity

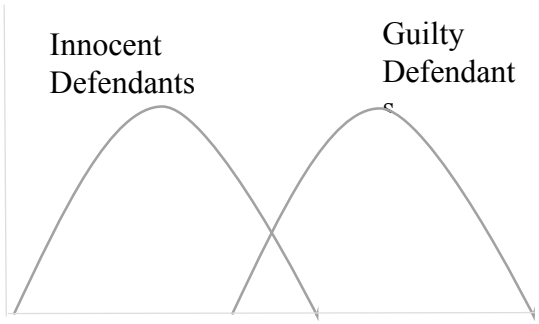

Figure 2a. The apparent evidence as viewed by jurors possessing more sensitivity

or innocence. As a result, the distribution overlap is reduced. Again, this area of overlap is where errors in judgment occur, and it is this difference between the distributions that can be measured using d prime (d').

Measuring ability to discriminate: Sensitivity. In SDT, d' is a common measure of discrimination, or diagnostic accuracy (i.e., sensitivity); it measures the standardized difference between the means of two distributions, which takes into account the standard deviation of the distributions. It can be calculated using the measurement (*Z*-scored) of the proportion of true convictions (hits) and the proportion of false convictions (false alarms): d' = *Z*(hit rate) – *Z*(false alarm rate). Z-scoring allows the comparison of means from two or more normal distributions and can be obtained using probability (*p*). When the difference between the means of the distributions increases, as a product of juror sensitivity, the overlap of the two distributions decreases, allowing for greater juror

accuracy and, thus, fewer errors. Therefore, d' is used as a standard measure of diagnostic accuracy, or juror sensitivity (Clark et al., 2015).

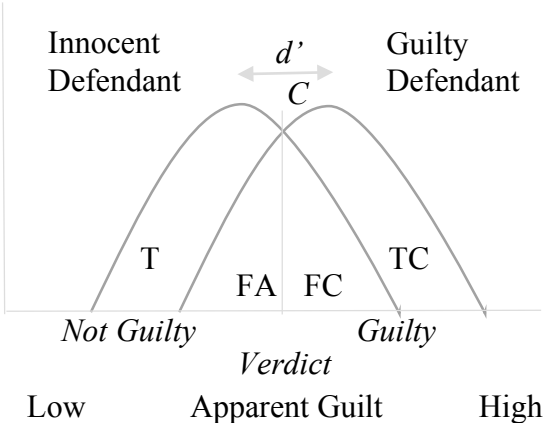

Figure 3a. Distributions of apparent evidence viewed by less sensitive jurors, with verdicts and potential outcomes.

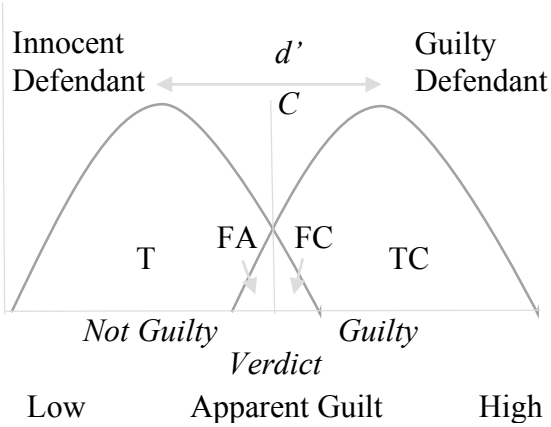

Figure 3a. Distributions of apparent evidence viewed by more sensitive jurors, with verdicts and potential outcomes.

In Figures 3a and 3b, there is a vertical bar marked "*C*", which indicates the decision criterion representing some arbitrary or mandated standard of proof (e.g., such as guilty beyond a reasonable doubt, known as BARD, or some other criterion), which specifies the amount of evidence that is necessary and sufficient for that juror to render a guilty verdict. If the evidence of guilt fails to reach that criterion point (remains to the left of bar "*C*"), the juror will vote not guilty, but if it reaches or exceeds that standard (reaches bar "*C*" or continues past to the right of bar "*C*"), the juror will render a guilty verdict. In Figure 3a, the less sensitive jurors cannot effectively discriminate well between good evidence and poor evidence, resulting in a greater number of judgment errors. Some innocent defendants are judged guilty (FC, false convictions) and some guilty defendants are judged not guilty (FA, false acquittals). Conversely, as shown in

Figure 3b, when jurors are more sensitive to the pertinent evidence, the distributions separate as a function of the juror's ability to discriminate the key information from the noise. As a result, the total number of errors decrease (both FCs and FAs) and *d'*, as a measure of the juror's sensitivity to the evidence, increases. However, that criterion placement can be shifted by various factors, such as the judge's instructions, a sympathetic victim or witness, or an expert witness that increases juror skepticism regarding certain types of evidence.

Criterion shift versus sensitivity. As illustrated in Figure 4a, shifting the criterion to a more liberal placement means less evidence is required to induce the juror

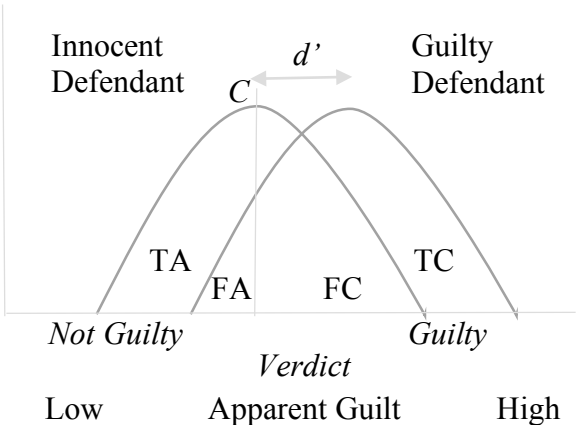

Figure 4a. Distributions of apparent evidence with verdicts and potential outcomes, reflecting a liberal shift in decision criterion.

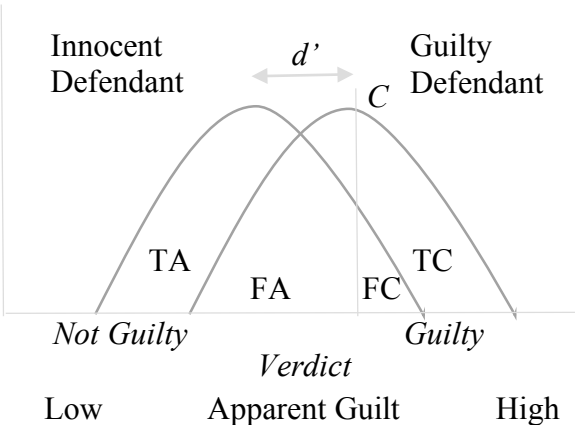

Figure 4a. Distributions of apparent evidence with verdicts and potential outcomes, reflecting a conservative shift in decision criterion.

to render a guilty verdict, whereas in Figure 4b, shifting the criterion to a more conservative position means more evidence is required to elicit a guilty verdict. A liberal shift (Figure 4a) results in fewer false acquittals and a greater number of false convictions, whereas a conservative shift (Figure 4b), such as juror implementation of the BARD standard, results in fewer false convictions and a greater number of false

acquittals. The measure of sensitivity, *d'*, remains constant, as the area of overlap, remains constant. Sensitivity has not increased, only the decision criterion has changed. As opposed to sensitivity, which involves increased accuracy, criterion shifts do not reduce the overall number of errors, they simply change the type of errors being made.

Knowing what to look for, and possessing the basic ability to pick the correct information from the surrounding testimony (the signal from the noise) is the more standard, or traditional, definition of sensitivity, the form of discriminability that is typically measured by *d*'. Cutler and Penrod (1995) slightly expanded the concept of sensitivity in their application to eyewitness evidence. They stated that sensitivity consists of two factors, knowledge and integration. According to Cutler and Penrod, "knowledge refers to an awareness of the manner in which a factor influences eyewitness memory, including the direction and magnitude of the effect for a given factor" (1995, p. 217). It is not enough for the juror to simply understand what testimony elements are important and discern those facts from the background information, they also need to know how heavily to weight the evidence, and in which direction (i.e., whether it speaks to guilt or innocence). Using the term sensitivity in a slightly different way, Cutler and Penrod (1995) discuss jurors as being sensitive or insensitive to the factors that distinguish between accurate and inaccurate identifications.

To illustrate, consider a hypothetical witness who has observed a robbery – under one of two sets of circumstances. In one case, the robbery occurred during daylight hours, the lighting was good, the witness had normal eyesight, was close by (but at a safe distance), and the eyewitness identification of the suspect was made the next day. In the

other case, the robbery occurred at night, with poor lighting, the witness was farther away, was near-sighted, and the identification of the suspect was not made until six months after the robbery. In both cases, there is evidence that the witness identified the suspect. However, all theories of perception and memory, from the most complex to the most intuitive, would consider these two identifications differently. An identification is more likely to be accurate when the conditions of observation and memory are good (i.e., scenario 1) relative to when the conditions of observation and memory are poor (i.e., scenario 2). To the extent that the juror fails to pick out the relevant information, or understand the significance of that information, the juror would attach as much weight to an identification made under poor conditions as he or she would to an identification made under good conditions. On that basis, it would be expected that if a juror gave more weight to an eyewitness identification made under good conditions and less weight to an identification made under poor conditions, that juror would be considered to possess greater sensitivity than a juror who weighted both identifications as equally inculpatory.

The second aspect of Cutler and Penrod's definition of sensitivity is integration, which "in this context, refers to the ability to render decisions that reflect knowledge" (1995, p. 217). In other words, integration is the ability of the juror to apply what they have discerned from the testimony to their verdict. Researchers have studied juror sensitivity a number of ways, but many studies use participant belief in the accuracy of the eyewitness identification, or the verdict rendered by the participant, as the measure of juror sensitivity to factors affecting identification accuracy.

Juror Sensitivity: A Brief Review of the Methodology and Empirical Literature

This section will describe different research methods for assessing the accuracy of jurors' decisions and their sensitivity to the factors that affect the accuracy of eyewitness testimony, noting the strengths and weaknesses of those methodologies. Following that brief methodological overview, I will present a brief review of the results from those studies.

Research methods: Strengths and weaknesses. Jury research has been conducted with methods as complex as case studies of actual jurors and as simplistic as multiple-choice surveys. Starting with a discussion of real jury trials, I will describe the issues inherent in the common research methodologies used to study juror sensitivity and decision making.

Analyses of real jury trials. Intuitively, one might evaluate the verdicts rendered in real cases by real jurors. Observing actual jurors making real decisions seems like it might be the best way to determine how jurors make decisions. However, such direct observations may tell us little about accuracy of those decisions. The problem, of course, is one of ground truth.

Consider, for example, the case of Ronald Cotton. Cotton was tried in 1984 for the rape of Jennifer Thompson. Thompson was a compelling eyewitness who allegedly had ample time to observe her attacker and, except for Ronald Cotton, everyone in the room (e.g., the judge, the jury, the prosecutor, the public, etc.), was certain that he was guilty. Thus, as data, Thompson's identification of Cotton would be scored as a correct identification, and his subsequent conviction would be scored as a correct conviction.

However, DNA evidence incontrovertibly proved that Cotton was innocent; Thompson's identification and the jury's verdict to convict were both incorrect. Consider also the cases of O.J. Simpson and Casey Anthony. Both accused of first-degree murder, they were considered by the public and law enforcement to be guilty of the crimes of which they were accused, but they were both acquitted by juries of their peers. There is no way to know the truth of who killed Nicole Brown Simpson, Ron Goldman, or Caylee Anthony, but the jury verdicts in cases such as Cotton, Simpson, and Anthony leave us wondering. Because of the unavoidable ground truth problem, studying the verdicts rendered by real jurors allows limited insight into the accuracy of those verdicts. It is therefore necessary to study juror sensitivity and decision making in other ways. There are studies that have attempted to determine what lay people know about the factors that influence eyewitness accuracy.

Lay knowledge: Multiple-Choice Tests and Surveys. These studies, some of which date back to the late 1970's, attempted to assess juror knowledge about relevant eyewitness factors by means of surveys. Some studies used a multiple-choice format, such as Deffenbacher and Loftus (1982) and Noon and Hollin (1987), whereas others used statements and ratings, such as Kassin and Barndollar (1992) and Read and Desmarais (2009). Early studies of this type led researchers to conclude that laypeople are largely ignorant of the importance of factors affecting the accuracy of eyewitness testimony, such as cross racial identifications, weapon presence, witness age, or stress (Deffenbacher & Loftus, 1982), although later studies challenged that conclusion.

The Deffenbacher and Loftus study tested participants with short situational

descriptions with four possible responses, using two college student samples and two samples of laypeople. One of the 14 questions, for example, was "[w]hen a person experiences extreme stress as the victim of a crime, there will be: (a) generally a greater than normal ability to perceive and recall the details of the crime. (b) generally the same ability to perceive and recall the details of the crime as under normal conditions. (c) a majority of people who will become better at perceiving and recalling crime details whereas others will become worse at it. (d) generally a reduced ability to perceive and recall the details" (p. 27). Whereas the college student populations recognized that stress and leading questions can impair witness accuracy, the results were taken to show that, overall, laypeople had limited understanding of factors affecting eyewitness memory and accuracy.

However, a larger ($N = 999$) and more recent survey study conducted by Read and Desmarais (2009) showed that the lay responses frequently approximated those of experts. Over separate trials, three samples of participants were given 27 to 29 statements, such as "[v]ery high levels of stress impair the accuracy of eyewitness testimony" and asked to rate them from 1 (strongly disagree) to 4 (strongly agree), or select "Don't know" rather than guess (p. 325). Because studies were yielding conflicting data about lay knowledge of eyewitness accuracy factors, Read and Desmarais conducted a meta-analysis.

Desmarais and Read (2011) analyzed 23 survey studies, which included both the Deffenbacher and Loftus (1982) and Read and Desmarais (2009) studies just mentioned. Based on their examination of the surveys, Desmarais and Read concluded that laypeople

are knowledgeable about what Wells (1978) referred to as system variables, those factors under the control of the judicial system (e.g., lineup size and presentation, leading questions, etc.). However, they also concluded that laypeople are not especially knowledgeable about what Wells (1978) called estimator variables, those factors involving the situation and events of the crime itself, which are not under the control of the justice system, and which "more frequently appear to be 'beyond the ken' of a jury" (e.g., weapon focus, violence, and length of observation) (p. 207).

So, the more traditional view, espoused by researchers such as Deffenbacher and Loftus (1982) or Cutler and Penrod (1995) is that jurors are not very knowledgeable, in terms of recognizing what factors are relevant to eyewitness accuracy. The 2009 Read and Desmarais study suggests they are as knowledgeable as the experts in this regard, whereas the Desmarais and Read (2011) study modifies that by stipulating that jurors are knowledgeable about procedural system variables, but do not understand the effects of the event-related factors (estimator variables).

An advantage of using surveys is that researchers can gather data quickly from large, diverse samples. With the advent of online survey engines or applications, thousands of participants can be polled in as little as a day. However, there are clearly limitations to the survey method of studying juror knowledge, as it does not embed the information in testimony. Actual jurors see and hear the evidence presented to them, they do not read testimony. Surveys are read, and much depends on the way the questions are worded. Finally, surveys and multiple choice tests do not evaluate participants' abilities to apply their knowledge to a decision (i.e., rendering a verdict). Read and Desmarais

(2009), referencing then unpublished research presented in 2006 by Alonzo and Lane, noted that participant responses to case relevant questions on surveys did not predict actual decisions made by those same individuals on simulated cases. Whereas recent survey studies suggest that jurors are more knowledgeable than they were previously believed to be, jurors must pick that information out of the noise of the embedding testimony and appropriately apply that knowledge to their verdict. To evaluate such questions about understanding and application, researchers have conducted trial simulation studies.

Application of knowledge: Simulated Trial Testimony. The empirical literature examining juror sensitivity has shown remarkably variable results regarding juror sensitivity to eyewitness accuracy factors. It is difficult to study the accuracy of decisions made by jurors in actual criminal trials, as previously discussed. Therefore, one way to replicate that process is to create a situation where the accuracy of the eyewitness identification is known. By staging an event, either a group task or simulated crime, involving naïve participants and a confederate, (i.e., the target individual), the naïve participants become real witnesses, who then may make a correct or incorrect identification of an individual. The other way is to present participants with hypothetical evidence in the form of scenarios. Staged event studies primarily depend on the jurors' ability to detect the accuracy of the identifications, whereas hypothetical scenarios examine juror awareness and application of factors affecting identification accuracy by varying witnessing conditions between scenarios.

The other major difference in methodology has been the choice of format of the

testimony that has been presented to the participants. Typically, the testimony is either audiovisual, such as live or videotaped testimony, or written, such as testimony transcripts or summaries.

Weiten and Diamond (1979) noted that many jury research simulations use severely shortened, or summarized, trial materials, omit many elements of a trial (e.g., an audience, *voir dire*, judge's instructions, etc.), and change the modality of information input by having the participants read testimony. They noted that by deviating in this manner from real world trial conditions juror perceptions of factors in the testimony could be artificially exaggerated. Diamond (1997) suggested that abbreviated trial simulations, such as those which present only eyewitness testimony to participants, may result in magnified effect sizes. Bond and DePaulo (2006), along with Weiten and Diamond (1979) comment that written formats deny participants access to auditory and non-verbal facial and body cues, which jurors use to judge deception and accuracy. Weiten and Diamond consider this to be detrimental to participant judgments, whereas Bond and DePaulo found that both auditory and audiovisual materials result in a "truthfulness bias" in the observer (i.e., participant) which encourages them to believe the heard information is more likely to be true.

Therefore, conclusions made about juror sensitivity may be contingent on how researchers present the testimony to the participants. Researchers may overestimate juror's abilities to understand and apply the key factors related to eyewitness accuracy, potentially as a result of inflated manipulation effects/effect sizes, when the testimony is presented in a reduced or summarized form, as opposed to when testimony is presented in

a more elaborate form that is closer to that of actual testimony given by witnesses in trials.

In examining the literature, there seems to be a difference in juror sensitivity based on the format of the materials, with written material studies eliciting greater juror sensitivity. Therefore, the review of the literature will be organized by the primary methodology, either staged or hypothetical scenario, and then by format, live, videotaped, or written testimony.

Simulated scenario studies.

Assessing the accuracy of real witnesses in staged events. There are relatively few studies involving staged events that utilize live testimony. In the Wells, Lindsay, and Ferguson (1979) study, groups of three participants witnessed the theft of a calculator by a female confederate (i.e., the target individual). The thief was in the room 75 seconds, and interacted with (looked at or spoke to) the three participants (witnesses) four times. After learning that they had witnessed a staged crime, they provided a description of the thief, and attempted to identify her from photographs. Twenty-four accurate and eighteen inaccurate witnesses were selected to testify. A separate group of participants, designated as jurors, were then asked to judge whether the identifications were accurate, based on live witness testimony under cross-examination. Each of the forty-two groups of three to six jurors observed the testimony of a different witness. The juror judgments of accuracy were compared to actual eyewitness accuracy and, collapsing across leading and non-leading question experimental manipulations, there was only a weak relationship between accuracy judgments and actual identification accuracy (r = .081). Wells et al. concluded

that jurors were not very good at determining identification accuracy based on their observation of witness testimony.

Unlike the Wells et al. study, most staged event studies use videotaped testimony. Such a study was conducted by Lindsay, Wells, and Rumpel (1981), using the Wells et al. (1979) design, but with three scenarios intended to either impair or improve the likelihood of an accurate identification. The "low situational accuracy," scenario involved poor viewing conditions: the target subject appeared very briefly (12 seconds), asked a single question ("Hey, is this your calculator?), and wore a hat (a knit cap covering the hair and part of the ears). The "high situational accuracy" condition involved good viewing conditions: the target subject was visible longer (20 seconds), made a second inquiry ("Has the experimenter told us what to do yet?"), and wore no hat (the hair and ears were visible) (p. 81). A moderate situational accuracy condition was identical to the low accuracy condition, except the hat was worn higher so that hair color was visible.

The witnesses followed the same identification procedure as in Wells et al. (1979), but testified during cross-examination on videotape. It should be noted that correct identifications by the witnesses in this study varied across viewing conditions (poor 33%, moderate 50%, good 74% accurate).

Participants, acting as jurors, viewed four videotaped testimonies and were asked to judge whether the identifications were accurate or inaccurate. There were only small to moderate differences in juror's judgments of accuracy between poor and good viewing conditions (low and high situational accuracy) (poor 62%, good 77%, $r = .163$). The rate at which jurors correctly assessed witness accuracy was 52.5% in the poor, 50% in the

moderate, and 51% in the good conditions, so approximately 51.2%, or chance levels, across all three viewing (situational accuracy) conditions. Based on these results, Lindsay et al. also concluded that the jurors could not judge witness accuracy from observing the testimony, and that the small to moderate variation in juror belief in accuracy between viewing conditions failed to indicate juror sensitivity to eyewitness accuracy factors.

Replications and extensions of the Lindsay et al. study using the same witness testimony videos again showed relatively small variations based on viewing conditions. In the Wells, Lindsay, and Tousignant (1980) study, participant judgments of witness accuracy varied only slightly between viewing conditions (poor 47.5%, good 61.25%, r = .138), and the participants' actual ability to detect witness accuracy was 45.5% in the poor and 49.8% in the good condition, averaging approximately 47.6% overall, or just below chance levels. Whereas the participants showed some sensitivity to the viewing conditions, their actual ability to judge accuracy was not good.

Wells and Leippe (1981) restaged the mock crime from the Wells et al. (1979) study, using a male thief and increasing his exposure time to 120 seconds. Consistent with the results in the 1979 Wells et al. study, Wells and Leippe found only a weak correlation between actual eyewitness accuracy and participant judgments of identification accuracy (45.8%, r = –.085). If the witness' memory of the event was unchallenged, juror ability to judge accuracy was 52%, or chance levels. If the witness was discredited by inability to remember trivial details in cross-examination, overall belief in the accuracy of the identification was lower, but the jurors' ability to judge accuracy was further impaired at 39.6%, with 37.5% of the jurors believing the accurate

witnesses and 58.3% believing the inaccurate witnesses ($r = -.208$).

Lindsay, Wells, and O'Connor (1989) replicated Wells et al. (1979) but used actual attorneys for the witness examinations. The videotaped testimonies yielded results consistent with the previous studies. There was virtually no correlation ($r = -.022$) between the actual accuracy of the eyewitnesses and the participants' verdicts of guilt or innocence, with only 49% of participant verdicts being consistent with eyewitness accuracy (i.e., voting guilty when the witness accurately identified the perpetrator and not guilty when the witness identified an innocent suspect).

Far more recently, Reardon and Fisher (2011) conducted a staged event study to assess juror sensitivity using videotaped testimony. Participants engaged in either a discussion or a puzzle task with a confederate (i.e., the target individual) posing as another participant. The (real) participants were asked to describe the target individual immediately after the task. A few days later, those participants made a photographic lineup identification. All witnesses, regardless of accuracy, gave testimony two to five weeks later under direct and cross-examination. The description, identification, and testimony phases were videotaped.

Another group of participants acting as jurors viewed the testimonies of the witnesses. To increase the realism of the jurors' task, they were told the testimony related to a theft in the laboratory, and were asked to rate the likelihood that the witness identified the perpetrator, rather than an innocent person (identification accuracy), on a scale of 1 (very unlikely) to 9 (very likely), and how likely they would be to convict the defendant based only on the witness testimony, using the same 9-point scale (verdict).

The results showed that the participants ratings of accuracy did not vary strongly between the accurate and inaccurate witnesses (r = .132), nor were their verdict ratings correlated with actual witness accuracy (r = .028). Based on these results, Reardon and Fisher also concluded that jurors have "poor evaluative abilities" when judging traditional eyewitness testimony (p. 74). It is interesting to note, however, that when the participants were permitted to view the videotaped description and identification process, in addition to the testimony, their overall ability to identify accurate witnesses improved substantially (r = .460), and correct verdict decisions improved as well (r = .282). This would be akin to real-world jurors being allowed to view video of the entire witness identification process from initial description through the physical identification from a lineup, as well as seeing and hearing the witness's testimony in court, something not typically done in actual criminal trials.

These studies, taken together, suggest that jurors are not very good at telling whether an identification is accurate from observing eyewitness testimony. Two of these also suggest that jurors either cannot sufficiently discriminate between good and poor viewing conditions, or do not apply that information to their decision regarding identification accuracy. However, the various manipulations between conditions in the Wells et al. (1980) and Lindsay et al. (1981) studies varied only by 6 seconds of exposure, 1 extra question, and a knit hat over the hair. This minimal manipulation may have been too weak to evoke and/or measure sensitivity to differences between viewing conditions. The attempt in Wells and Leippe (1981) to discredit the witness' memory of the event did not increase the ability of the juror to distinguish between accurate and

inaccurate identifications, but rather caused increased skepticism and further damaged the juror's ability to correctly judge witness accuracy.

Similarity to real crime conditions. What staged events lack are the other factors in a real crime that tend to make eyewitnesses less accurate. It is these factors (e.g., inducing panic, stress, or fear in participants) that the Human Research Review Board (HRRB), will not sanction in human research. Therefore, in a staged scenario, the events must be calm and non-threatening, so as not to harm the participants. Real crimes are usually not so tame, and witnesses are often stressed, frightened, and emotionally and/or cognitively aroused. The witnesses to staged events do not give descriptions or make identifications in interviews conducted by the police, and the testimony is often in response to scripted direct or cross-examination. Therefore, the staged identification conditions and testimony are not necessarily representative of actual witness conditions and testimony. As a result, these staged events lack realism when compared to actual eyewitness identifications and testimony.

Assessing juror sensitivity to factors in hypothetical scenarios. Often, rather than staging a witnessed event and deposing actual witnesses, hypothetical scenarios are created for experimental use. As there are no actual witnesses, no actual crimes, and therefore no ground truth in these fictionalized testimonies, participant belief in the accuracy of the identification cannot be compare to actual eyewitness accuracy. Scenarios typically vary factors such as the viewing conditions, the defendant's or witness's race, the strength of the evidence, or other aspects of the testimony. The focus in these studies is whether the participants' assessments of the testimony, beliefs

regarding the accuracy of the identification, and verdicts vary based on the witnessing conditions. It is expected that more sensitive jurors will be more likely to associate poor witnessing conditions with the potential for an inaccurate, or false, identification than the less sensitive jurors, and therefore be less likely to give a guilty verdict. The scenario studies typically use videotaped or written testimony. Again, there is some discrepancy in results found between formats, with written scenarios seeming to elicit greater juror sensitivity.

Scenario studies using videotaped testimony. A set of studies conducted by Cutler, Penrod, and Stuve (1988), Cutler, Penrod, and Dexter (1989), and Cutler, Penrod, and Dexter (1990) used a hypothetical liquor store robbery to create and videotape a series of simulated 45-minute trials. Ten variables were manipulated across different versions of the testimony, which included (among others) the level of stress experienced by the witness during the crime (high/low), the presence of a weapon (visible/not visible), whether the perpetrator had a knit cap covering his hair (hat/no hat), and the length of time that elapsed between the crime and the identification (2 days/14 days).

There is empirical evidence to suggest that such factors as stress, a weapon, and length of time between observation and identification all adversely affect eyewitness accuracy. For example, a meta-analytic examination of the effects of stress on eyewitness memory by Deffenbacher, Bornstein, Penrod, and McGorty (2004) suggested a mild to moderate reduction in accuracy in eyewitness recall involving stressful crimes (Cohen's d = .31). Morgan et al. (2004) determined that recall was better in low, rather than high, stress crimes (65.6% accurate identifications/correct lineup rejections in the low stress

condition versus 37.7% in the high stress condition), with the high stress scenarios revealing substantial recall errors (54.3% false identifications in the high stress condition versus 28.8% in the low stress condition). A meta-analysis by Steblay (1992) examining weapon focus effects on eyewitness memory identified weak to moderate reductions in accuracy and feature recall, respectively (d = .13 and d = .55).

However, contrary to these empirical results, Cutler et al. (1988) showed that none of the manipulations significantly affected the participants' verdicts (stress d = .04, weapon d = .10, hat d = .04, length of time elapsed d = .06), overall relationship of viewing conditions to verdicts was extremely minimal (r = .068). In a follow-up study, Cutler et al. (1990) combined the data of 129 eligible and experienced jurors with the data from the 321 undergraduate subjects in the Cutler et al. (1988) study. The effects were, if anything, reduced in this combined replication (stress d = .00, weapon d = .03, hat d = .00, length of time elapsed d = .00). The Cutler et al. (1989) replication revealed no significant difference in proportions of belief in the identification between the poor viewing condition (i.e., hat, visible handgun, 14-day delay, and suggestive lineup instructions), and the good viewing conditions (i.e., no hat, no handgun, 2-day delay, and unbiased lineup instructions), r = .010. The researchers concluded that, in each of these studies, the participants were not especially sensitive (in a manner consistent with Cutler and Penrod's [1995] later definition including knowledge and integration) to factors affecting eyewitness identification accuracy.

Scenario studies using written testimony or crime descriptions. A 1974 study by Loftus used a written description of a grocery store robbery and murder. In one version,

the description stated that there was an eyewitness to the crime (eyewitness condition). In the other scenario, the description noted that there was an eyewitness, but his vision was very poor and he was not wearing his glasses at the time of the crime (discredited eyewitness condition). These data yielded no significant difference in guilty verdicts between the eyewitness and the discredited eyewitness condition (r = .044), suggesting the participants were not sensitive to factors such as witness visual acuity (Loftus, 1974).

Following the study by Loftus (1974), several replications were conducted all using written materials. Weinberg and Baron (1982) conducted two independent replications (N = 217 and N = 156). In these two studies, the testimony again featured either an unchallenged witness or witness testimony discredited by proof of poor eyesight and a lack of glasses. Their results showed a significant drop in guilty verdicts when the witness was discredited (r = .262, n = 70 and r = .217, n = 105). In contrast to the Loftus (1974) study, these results suggest that the participants were at least aware that a witness with poor vision might make an error in identification, and that this knowledge was integrated into their verdicts (Weinberg & Baron, 1982). In other words, by Cutler and Penrod's (1995) definition, these participants were somewhat sensitive to at least one factor affecting eyewitness identification accuracy.

Kennedy and Haygood (1992) replicated Loftus (1974) using written materials from McCloskey, Egeth, Webb, Washburn, and McKenna (1981). The new materials used a different crime scenario, but was essentially comparable to the Loftus (1974) study, in that it compared discredited versus unchallenged testimony. Kennedy and Haygood conducted three independent replications. The first used a lengthy trial

transcript (N = 147) from the McCloskey et al. study, the second employed an abbreviated trial summary of that transcript (N = 183), and the third directly replicated Loftus by using the summaries from the 1974 study (N =145). What these data revealed was that the verdicts varied substantially in all three experiments, with fewer guilty verdicts in the discredited eyewitness condition than in the non-discredited condition (the longer transcript version yielding discredited 19%, unchallenged 42%, r = .228, n = 98; the McCloskey et al. summary version yielding discredited 23%, unchallenged 52%, r = .299, n = 122; and the Loftus replication (summary) yielding discredited 44%, unchallenged 72%, r = .284, n = 72). The summary materials showed somewhat greater variation in verdicts than the full transcript, perhaps because the summaries reduced the amount of embedding information (noise) and made it easier for the participants to focus on the pertinent information. It is important to note, however, that whereas Loftus found very little variation (r = .044) between discredited and unchallenged witness testimony in her 1974 study, Kennedy and Haygood, *using that same set of stimulus materials*, saw significant variation (r = .284). Once again, the Kennedy and Haygood data suggest that the jurors were sensitive to the visual impairment of the witness.

Moving away from the discredited witness scenarios, Bradfield and Wells (2000) used two sets of eight written scenarios to manipulate five factors in the testimony. One set varied witness certainty (high/low), view (clear/somewhat obstructed), and attention paid to the perpetrator (high/low). The other set manipulated certainty (high/low), description (good/poor), and time from crime to identification (same night/six months). Belief in eyewitness accuracy and willingness to convict were rated on a scale of 1

(definitely not) to 7 (yes, without a doubt). They found that participants were more likely to believe the witness made an accurate identification, and were more inclined to convict, when the witness paid a high level of attention to the perpetrator (belief $r = .220$, verdict $r = .237$, $N = 192$). The manipulation for view did not produce a significant difference in belief or verdict (belief $r = .105$, verdict $r = .050$, $N = 192$). The separate manipulation for description showed participants were more willing to believe the identification was accurate and more willing to convict when the match of the witness' description to the perpetrator's appearance was described as good than when it was described as poor (belief $r = .262$, verdict $r = .274$, $N = 193$). These results, and those of Kennedy and Haygood (1992), suggest that the participants were sensitive to the details of the testimony associated with eyewitness accuracy (Bradfield & Wells, 2000).

Geiselman et al. (2002) and Geiselman and Mendez (2005) used simulated testimony about a gas station robbery/homicide, with manipulated viewing conditions. The good viewing condition was conducive to making an accurate identification (e.g., bright light, the crime occurred five feet from a witness who had good vision, no perpetrator disguise, and identification occurred the same day). The poor condition was not conducive to making an accurate identification (e.g., dim light, occurring forty feet away from a witness who had less than 20/20 vision, the perpetrator wore a hat and sunglasses, and identification happened two days later). Geiselman et al. (2002) showed that participants were significantly less likely to convict in the poor viewing conditions than in the good viewing conditions (good 64.5%, poor 25.6%, $r = .390$). Similarly, Geiselman and Mendez (2005) also showed that participants were less likely to render

guilty verdicts when the viewing conditions were poor than when they were good (good 46.2%, poor 26.9%, $r = .200$).

Different empirical examinations of juror sensitivity have produced conflicting results. There is some evidence that participants, acting as jurors, exhibit at least a small to moderate amount of sensitivity to experimental manipulations of factors that affect eyewitness accuracy, such as viewing conditions or attention paid to the perpetrator during the crime, whereas other studies show only small differences in participant judgments of accuracy, belief in accuracy, or verdicts between manipulations.

Stimulus material format comparisons. As discussed, different researchers have used different formats to present the stimulus materials (e.g., a crime description or testimony) to the participants. As sensitivity seems to be stronger in studies using written formats, some researchers have attempted to explore whether different stimulus material formats create differences in participant verdicts, but these studies do not adequately permit an examination of sensitivity to viewing condition or evidence by testimony format.

One of the studies that examined format effects was that of Bermant, McGuire, McKinley, and Salo (1974). Using one transcript, drawn from an actual trial, they presented the testimony to participants in one of four formats. The audiovisual presentation was a 50-minute dramatized reading of the transcript on audiotape with an automated slideshow. The audio condition used the audio alone, the transcript condition used the full 30-page trial transcript, and the summarized format was a 4-page summary. Each participant received only one format, and tendered a verdict of first-degree murder,

second-degree murder, manslaughter, or not guilty. The results showed significant differences in verdicts across formats. The participants who were given the written materials were significantly more likely to convict than those who were presented with the audio-only or audiovisual materials (r = .377). Although differences in verdicts between formats were shown, this study is mute on the issue of sensitivity. Across all formats, the same evidence details from the same trial transcripts were used. As there were not separate scenarios varying viewing conditions (good versus poor), or other details of testimony such as strength of evidence, that could permit a comparison of participant verdicts by type of evidence over the different formats, sensitivity to those factors could not be assessed.

Another examination of stimulus format was conducted by Martin, De La Fuente, De La Fuente, and Garcia (2007). They presented testimony to participants in either a videotaped or a written format. They varied the strength of evidence (i.e., strong pro-prosecution vs. ambiguous) and asked for ratings of guilt. They found that participants were slightly more likely to convict in the video format condition (r = .057), and slightly more likely to convict in the strong evidence condition (r = .115). However, they did not report the key data needed to determine how verdicts varied between evidence conditions (strong vs. ambiguous) by stimulus material format (video vs. written), so differences in sensitivity to evidence over different formats could not be assessed.

Bermant et al. (1974) showed that written stimulus format materials produced more guilty verdicts, but did not vary scenarios to allow an assessment of sensitivity. Martin et al. (2007) measured a factor that could speak to sensitivity (strength of

evidence), but did not examine that sensitivity by format. The key question remains, does the stimulus material format used by researchers significantly impact juror sensitivity?

Chapter 2 – Study 1: Meta-analysis

Introduction

It appeared, at this point, that there was a disparity in juror sensitivity emerging between studies using videotaped testimony and those using written testimony. Although there seemed to be a tendency for the written studies to elicit greater sensitivity to eyewitness accuracy factors, it was necessary to conduct a meta-analysis to determine if there was a significant difference between formats.

Predictions. Weiten and Diamond (1979) suggested that due to the "sparse simplicity" of written simulations, which are often truncated or simplified, certain factors in the testimony, such as defendant attractiveness, might be "substantially exaggerated" (p. 77). Diamond (1997) also suggested that as simulations become more simplified, effect sizes may be magnified. The implication is that sensitivity to testimony details, both relevant and irrelevant, might be artificially inflated when using written testimony materials. Based on this, I predicted that studies using written materials would show participants to be more sensitive to differences in viewing conditions (i.e., good viewing conditions that are optimal for accurate identification versus poor or disadvantaged viewing conditions) than those using videotaped materials.

Method

Search Strategies. Search strategies included examining known studies on juror sensitivity and eyewitness identification. Searches of the references used in those articles

and citation searches located more articles. Database searches included, to name a few, the Social Science Citation Index, PsycInfo, and PsycArticles. Key phrases used in the database searches included eyewitness*, witness*, eyewitness* identifi*, witness* identifi*, eyewitness* method*, witness* method*, eyewitness* research, witness* research, eyewitness* testimon*, witness* testimon*, eyewitness* accur*, witness* accur*, trial testimon*, trial method*, trial research, jur* method*, jur* sensitiv*, jur* research, jur* verdict, testimon* form*, testimon* medium, testimon* present*, live testimon*, video* testimon*, audio* testimon*, transcript testimon*, written testimon*, summar* testimon*.

Inclusion / exclusion. Studies included in the meta-analysis manipulated one or more factors that have been shown to affect eyewitness accuracy, such as lighting (bright, dim), clothing or disguise (hat, sunglasses, etc.), vision (witness wearing glasses or not), length of observation (brief, long), etc. For each study, there was a "good" versus a "poor" condition, such that these factors were manipulated to create an experimental condition conducive to eyewitness accuracy and a condition that was not conducive to accuracy. The studies assessed juror sensitivity through various outcome measures, including: the proportion of guilty verdicts, guilt ratings, willingness to convict ratings, and ratings of belief in the eyewitness's accuracy. Testimony materials were audiovisual (such as videotaped testimony) or written (such as full or abbreviated testimony transcripts). Useful statistics had to be present in the article, such as t, $F_{(1,---)}$, $\chi^2_{(1)}$, or Pearson's r, or proportions of guilt or belief that could be converted to $\chi^2_{(1)}$. Studies which used ambiguous methods of assessing juror judgment, such as financial awards or

ratings of eyewitness appeal were excluded, as they did not allow for a clear assessment of sensitivity. Therefore, studies lacking appropriate independent or dependent variables, useable statistics, or sufficient information to derive useable statistics were excluded.

Analysis. Seventeen articles yielded twenty-one comparisons. All *F* or χ^2 statistics were transformed into a single, common effect size, using Pearson's *r*, and a random effects analysis using an independent samples t-test was conducted comparing *r* in studies that used audiovisual versus written materials.

Results and Discussion

There was a significant difference between stimulus material formats when examining participant ratings over the various experimental conditions. The ten studies that used written stimulus formats showed significantly greater sensitivity (mean *r* = .243), than the eleven studies that used videotaped stimulus materials (mean *r* = .078), $t(19) = 3.533$, $p = .0011$ (1-tailed), $r = .630$. Fail safe *N*, using Rosenthal's (1979) suggested calculation for determining the number of unpublished studies (i.e., "file drawer studies") averaging null results that would be required to change the outcome from robustly significant to *just barely significant*, was calculated to be 3. This indicates that the findings are strong enough to be considered valid with the number of studies included in this sample. It is clear, based on these studies, that there is a significant difference in sensitivity seen in written stimulus material studies as opposed to videotaped stimulus material studies.

Study	Ind. Variable	Dep. Variable	*N*	*r*
Written Stimulus Materials				
Loftus (1974)	Glasses Yes / No	Verdict	100	.0463
Weinberg and Baron (1982)	N/A / No glasses[1]	Guilt ratings	70	.2618
Weinberg and Baron (1982)	N/A / No glasses[1]	Guilt ratings	105	.2173
Kennedy and Haygood (1992)	N/A / No glasses[1]	Verdict	98	.2276
Kennedy and Haygood (1992)	N/A / No glasses[1]	Verdict	122	.2993
Kennedy and Haygood (1992)	N/A / No glasses[1]	Verdict	73	.2836
Bradfield and Wells (2000)	Cond.: Good/Poor	Verdict	192	.0490
Geiselman et al. (2002)	Cond.: Good/Poor	Verdict	328	.3897
Geiselman and Mendez (2005)	Cond.: Good/Poor	Verdict	675	.1999
Pozzulo et al. (2006)	Id. Reliability[2]	Verdict	180	.3500
Videotaped Stimulus Materials				
Wells, Lindsay and Ferguson (1979)	Eyewitness Acc.[4]	Ident. Belief	160	.0814
Wells, Lindsay, and Tousignant (1980)	Situational Acc.[3]	Ident. Belief	92	.1380
Wells, Ferguson, and Lindsay (1981)	Eyewitness Acc.[4]	Verdict	152	.0061
Lindsay, Wells, and Rumpel (1981)	Situational Acc.[3]	Ident. Belief	64	.1289
Wells and Lieppe (1981)	Eyewitness Acc.[4]	Ident. Belief	96	.0852
Cutler, Penrod, and Stuve (1988)	Ten Items[5]	Verdict	321	.0675
Cutler, Penrod, and Dexter (1989)	Situational Acc.[3]	Ident. Belief	538	.0098
Lindsay, Wells, and O'Connor (1989)	Eyewitness Acc.[4]	Verdict	178	.0215
Ramirez et al[6]., exp. 1 (1996)	Cond.: Good/Poor	Verdict	349	.1464
Ramirez et al[6]., exp. 2 (1996)	Cond.: Good/Poor	Verdict	338	.2199
Reardon and Fisher (2011)	Eyewitness Acc.[4]	Ident. Belief	122	.1321

[1] Replication of Loftus (1974) using different discrediting information sources, statistics collapsed across source.
[2] Overall reliability of the identification based on subject ratings (1 = not at all reliable to 6 = very reliable).
[3] Good viewing conditions versus poor viewing conditions during eyewitness observation.
[4] Staged crimes or tasks were used to produce actual witnesses to an event, so the witnesses were, in fact, accurate or inaccurate, making eyewitness accuracy an independent variable in the study.
[5] Items analyzed were ten factors believed to affect eyewitness accuracy. A between-subjects 2^{6+4} fractional factorial hierarchical regression was run with 64 cells of 4 to 6 subjects each, yielding $F(1, 310) = 1.410$.
[6] Ramirez, Zemba, and Geiselman

Table 2. List of Included Studies from Study 1: Meta-analysis, with Independent and Dependent Variables, Number of Participants, and Pearson's *r* for Each Study.

As mentioned, Weiten and Diamond (1979) had suggested that evidence details might be especially obvious or exaggerated for participants reading written testimony materials, which could make participants more sensitive to experimental manipulations than they would be if the same evidence were presented in a fuller context. Diamond (1997) also suggested that effect sizes may be magnified in abbreviated trial simulations. Bond and DePaulo, in their 2006 meta-analysis examining deception detection, determined that lie detection from video-only formats was less accurate than lie detection from either audio/audiovisual or written transcript formats. Accuracy remained stable across audiovisual, audio-only, and written formats. Whereas the Bond and DePaulo study found that participants were inclined to judge messages as more truthful in audiovisual or audio-only formats than in written formats, suggesting a potential participant bias to over-believe audiovisual testimony, the 21 studies examined in the current meta-analysis were not utilizing deception or measuring belief across formats, but were examined for evidence of participant sensitivity to eyewitness accuracy or eyewitnessing conditions between testimony presentation formats.

It is important to note that this comparison of live or videotaped to written testimony materials was based on different studies, which used different methodologies and varied in numerous aspects. It is not possible to gauge the relative strength of the experimental manipulations in these studies. Therefore, an experiment was conducted to examine sensitivity using the same testimony, modified to reflect good and poor viewing conditions, across four formats (i.e., videotaped, audio, transcript, or summarized testimony).

Chapter 3 – Study 2: Experiment

Introduction

The significant difference in effect sizes between written (mean r = .243) and videotaped (mean r = .078) testimony formats raises questions (in addition to those of manipulation strength) that cannot be answered by the meta-analysis. Does the variance in sensitivity arise from differences in discerning and remembering the critical details? Does it arise from differences in the application of those details to the assessment of identification accuracy? Does it arise from differences in the application of that knowledge to the verdict? There is no study in the literature that directly examines how different ways of presenting the testimony may be associated with differences in participants' sensitivity to eyewitness conditions. The experiment presented here provides such a direct comparison.

Participants were presented with the testimony of a victim of an armed robbery. The witnessing conditions were good in one version of the study and were rather poor in the other version. To the extent that participants were sensitive to this difference they should render more guilty verdicts when the witnessing conditions were good than when the witnessing conditions were poor. In addition, the testimony was presented in one of four formats: a video and audio recording, an audio-only recording, a complete written transcript, or a very brief summary.

The testimony did not include any type of statement referencing the witness's confidence in the accuracy of her identification of the perpetrator, as assessed at the time of the identification. It has been shown in numerous studies, including several discussed

here (e.g., Cutler et al., 1988; Bradfield & Wells, 2000) that participants rely heavily on eyewitness expressions of confidence. At the time these studies were conducted, that was believed to be a poor basis for a judgment of eyewitness accuracy. More recent research suggests that confidence is diagnostic for accuracy, such as Wixted and Wells (2017) which suggests that under pristine conditions confidence is related to accuracy, and Wixted, Mickes, Clark, Gronlund, and Roediger (2015) which notes that confidence expressed at the time of the identification (as opposed to during the actual trial) can be diagnostic of witness accuracy. That is consistent with the findings of Reardon and Fisher (2011), who found that having the participants view a video of the description and identification process, as well as the testimony, improved accuracy. It is possible, therefore, that jurors should place considerable weight on eyewitness confidence, especially witness confidence levels expressed during the initial identification. However, expressed witness confidence was not included as a variable in this design.

After presentation of the testimony, participants were asked about their verdict, and were also asked a number of additional questions – about their confidence in their verdict, their belief in likely accuracy of the witness's identification, their assessment of the conditions of eyewitness observation, and their memory for the critical details from the witness's testimony.

Predictions

The goal of the study was to determine the extent to which measures of sensitivity differed based on the format of the testimony, and to determine where sensitivity lacked application (if a lack existed) in the juror decision-making process. Again, based on the

theory suggesting that as formats become more simplified certain factors contained in testimony are more likely to be exaggerated (Weiten & Diamond, 1979; Diamond, 1997), I posited that the four formats (video, audio, full written transcript, and written summary) would show substantial variations in participant sensitivity. Specifically, I expected the written formats to yield far better recall for detail, and as a product of better recall, greater sensitivity to the viewing conditions. Memory would be assessed by open-ended questions testing recall of pertinent testimony details. Sensitivity to that detail would be assessed by ratings of the quality of the observation (i.e., how good a look the eyewitness was able to obtain of the perpetrator during the commission of the crime). Additionally, sensitivity in terms of application (i.e., applying those details to the identification) would be gauged based on verdicts and identification accuracy ratings.

Given that prediction regarding recall and sensitivity, and the results of the meta-analysis detailed in chapter 2, I predicted that the experimental condition using the longer, trial-like, audiovisual testimony would evoke little difference in guilty verdicts between the good and the poor viewing conditions, indicating less participant sensitivity. The audio-only testimony, being less complex than video due to the lack of visual cues, should result in slightly greater sensitivity, whereas I predicted that the written transcripts and summary transcripts would show the most sensitivity, demonstrated by more discrepant proportions of guilty verdicts between the good and poor viewing conditions. If so, and if recall for detail is the factor driving the effect, then the simpler written formats may indeed, as Weiten and Diamond (1979) and Diamond (1997) suggested, be artificially inflating estimates of participant sensitivity to factors affecting eyewitness

identification accuracy. If sensitivity is a function of awareness and recall only, and if materials exaggerate awareness as they become increasingly more simplified, then would the audio-only testimony exhibit a pattern of results closer to that of the videotaped testimony, or closer to that of the written testimony? If the issue is one of perceptual processing (e.g., processing more perceptually complex tasks), then it would be expected that audio-only results would be more similar to those of written materials. If, instead, the issue is related to an attempt to determine truthfulness or accuracy from body cues or audio cues (e.g., the presence of the eyewitness, in terms of the voice or view of a human stating the identification), then the audio-only testimony results should be closer to the videotaped testimony results. Based on Bond and DePaulo's (2006) observation that participant's ability to detect deception and tendency to perceive messages as truthful was consistent across audio-only and audiovisual messages, I hypothesized that the audio-only format would show a pattern of results more similar to the audiovisual format. The summarized material condition, included because many studies use summaries, was a less predictable format, in that it might, in its severely condensed form, be more stark and compelling, eliciting highest sensitivity, as suggested by Weiten and Diamond (1976), or instead, by virtue of containing fewer repetitions of the key facts, it might elicit lower sensitivity. I tentatively hypothesized, based on the Weiten and Diamond research, that the summary format would elicit greater juror sensitivity than the other three formats. Therefore, I expected sensitivity to be ranked, from highest to lowest, summary > transcript > audio-only > video.

Method

Participants. Five hundred ninety-four participants were recruited from the participant pool of students at the University of California, Riverside. Participation was voluntary, and participants were compensated with 1 credit hour to fulfill a requirement for an introductory Psychology course. Of the 594 participants, 378 were female, 216 were male. Ages ranged from 18 to 35, with a mean age of 19.49. The sample was ethnically diverse, with participants self-identifying as 45.1% Asian, 34.0% Hispanic, 10.1% Caucasian, 3.2% African-American, and 7.5% self-identifying as "other." English was listed as the first language for 317 (53.4%) of the participants. Of the 277 (46.6%) participants who indicated that English was not their first language, 274 (98.9%) rated their English reading and writing proficiency as "very good" or "ok". Only 13 (2.2%) participants indicated they had previously served on a jury, and 72 (12.1%) participants indicated they had been the victim of, or a witness to, a serious crime in the two years prior to the experiment.

Materials. The stimulus materials for this experiment were based on the testimony given in a preliminary hearing for an actual ATM robbery in Riverside County, California. The testimony was modified only slightly to create two versions of the robbery. The good viewing condition specified that that the conditions for observing the perpetrator were relatively good, whereas the poor viewing condition specified that the conditions for observation that were relatively poor (i.e., 11pm, dim / night, perpetrator's hair was covered by a dark hoodie). The version detailing good eyewitness viewing conditions specified that the crime occurred at 2pm, with good lighting ("well-lit" and

"daylight"). The perpetrator was described as clearly visible with no disguise, such that the eyewitness could clearly see his face, and describe skin color, hair color, and hairstyle. The perpetrator's face was described as clearly visible for one to two minutes. In the version detailing poor eyewitness viewing conditions, the eyewitness stated that the crime occurred at 11pm, with poor lighting ("poorly lit" and "very dark"). The perpetrator was wearing a dark "hoody" jacket with the hood pulled up around his face and hair, so that all the eyewitness could see was his skin color and eyes, which were visible to the eyewitness for less than a minute. In both versions, the perpetrator had a weapon (a gun), which was clearly visible to the eyewitness.

Actors used the modified transcripts of this testimony to create videotaped and audiotaped versions of the stimulus materials. A female actor in court-appropriate clothing played the part of the victim-eyewitness and two female actors in court-appropriate business attire played the parts of the prosecution and defense attorneys who examined and cross-examined the victim. A female actor in judicial robes played the part of the judge and a male actor in a uniform (similar to a court bailiff's uniform) assumed the role of bailiff/court clerk for the taping. The witness was videotaped and audio-recorded while giving testimony, using both the good viewing condition and the poor viewing condition testimony scripts. Each actor-attorney was filmed and recorded in both the role of prosecutor and defense attorney, in both the good-observation condition and the poor-observation condition.

The four versions of the testimony (good-poor observation, prosecutor-defense counterbalancing), including both direct examination and cross-examination, were filmed

in a courtroom at the Riverside County Courthouse. The witness gave testimony while seated in the witness box, and the camera was located in the jury box at the approximate position of juror number 1 (to give a perspective commensurate with that of the juror sitting closest to the judge's bench), and positioned so that the field of view encompassed the witness, both attorneys, the bailiff/court clerk, and the judge. The audio testimony was transcribed word-for-word, to assure that the video, audio, and transcript versions all matched, as the actors may have added a few words or sounds as they performed the script, and the full transcript was also reduced to a short one-page summary. Eight versions of the testimony were presented to different groups of witnesses: good or poor viewing condition, in either a full video-audio recording (V), an audio recording without video (A), a verbatim transcript (T), or a condensed summary (S). The transcript and summarized versions are detailed in Appendix A.

Design and procedure. The experiment conformed to a 2 (eyewitness viewing condition: good vs. poor) x 4 (stimulus material format: video, audio, transcript, or summary) design with both variables manipulated between groups. There were four versions of each video (and each audio) recording, two each detailing good or poor viewing conditions, with the roles of defense and prosecution counterbalanced across the two actors. This counterbalancing was done to control for actor-driven verdict differences.

The participants were welcomed to the lab and asked to read a consent form. The participants were randomly assigned by a rotational log-in procedure to one of the twelve experimental conditions (four video: good/poor, prosecution/defense, four audio:

good/poor, prosecution/defense, two transcript: good/poor, two summary: good/poor).

Once the consent forms were completed, the participants were seated at computers and instructed on the procedures. Participants were provided with headphones in all conditions (video, audio, or written) to dampen sound and direct attention. Each participant was presented with only one version of the testimony in one stimulus material format. After presentation of the testimony materials, each participant was presented with the same set of questions regarding the testimony (see Appendix B). They were asked to answer each question in order, and saw only one question at a time. They were instructed to not look back at their previous answers after they had turned the page to the next question.

The participants were presented with the following verdict questions, in the following order: (a) "Do you think DeShaun Brown, the defendant, is: Guilty or Not Guilty" and (b) "How confident do you feel about your verdict of Guilty or Not Guilty?" (verdict and verdict confidence: 6-point scale with 0 labeled as "unsure" and 6 labeled as "sure"). Following the verdict questions, questions assessing testimony comprehension were asked, such as (c) "How good of a look did the witness get at the perpetrator (robber)?" (quality of observation: 6-point scale, 1=extremely poor look to 6=extremely good look), (d) "Do you think the witness correctly identified the perpetrator (robber)? (Did she identify the bad guy?)" (identification accuracy: 6-point scale, 1=definitely yes to 6=definitely no). Open-ended questions were also used to assess memory and comprehension of the testimony, asking the participant to state the time of day, lighting conditions, describe the witness view of the perpetrator, weapon (if any), and disguise (if

any). The participants were asked to provide demographics, indicating their age, sex, ethnicity, first language spoken (first language English - yes or no?), English reading and writing proficiency, prior jury service, and whether he or she had been a victim or witness of a crime in the prior two years. At the end of the experiment, participants were fully debriefed and given credit for participation.

Analyses were conducted to examine the potential differences in ratings of guilty verdicts and probability of guilt across viewing conditions and differing stimulus material formats. Analyses were also conducted to check for potential effects of attorney roles, effectiveness of manipulation, participant demographics, and memory for detail.

Results and Discussion

The primary analysis examined the extent to which there were differences in participants' sensitivity to the eyewitnessing conditions as a function of the four stimulus material formats. First, as a preliminary analysis, I analyzed participants' memory for the testimony. These results are presented below.

Recall of testimony. Participants were asked four open-ended questions about the testimony, three of which differentiated between good and poor witnessing conditions. They were asked about the time of day, the lighting, and how much of the perpetrator the witness saw. They were also asked about the weapon used by the perpetrator. Accuracy for these questions was generally very good. All participants correctly indicated that the perpetrator used a gun in the commission of the robbery. Accuracy for the other three questions ranged from .944 to .976. Overall accuracy across the four questions was calculated as a score between 1 and 4 for each participant. These

accuracy scores were also generally very high, with 90.4 % (536 of 594) of participants answering all four questions correctly.

What relationship did accuracy for the testimony have with assessments of the witnessing conditions, judgments of the likelihood that the witness's identification was correct, and the proportions of guilty verdicts? The relevant data are shown in Table 3. The Table shows the average ratings and proportions of guilty verdicts for good and poor witnessing conditions. Pearson's *r* was calculated as a measure of effect size.

Number Correct	Measure	Good	Poor	*r*
1	Observation	3.500	4.000	–.577
	ID Accuracy	4.500	4.500	.000
	Guilty Verdict	1.000	1.000	.000
	n	2	2	4
2	Observation	4.167	3.750	.186
	ID Accuracy	4.333	4.500	–.073
	Guilty Verdict	0.667	0.750	–.089
	n	6	4	10
3	Observation	3.952	2.773	.492
	ID Accuracy	4.048	3.696	.122
	Guilty Verdict	0.619	0.652	–.034
	n	21	23	44
4	Observation	4.493	2.564	.725
	ID Accuracy	4.419	3.248	.446
	Guilty Verdict	0.726	0.357	.370
	n	270	266	536

Table 3. Assessment of Observation, Likelihood of Correct Identification (ID Accuracy), and Guilty Verdicts for Good and Poor Witnessing Conditions as a Function of Accuracy of Recalling Testimony.

The interpretation of the results for less accurate participants must be considered carefully due to the very small number of observations. To increase the stability of the

data, the results for participants with accuracy scores of 1 and 2 were combined, and the

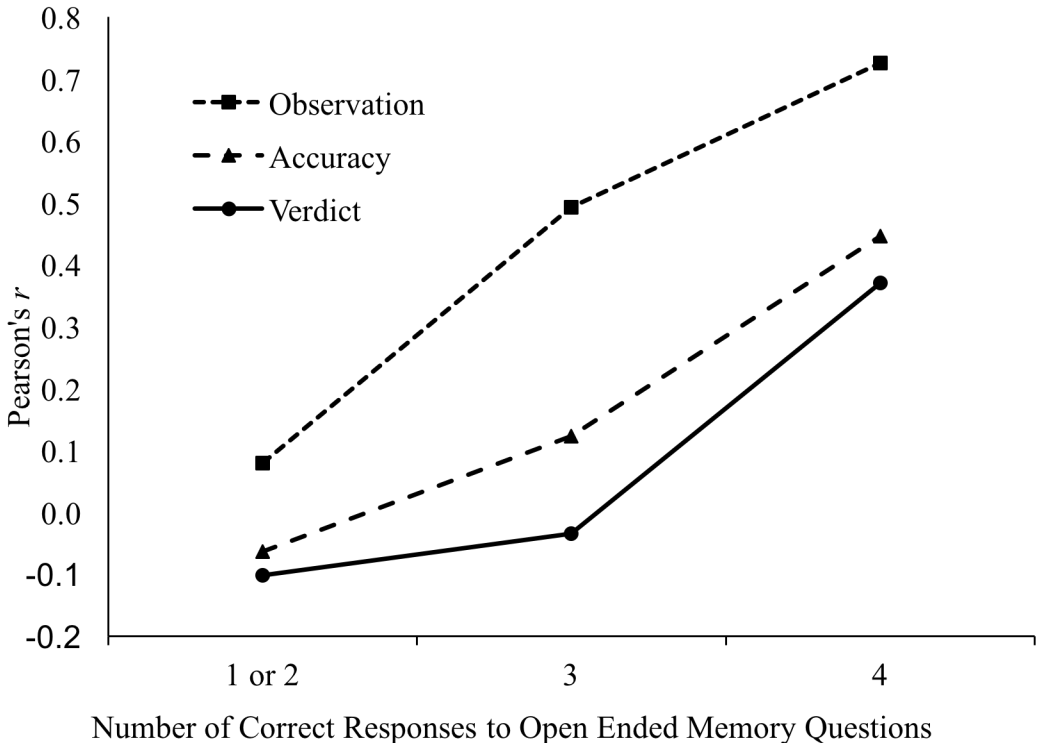

Figure 5. Relationship of Viewing Condition to Quality of Observation Ratings, Accuracy of Identification Ratings, and Verdicts, by Accuracy of Recall Assessment

Figure 5 shows that the effect sizes are very close to zero for participants who recalled only one or two items from the testimony questions correctly. Such poor recall of the testimony, combined with a clear inability to distinguish between good and poor witnessing conditions, raises the possibility that these 14 participants paid little or no attention to the presentation of the testimony, failed to follow instructions, or simply did not fully participate in the study. Of those 14 participants, seven stated that English was their first language and, of the other seven, six rated their English proficiency as "very good," with only one rating their English proficiency as "Ok." Therefore, it was not a

lack of English language comprehension, in at least 13 of the 14 participant's cases, which caused poor comprehension or recall of testimony detail. To the extent that the poor performance reflected a failure to follow instructions or properly participate in the study, it seemed reasonable to exclude them from subsequent analyses. However, we should note that these participants were not uniformly distributed across the four stimulus format conditions. Eleven of the fourteen excluded participants were from the audio or audiovisual conditions and only three were from the two written conditions, $\chi^2(1, N = 594) = 4.464$, $p = .035$, $r = .087$. This disproportionate distribution could have been due to (a) it being more difficult to extract the key information from the audio and audiovisual conditions, or (b) it being easier to "tune out" in those conditions. The first possibility suggests a true memory effect, which is potentially interesting. The second possibility simply reflects differential ease of non-compliance, which is much less interesting. Although we cannot distinguish between these two possibilities, the data from these 14 participants are excluded from all subsequent analyses (thus, study $N = 580$).

Although there was, overall, a high percentage of participants who answered all four open-ended memory questions correctly, there were differences in recall between formats. Accurate recall of all four questions was 90.1% in the video format ($m = 3.874$, $sd = .405$), 84.0% in the audio format ($m = 3.773$, $sd = .592$), 96.6% in the transcript format ($m = 3.959$, $sd = .232$), and 90.5% in the summary format ($m = 3.885$, $sd = .396$). Whereas recall could have been a function of extracting key information, which could be more difficult in an audio or audiovisual format, recall performance in the written

summary format was nearly equal to that of the video format. Of the four formats, participants reading the full transcript exhibited the best recall, and those hearing the audio testimony exhibited the poorest recall for the pertinent details of the testimony. However, recall was fairly stable (four out of four correct answers) across good and poor viewing conditions by format as follows: video (90.8% and 89.3%), audio (83.1% and 84.9%), transcript (97.2% and 95.9%), and summary (90.5% and 90.5%).

	14 Low Memory Participants Included			14 Low Memory Participants Excluded		
	Good Viewing Condition	Poor Viewing Condition	*r*	Good Viewing Condition	Poor Viewing Condition	*R*
Video	.697	.520	.182	.699	.514	.189
Audio	.792	.534	.274	.795	.529	.282
Transcript	.722	.233	.490	.722	.222	.501
Summary	.622	.270	.393	.658	.260	.399

Table 4. Pattern of Verdicts for Study 2, Either Including or Excluding the 14 Low Memory Participants, Expressed as Proportions of Guilty Verdicts and Relationship of Viewing Condition to Verdicts.

Before moving on, we note two potentially interesting relationships between accuracy in recalling the testimony and verdict decisions. First, the likelihood of a guilty verdict decreased as the accuracy of recall increased. The proportions of guilty verdicts were 1.0, .700, .636, and .543, for accuracy scores of 1, 2, 3, and 4, respectively, $t(2) = 3.805$, $p = .031$ (one-tailed), $r = -.937$. Those who remembered best convicted least. Second, the proportion of guilty verdicts did not differ across good and poor witnessing conditions for participants who were less than 100% accurate on the questions about the testimony. This pattern held irrespective of whether the data from the 14 lowest-performing participants were included or excluded. In Table 4, it is clear that the pattern

of verdicts seen with the 14 participants included is virtually the same as that seen when the 14 poor memory participants are excluded.

In both cases, without the 14 low recall participants, the proportions of guilt in the two viewing conditions are virtually identical across formats. There is a strong relationship between verdict and viewing condition (indicating the greatest disparity in verdicts between viewing conditions) is seen in the testimony format. The next highest correlation is a moderate to strong result in the summary format. The audio-only format shows a small to moderate correlation and finally the video format shows a relatively small correlation (compared to the transcript format) between viewing condition and verdict (the least disparity in verdicts), whether those participants are included or excluded.

Attorney role effects, manipulation effectiveness, and demographics. Using chi-square analysis, there were no effects on the dichotomous variable of verdict of the attorney counterbalancing (switching roles for prosecution and defense). Only the video and audio formats had the counterbalancing, as only in those formats were the attorney-actors seen/heard. In the video format, there was no significant difference in verdicts between the attorney roles when collapsing across viewing conditions ($p = .591$), or separately by viewing condition (good viewing condition, $p = .345$; poor viewing condition, $p = .990$). In the audio format, there was no significant difference in verdict by attorney roles across viewing conditions ($p = .111$), or separately by viewing condition (good viewing condition, $p = .204$; poor viewing condition, $p = .231$).

The manipulation of viewing condition (good or poor) was effective across

formats. Participants rated the quality of the observation as significantly better in the good viewing condition (m_{good} = 4.45), than in the poor viewing condition (m_{poor} = 2.58), $t(577) = 24.162$, $p < .001$, $r = .709$.

There was no significant effect of participant ethnicity on verdicts, $F(5, 579) = 5.881$, $p = .215$, $r = .100$. There was, however, a significant effect of sex on verdict collapsing across formats and viewing conditions, with females more likely, overall, to render guilty verdicts (58.8%) than males (48.3%), $\chi^2(1, N = 580) = 5.881$, $p = .019$, $r = .101$. However, when analyzing by format, collapsing across viewing condition, there was only one marginally significant result for verdict variation by sex, in the audio format ($p = .067$, one-tailed, 70.0% guilty verdicts$_{female}$, 57.4% guilty verdicts$_{male}$). video ($p = .141$, 64.2% guilty verdicts$_{female}$, 55.4% guilty verdicts$_{male}$), transcript ($p = .273$, 49.5% guilty verdicts$_{female}$, 44.2% guilty verdicts$_{male}$), and summary ($p = .134$, 50.0% guilty verdicts$_{female}$, 40.7% guilty verdicts$_{male}$) formats showed no effect of sex on verdict (all p's one-tailed). Females did outnumber males in the experiment (64% female, 36% male).

Verdicts. To the extent that jurors are more sensitive to the conditions of observation, guilty verdicts should be more likely when the eyewitness viewing conditions (observation conditions at the time of the crime) are described as good compared to when those conditions are described as poor. Thus, the key data are the proportions of guilty verdicts obtained in the two viewing conditions. The proportions of guilty and not-guilty verdicts are presented in Table 5 for all four stimulus material formats.

The proportions of guilty and not-guilty verdicts were compared in separate 2 x 2 Chi-square calculations comparing good and poor conditions of observation for each stimulus presentation condition. A statistically significant Chi-square indicates that the proportions of guilty and not-guilty verdicts differed across good and poor conditions of observation – indicating that participants were sensitive to the conditions of observation and that their verdicts reflected the differences in those conditions.

Two measures of sensitivity are given in the Table, Pearson's *r*, calculated from the Chi-Square statistic, and *d'*, derived from signal detection theory, using the proportion of guilty verdicts in the good observation condition as the "hit" rate and the proportion of guilty verdicts in the poor observation condition as the "false alarm" rate.

	Good	Poor	*d'*	*Zr*	*r*	*n*
Video	.699	.514	0.556	.191	.189	147
Audio	.795	.529	0.896	.289	.282	143
Transcript	.722	.222	1.354	.551	.501	144
Summary	.658	.260	1.050	.422	.399	146

Table 5. Proportion of Guilty Verdicts, by Viewing Condition and Testimony Presentation, and Stimulus Material Format of the Testimony.

The statistical details for Chi square and Pearson's *r* are as follows: For the video condition, $\chi^2(1, n = 147) = 5.272$, $p = .0217$, $r = .189$; for the audio condition $\chi^2(1, n = 143) = 11.334$, $p = .0008$, $r = .282$; for the transcript condition, $\chi^2(1, n = 144) = 36.111$, $p < .00001$, $r = .501$; and for the summary condition, $\chi^2(1, n = 146) = 23.198$, $p < .0001$, $r = .399$. These analyses show that participants were sensitive to the conditions of observation as they were described by the witness in her testimony. For comparison, if desired, Appendix C contains the proportions of guilty verdicts, by viewing condition and testimony presentation format, and the related statistics, with no participants excluded.

The key question is whether this sensitivity varied across testimony presentation conditions. The most straightforward assessment of this question examines the conditions for which the verbal content was constant, comparing video, audio, and transcript conditions.

The critical prediction, that the magnitude of the effect would be ordered from, smallest to largest, video < audio < transcript, was confirmed. The comparison of *d'*, as a measure of discriminability (as seen on Table 5), showed that sensitivity varied as predicted, transcript > audio > video. The summary format fell between the transcript and audio formats. Pairwise comparisons were conducted by calculating the Pearson's *r* effect size from the χ^2 statistic, and comparing the *z*-transformed *r*'s. Comparisons of the Pearson *r* effect sizes in Table 5 show that the magnitude of the sensitivity effect was statistically significant comparing transcript to audio ($z = 2.19$, $p = .0143$), and transcript to video ($z = 3.03$, $p = .0012$), but not audio to video ($z = .83$, $p = .2033$). The summary condition, which was included because many experiments of this type use summarized testimony, and which provided considerably less text, showed a non-significantly smaller sensitivity effect compared to the transcript condition, ($z = 1.08$, $p = .1401$), a larger sensitivity effect than the video condition ($z = 1.96$, $p = .025$), and a non-significantly larger sensitivity effect than audio condition ($z = 1.12$, $p = .131$).

The variation in sensitivity across format conditions was due primarily to differences in the poor witnessing condition, $\chi^2(3, n = 289) = 24.191$, $p = .000023$. The proportions of guilty verdicts were lower for the transcript (.222) and summary conditions (.260) than for the video (.514) and audio (.529) conditions. The variability in

the proportions of guilty verdicts in the good witnessing condition was much smaller and statistically non-significant, $\chi^2(3, n = 291) = 3.572$, $p = .3115$. Those proportions were .699 (Video), .795 (Audio), .722 (Transcript), and .658 (Summary).

A separate analysis was restricted to only those guilty verdicts that were given with high confidence. The rationale for this analysis is based on the BARD (Beyond a Reasonable Doubt) standard of proof for convicting a defendant in the U.S. Although the specifics of the BARD standard are not precisely or quantitatively defined in the jury instructions of any U.S. State or Federal Court, the standard implies that a juror who believes that the defendant is only *probably* guilty may, and perhaps should, render a not-guilty verdict. Thus, only jurors who are highly confident of the defendant's guilt should, in fact, render a guilty verdict. It is important to determine whether the pattern of results shown in the main analysis (Table 5) is shown when only high-confidence guilty verdicts are considered.

	Good	Poor	*d'*	*Zr*	*r*	*n*
Video	.329	.203	0.389	.144	.143	147
Audio	.507	.186	0.911	.351	.337	143
Transcript	.431	.069	1.306	.444	.417	144
Summary	.206	.069	0.665	,202	.199	146

Table 6. High Confidence Guilty Verdicts, by Viewing Condition and Testimony Presentation Condition.

The results are shown in Table 6. In this analysis, guilty verdicts made with confidence levels of 5 or 6 are categorized as guilty verdicts; all other responses – not-guilty verdicts and guilty verdicts made with lower levels of confidence – were categorized as not-guilty verdicts. The ordering of the effect sizes was very similar to that shown in the full data set: transcript > audio > video. The summary condition fell in

between the audio and video conditions. The statistical analysis of these results is as follows: For the video condition $\chi^2(1, n = 147) = 2.996$, $p = .0835$, $r = .143$; for the audio condition $\chi^2(1, n = 143) = 16.206$, $p < .00001$, $r = .337$; for the transcript condition $\chi^2(1, n = 144) = 25.037$, $p < .00001$, $r = .417$; and for the summary condition $\chi^2(1, n = 146) = 5.794$, $p = .0161$, $r = .199$. These analyses exhibit a pattern of results that is consistent with the results for the verdict analyses that did not factor in verdict confidence (i.e., simple verdicts, rather than those "beyond a reasonable doubt"). Once again, the findings for the transcript, audio, and summary formats were significant, and those for the video format were not. Additionally, the order of the effect sizes did not vary.

Conceptual model. As illustrated in Figure 6, it makes sense to consider the witnessing, identification, and guilt questions in that order: How good a look did the witness get (quality of the eyewitness observation)? How likely is it that her identification of the suspect is correct (ID accuracy)? Is the defendant guilty, and how

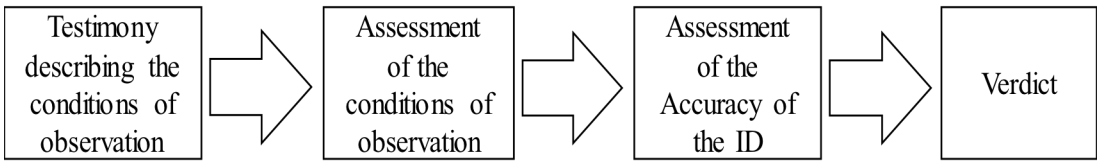

Figure 6. Conceptual ordering of questions from testimony exposure to verdict.

confident are you in that judgment of guilt (verdict and confidence)? The basis of this conceptual ordering is that the juror's determination of guilt is based, in part, on the subjective likelihood that the eyewitness's identification is correct, and the subjective likelihood that the eyewitness identification is correct is based, in part, on the perceived quality of the eyewitnessing conditions.

Assessment of witnessing conditions and ratings of identification accuracy. Participants were asked two questions about the quality of the eyewitness's observations (observation) and the likely accuracy of the eyewitness identification (ID accuracy). Each of these was based on a six-point scale. In addition, participants' verdicts were combined with their verdict confidence assessments to create a six-point guilt scale, ranging from 1 (high confidence, not guilty) to 6 (high confidence, guilty). Thus, all three responses, observation, ID accuracy, and verdict, were measured on similar six-point scales. The results were analyzed in two ways – through analyses of variance with witnessing condition (good or poor) and presentation format (video "V", audio "A", transcript "T", or summary "S") as between-subject factors, and also through separate *t*-tests for each presentation format, each one comparing good and poor witnessing conditions. Variation in sensitivity to witnessing conditions can be evaluated in two ways – through the interaction term for the analysis of variance, and through a comparison of Pearson's *r* effect sizes, calculated from the separate *t*-values. The Pearson's *r* effect sizes are listed in Table 7, and plotted in Figure 7.

	Observation	ID Accuracy	Verdict
Video	.630	.295	.189
Audio	.757	.401	.282
Transcript	.685	.413	.501
Summary	.774	.534	.399

Table 7. Sensitivity (Pearson's *r*) to Viewing Conditions for Assessment of Observation, Likelihood of a Correct Identification (ID Accuracy), and Verdict for Video, Audio, Transcript, and Summary Testimony Presentation Formats.

The means for these analyses are listed in Tables 6 and 7, and plotted in Figures 7 and 8. Two general trends are shown in Figure 7. First, the variation in the effect sizes across the four stimulus material formats is smallest for the question about the quality of the observation (*sd* = .066, range = .143), larger for the question about the accuracy of the witness's identification (*sd* = .098, range = .239), and largest for the question about guilt (*sd* = .122, range = .294). Second, the conceptual ordering of the effects, depicted in Figure 3, shows that, with the notable exception of the transcript condition, the witnessing conditions were associated with smaller effects with each added conceptual step.

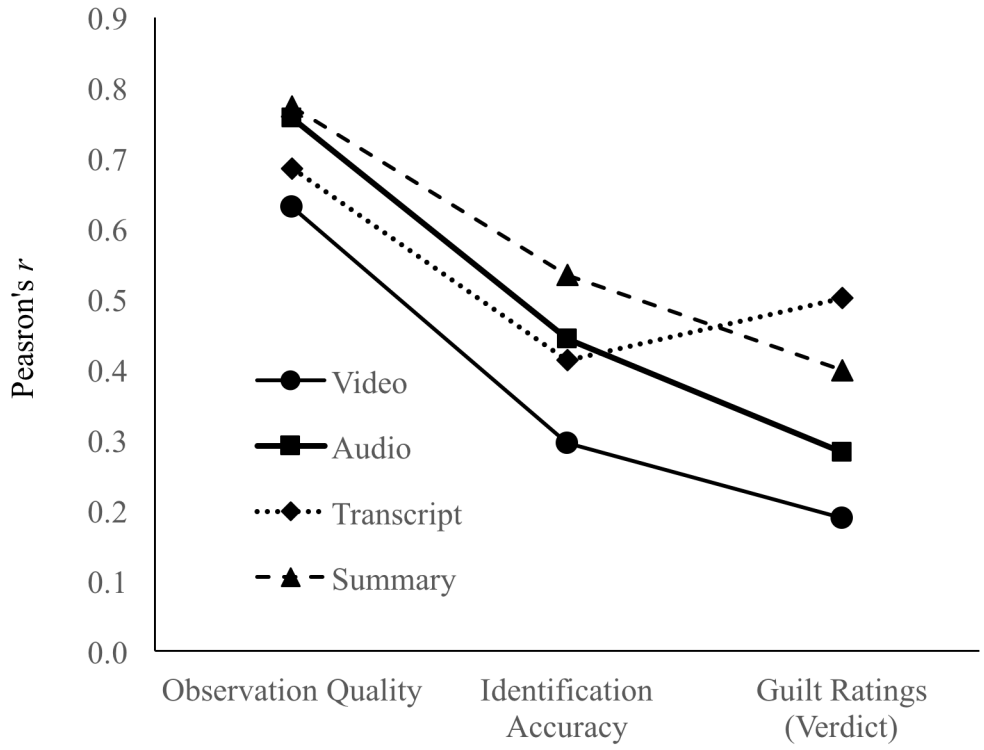

Figure 7. Sensitivity (Pearson's *r*) to Viewing Conditions for Assessment of Observation, Likelihood of a Correct Identification (ID Accuracy), and Verdict for Video, Audio, Transcript, and Summary Testimony Presentation Formats.

Based on the conceptual model, there should be a strong correlation between assessments of the observation conditions during the crime and the likelihood that the identification is correct. This relationship is illustrated in Figure 8. The diagonal represents the outcome if the means of the identification accuracy ratings mapped on exactly to the assessed conditions of observation, specifically if participants who rated the conditions of observation as a 1 gave identification accuracy ratings of 1, participants who rated the conditions of observation as a 2 gave identification accuracy ratings of 2, and so on.

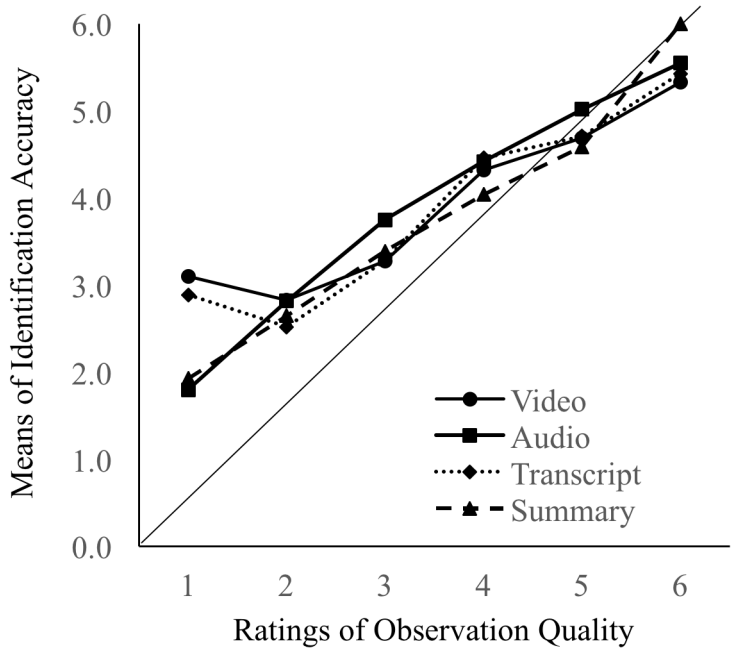

Figure 8. Likelihood of an Accurate Identification as a function of Observation Ratings, for Video, Audio, Transcript, and Summary Conditions.

There were strong correlations between these variables in all four formats, with the strongest relationships found in the audio ($r = .727$, $n = 143$, $p < .001$) and summary ($r = .716$, $n = 146$, $p < .001$) formats, with less robust relationships found in the transcript ($r = .655$, $n = 143$, $p < .001$) and video ($r = .555$, $n = 147$, $p < .001$, all p's one-tailed).

There was a slight tendency for the participants to overestimate the identification accuracy when they assessed the observation conditions as very poor, particularly in the video and transcript formats.

Assessed conditions of observation and ratings of guilt. The final two analyses examine the relationship between the assessed quality of the observation and guilt ratings, and the assessed ID accuracy and guilt ratings. To demonstrate the extent to which guilt ratings are associated with these ratings of observation and accuracy, these relationships are shown in Figure 7 for the observation assessments, and in Figure 8 for the rated accuracy of the identification.

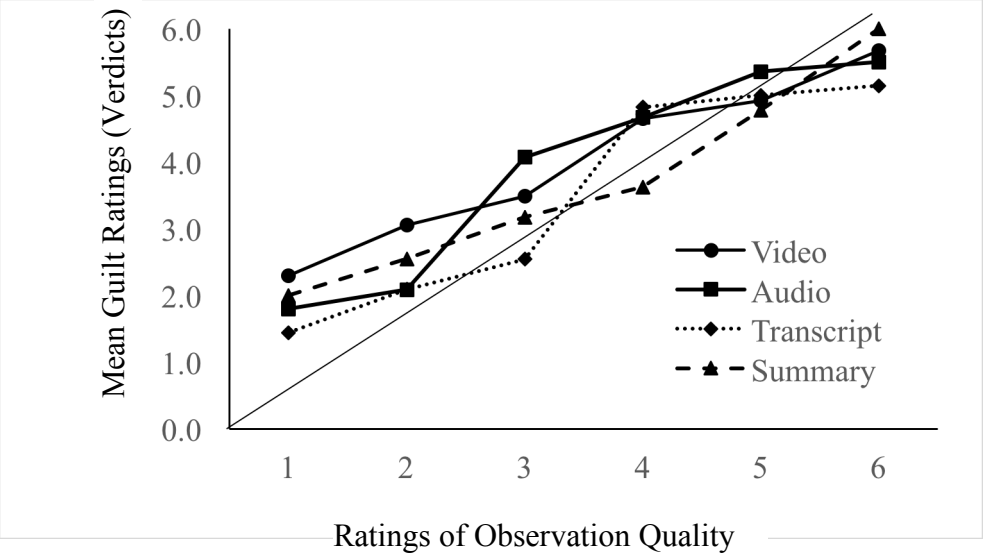

Figure 9. Mean Guilt Ratings as a Function of Observation Quality Ratings

Intuitively, it is reasonable to expect strong associations between the assessment of the observation (observation quality) and the likely ID accuracy, and between the ID accuracy and the judgment of the defendant's guilt (verdict). Indeed, both of these

correlations were very strong, $r = .670$ ($N = 579$, $p < .0001$, observation quality) and $r = .705$ ($N = 580$, $p < .0001$ identification accuracy and verdict).

Figure 9 plots the average guilt ratings for each level of the assessed quality of the observation during the crime. The Transcript and Audio formats showed the strongest relationship between mean ratings of guilt and the rated quality of the observation ($r = .669$, $n = 143$, $p < .001$ and $r = .549$, $n = 143$, $p < .001$), with summary and video correlations of ($r = .503$, $n = 146$, $p < .001$ and $r = .418$, $n = 147$, $p < .001$). As shown in Figure 9, all formats show somewhat inflated guilt ratings when the observation quality was rated as extremely poor to somewhat good (1, 2, 3, or 4), a trend which lessened as the participants rated the quality of the observation as very or extremely good (5 or 6).

Note: In the transcript condition, there were no identification accuracy ratings = 1, the data for the identification accuracy ratings in the transcript condition ranged from 2 to 6.

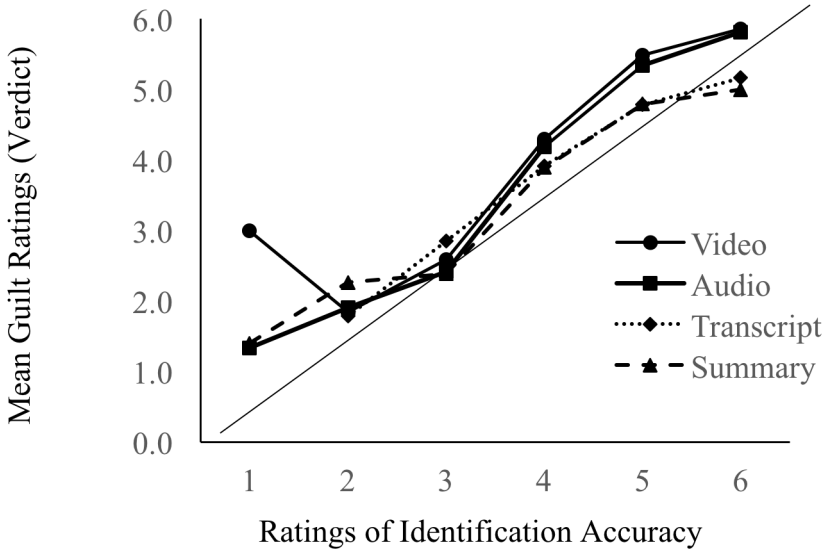

Figure 10. Mean Guilt Ratings for Each Level of Rated Identification Accuracy

For those reading the summarized testimony, when the observation quality was rated as

4, 5, or 6, mean ratings of guilt were 3.63, 4.78, and 6.00, respectively.

Guilt ratings and Identification (ID) accuracy. Figure 10 plots average guilt ratings for each level of ID accuracy ratings. Again, the diagonal represents the outcome if the means of the guilt ratings mapped on exactly to the ID accuracy ratings. Guilt ratings were more strongly correlated with ID accuracy ratings for the Video and Audio conditions ($r = .735$, $n = 147$, $p < .001$ and $r = .718$, $n = 143$, $p < .001$) than for the Transcript and Summary conditions ($r = .646$, $n = 144$, $p < .001$ and $r = .636$, $n = 146$, $p < .001$). There is what appears to be an unusually high mean rating of guilt in the video condition, but as there were only 3 participants in the video condition that gave a rating of 1 for identification accuracy (two guilt ratings of 2 and one guilt rating of 5) this limits possible speculation about verdict inflation for that format at the lowest rating of the scale. Notably, however, the Video and Audio conditions separate from the Transcript and Summary conditions at the high end of the ID accuracy scale. Thus, for ID accuracy ratings of 4, 5, and 6, the Guilt ratings tend to be higher for the Video and Audio conditions than for the Transcript and Summary Conditions. Considered another way, the proportions of guilty verdicts for a given level of rated ID accuracy were higher for the Video (.888) and Audio (.867) conditions than for the Transcript (.750) and Summary (.753) formats, collapsed across viewing conditions. This was consistent with Martin et al.'s (2007) findings, but not Bermant et al.'s (1974) results. However, the reduced number of guilty verdicts in the Transcript and Summary conditions, when collapsing across formats, is attributable to the sensitivity shown in those formats, as reflected by the lower proportions of guilty verdicts in the poor viewing conditions.

Chapter 4 – General Discussion

This final chapter summarizes the results, explores theoretical explanations and implications of those results, and closes with a discussion about the limitations of the study and future studies to examine format effects in jury simulation studies.

Summary of Results

Consistent with predictions, participants who read the full testimony transcript evinced more sensitivity to eyewitness viewing factors than those who received audio or video testimony, as shown by the proportions of verdicts between good and poor viewing conditions and *d'*. This finding was robust, even when applying a stricter standard of guilt to approximate the BARD standard. The summary format resulted in a lesser degree of participant sensitivity. Although there were fewer references to the nature of the observation in the summary, the lack of sensitivity does not seem to be a product of the participants' perceiving and understanding the elements of the testimony related to the observation. In all four formats, video, audio, transcript, and summary, there was a strong relationship between good or poor viewing conditions and ratings of observation. The loss in participant sensitivity occurred in the progression from the participant's understanding of the observation conditions, to rating the accuracy of the identification, to applying that belief in identification accuracy to the verdict. In the video format, the relationship between rated observation and viewing condition was strong (r = .630), the relationship between identification accuracy and viewing condition was moderate (r = .295), and the relationship between verdict and viewing condition was weak to moderate (r = .195). All formats suffered some loss in relational strength along this conceptual

pathway from testimony to verdict, with audio moving from very strong (viewing condition and observation, $r = .757$) to moderately strong (viewing condition and identification, $r = .401$), to moderate (viewing condition and verdict, $r = .343$), summary moving from $r = .774$ to $r = .534$, and dropping to $r = .389$. Only the transcript format varied from this pattern, starting with a strong relationship between viewing condition and observation ($r = .685$), the relationship between viewing condition and identification accuracy weakening ($r = .413$), then rebounding with the relationship of viewing condition and verdict ($r = .490$). This drop in correlation along the conceptual pathway from ratings of observation to verdict indicates that in all formats the participants recognized the significance of the viewing conditions, but they were less adept at applying that information to their verdicts in the video and audio formats. The critical question is why?

A series of analyses examined the extent to which variation in sensitivity was tied to variations in memory for the critical testimony. There were differences in accurate recall of details pertinent to the eyewitness identification accuracy across the formats. The participants reading the transcript had the best recall, with 96.6% of them accurately recalling four out of four items, participants receiving the video and summary formats had very similar rates of recall accuracy, 90.1% and 90.5% respectively, and those who heard the audio testimony exhibited the lowest percentage of accurate recall at 84.0%. If recall were driving the differences in sensitivity, based on differences in verdict between formats, it would be expected that the audio format would show the least sensitivity, and this should also be reflected in the correlations between the three variables of interest:

ratings of observation quality, ratings of identification accuracy, and verdicts. This was not the observed pattern of results. If recall differences between good and poor were driving participant sensitivity, it would be expected that there would be significantly different rates of accurate recall by format between viewing conditions. This was also not found to be the case. However, the transcript format did show the highest rate of participant recall and the most participant sensitivity; therefore recall accuracy cannot be ruled out as a component. Nonetheless, if it were the primary factor driving sensitivity, a different pattern of results would be expected, with transcript > summary ≈ video > audio.

There was a strong correlation between viewing condition and ratings of observation quality overall (r = .698), and in all four formats. This relationship was somewhat affected by recall, with participants responding correctly on three out of four recall questions having a strong correlation between viewing condition and their ratings of observation quality (r = .492), whereas the correlation for the more accurate participants was very strong (r = .725). All formats showed slightly inflated identification accuracy rating means over the lowest three ratings of observation quality, although the overall correlation between these two variables was very strong (r = .670). There was also a strong correlation between verdict and observation ratings (r = .540), but by format, mean ratings of guilt are somewhat inflated, particularly over the lower ratings of observation quality.

Ratings of identification accuracy were not as strongly correlated to viewing conditions overall (r = .411), and varied more between formats (r_{video} = .295, r_{audio} = .401,

$r_{transcript}$ = .413, $r_{summary}$ = .534). Recall may have influenced ratings of accuracy, as those with only three correct recall questions showed a weak correlation between viewing condition and accuracy (r = .122) and those with four out of four correct showed a moderately strong correlation between variables (r = .446). Collapsing across formats there was a very strong correlation between ratings of accuracy and verdict (r = .690).

Overall, then, while recall may have been a minor factor, the differences seen between viewing conditions and formats cannot be attributed to inattention, inability to glean the pertinent details from the testimony, or memory problems. The differences are also not the product of a simple criterion shift. If the participants were simply made more cautious or more skeptical by a written format than the video or audio formats, the verdicts would drop in both viewing conditions, reducing guilty verdicts overall, rather than resulting in better discrimination between viewing conditions reflected by verdicts, and a resulting increase in *d*'. When a more stringent criterion is imposed, such as raising the bar as to what constitutes a guilty verdict, factoring in confidence and considering only guilty verdicts made with high confidence, such as would be expected with the BARD standard (guilt beyond a reasonable doubt), the same pattern of results is seen across formats.

Theory

Bermant et al. (1974) found that participants were less likely to render any kind of guilty verdict (first degree murder, second degree murder, manslaughter, etc.) when viewing an audiovisual presentation or hearing audiotaped testimony than then reading a transcript or summary. Martin et al. (2007) found that participants were more likely to

vote guilty when viewing videotaped testimony than when reading testimony. Others have found patterns of verdicts more similar to Martin et al. than Bermant et al., such as Juhnke et al. (1979) finding higher rates of guilty verdicts in the video and audio formats than in the written transcript or summary formats. This led Bornstein (1999) to conclude that "studies that have directly compared presentation media…fail to offer consistent findings" (p. 82). It is clear in the data of this study that the pattern of guilty verdicts, although slightly higher in the video/audio formats (74.7%) in the good condition than in the transcript/summary formats (69.0%), is not significantly different ($t(289) = 1.078$, $p = .141$). The appearance of a greater number of guilty verdicts in the video and audio formats (63.4%) than in transcript and summary formats (46.6%) when collapsing across viewing conditions is a product of the extreme drop in guilty verdicts in the poor viewing condition for participants reading the testimony, creating a significant difference in guilty verdicts between the video/audio formats (52.0%) and the transcript summary formats (24.1%) in the poor viewing condition ($t(287) = 5.091$, $p < .001$).

Is it the more simplified formatting exaggerating the details as suggested by Weiten and Diamond (1979) and Diamond (1997)? Are people better at distinguishing good versus poor viewing conditions when they have less information (as when they read it) than when they have more information (when they either hear or see and hear it)? That does not seem to be the issue. Although there were differences in recall accuracy across formats, such that recall ranked across formats was transcript > summary ≈ video > audio, in all four formats, a significant number of the participants recognized that the viewing conditions affected the quality of the observation ($r_{video} = .6301$, $r_{audio} = .7568$,

$r_{transcript}$ = .6853, and $r_{summary}$ =.7735). They understood that. The disconnect between the facts surrounding the observation came later, during the ratings of identification accuracy and the verdict. Guilty verdicts were largely consistent in the good viewing condition across formats. However, in the poor viewing condition, there was a dramatic drop in guilty verdicts when the material was read, that was not seen in the video and audio formats. The participants did not apply what they knew about the observation as effectively in the video and audio formats as they did in the transcript format.

Diamond (1997) suggested that the more simplified and artificial scenarios artificially magnified effect sizes. Diamond was correct in that the effect sizes were larger in the less complex (lacking audio and visual cues) written formats, but Diamond suggested that enhanced effect sizes would be a product of more starkly presented, therefore more clearly perceived, details. Although Diamond was correct about what occurs, the data suggest that there is a different mechanism underlying the pattern of results, why they were inflated. It is not that the details stand out more, or they get the information better, but that there is a difference in the participant's ability to apply that knowledge to the verdict. They apply the knowledge better when they read it than when they see or hear it.

Bond and DePaulo (2006) have found differences between formats when examining deception detection. They noted that in video-only formats (no audio) participants were more likely to rely on stereotypes, and to attempt to read facial and body cues that actually impaired their ability to accurately detection deception. In video-only formats, participants who only had access to the face or the body were less

successful at detecting deception than participants who could see both the face and body. Overall, detecting deception from video-only messages was less successful than from transcripts, but they did not find significant differences in the ability to detect deception from transcripts, audio-only, or audiovisual messages. However, when audio was used, either alone or in an audiovisual format, such that the messages could be heard, participants exhibited a "truthfulness bias," believing more than 50% of the messages presented to be truthful (p. 225). Bond and DePaulo determined that messages in transcript form were judged as less truthful than messages than audio or audiovisual messages, and video-only messages were judged to be less truthful than transcripts.

It must be noted that calling this skepticism a "truthfulness bias" in relation to the present data would be inaccurate, as there is no actual ground truth in this experiment, and thus, no way to judge responses in reference to some objective truth. If, however, there is some form of skepticism that manifests more strongly when material is read than when it is heard, you would expect to see the application of knowledge diminish more in the written formats than in the video/audio formats, and in both viewing conditions equally. In the video and audio formats the correlations between viewing condition and ratings of identification accuracy, then the subsequent verdict, showed diminishing relationships. In the video format, $r_{observation} = .6301$, $r_{accuracy} = .2953$ (-.3348), and $r_{verdict} = .1954$ (-.1394), whereas in the audio format, $r_{observation} = .7568$, $r_{accuracy} = .4011$ (-.3557), and $r_{verdict} = .3432$ (-.0579). If, as suggested by Bond and DePaulo, there is a bias to believe, or over believe, the stated messages in auditory formats, perhaps the participants understood the facts pertaining to the observation, but were unwilling to disbelieve the

witness when she identified the defendant as the perpetrator. It is important, nonetheless, to remember that this cannot be a truthfulness bias if there is no objective truth to be judged.

Is it that the participants were looking for cues to accuracy (or inaccuracy, or deception) in the video or audio formats, such as confidence or verbal/non-verbal affect, that simply did not vary sufficiently between experimental video and audio conditions, but were absent (and were therefore not expected or missed) in the written formats? That is a distinct possibility. Bond and DePaulo (2006) note the reliance of receivers (i.e., those receiving a message and attempting to determine truth from lies) depend on visual cues. The nature of experimental control is to hold constant all possible variables save for the chosen, manipulated variables, which in the case of the experiment under discussion were viewing condition and format. The actor playing the eyewitness was asked to not overdramatize her role, but deliver the lines as consistently as possible for both the good and poor viewing condition testimony videos. The other actors were asked to behave professionally and consistently across the different versions of testimony as befitted their roles (e.g., judge, attorneys, bailiff). The audio recording was recorded concurrently with the video. Vocal inflection, facial expression, body movement, emphasis of speech, verbal and non-verbal cues were scripted and kept as consistent as feasibly possible. As a result of this experimental control, the participants would be privy to relatively few visual and auditory cues aside from the actual scripted words spoken by the actors and the odd, occasional, random movement or expression.

The core issue is the extent to which non-verbal cues to confidence were

eliminated from the testimony and therefore unavailable to participants. As noted before, the view regarding the utility of confidence as a cue to accuracy has changed over time. Cutler and Penrod (1995) argued that jurors erroneously tend toward an overreliance on eyewitness confidence as an index of reliability. This was held to be unfortunate, as they believed confidence to be an unreliable accuracy cue. However, more recent analyses strongly suggest that eyewitness confidence, perhaps not at the time of the trial, but at the time of the initial identification was made, is highly diagnostic of identification accuracy (Wixted and Wells 2017; Wixted et al., 2015). However, confidence, not being a variable of interest in this study, was not manipulated and did not vary appreciably between versions of the testimony.

Implications

In this study, participants exposed to video and audio testimony clearly related viewing conditions to observation quality, indicating they understood the meaning of those details. Therefore, jurors in real trials are likely, in fact, more sensitive to the details of the testimony that relate to the conditions of observation than previously suggested by video format methodologies. This may mean that expert testimony, meant to educate jurors about these factors, is unnecessary. Participants did not, however, show as much sensitivity based on verdict as those reading the testimony. This is likely not a problem of knowledge and understanding, but rather it may be a disconnect in applying that knowledge to the verdict, perhaps as a function of a tendency to over-believe information that they process auditorily, as suggested by Bond and DePaulo (2006). Expert testimony could, perhaps, focus on other issues such as the truthfulness bias, but

the data suggest that the jurors can grasp the relevant facts that affect eyewitness accuracy from the testimony without expert guidance.

Based on the findings of this study, written forms of testimony do elicit greater sensitivity, in terms of application of knowledge to verdict, than videotaped or, presumably, live testimony. As the judicial system uses live testimony and, if clarification is required, testimony is read back to the jury, research conducted with written testimony will likely not accurately capture the probably decisions of actual jurors in trial situations. Consistent with variations noted in previous research, artificial or reduced forms of testimony such as summaries may not be representative of testimony presented in real trials. Therefore, the responses of participants viewing such materials will not be indicative of the responses of real jurors hearing real testimony.

Written formats, in this study, resulted in better participant sensitivity and better application of testimony details to verdicts – ideally, therefore, jurors should read testimony. However, the real world judicial system is not going to eliminate live witness testimony in front of a jury and transition exclusively to written eyewitness depositions. According to an assistant District Attorney of Clark County, many attorneys, and probably judges as well, hold the opinion that auditory and visual cues are crucial to jury decision-making (B. Nelson, personal communication, September 2, 2017). Given that stance, one possible (although highly unlikely to be implemented) suggestion would be to have witnesses give testimony before the judge, attorneys, audience, and defendant, but with the jury recused. The testimony could be videotaped and transcribed, in order that the jurors might first read, and then, if necessary, view, the direct and cross-examination

testimony. This would increase the potential for jurors to identify relevant information and deception from the testimony, then subsequently allow them to assess demeanor, non-verbal cues, affect, and confidence via the videotaped testimony. A possible objection to this procedure could be that it would violate the defendant's sixth amendment rights. The sixth amendment of The Bill of Rights of the Constitution of the United States reads, in part, "In all criminal prosecutions, the accused shall enjoy the right…to be informed of the nature and cause of the accusation [and] to be confronted with the witnesses against him" (U.S. Const. amend. VI). However, nothing in this clause mandates that the jury must be physically present during the testimony; as long as the defendant is present during while the witness testifies, they have had the opportunity to face their accuser, and their sixth amendment protections have not been violated. Nonetheless, it is still highly improbable that the judicial system would adopt this procedure. Not only is it a tremendous departure from the way things have traditionally been done, it lessens the emotional impact of witness testimony as a tool of prosecution or defense, and there is a concern among officers of the court that reading material, rather than seeing or hearing it, can cause jurors to place too much weight on that information relative to other information presented during the trial (B. Nelson, personal communication, September 2, 2017).

Another, less extreme, possibility would be to have the jurors witness the trial testimony as usual, but then provide the jurors with a written transcript of the eyewitness testimony prior to deliberation, and to instruct them to read the entire testimony transcript before beginning deliberation. Currently, according to both the California Civil Jury

Instructions (CACI) 5011. “Reading Back of Trial Testimony in Jury Room” and the California Criminal Jury Instructions, Post-Trial: Introductory, Criminal Law 222. “Evidence,” upon written request the jurors may have the trial testimony read back to them verbally by the court reporter (“California Civil,” 2017; “Criminal Law,” 2017). However, in these statutes there is no currently no provision for having the material transcribed and given to the jurors to read (deaf jurors are allowed a sign language interpreter). As having the testimony read by the court reporter is an audio format, it is probable that the same disconnect between comprehension of testimony detail and application of that knowledge to the verdict, as seen in the audio-only format in this study, would pertain to this procedure. However, if the testimony were transcribed for the jurors and read by them prior to deliberation, their application of knowledge to verdict could potentially be increased. Although this would be a less disruptive way to introduce a written format of testimony into the judicial process, it is still unlikely to be adopted. In addition to the previous concerns, there is the issue of making the transcript available in a timely manner. Although most court reporters now use a digital medium to input court proceedings allowing simultaneous translation to standard English or any other desired language, rather than typing the information onto paper and later transcribing the symbols into language, court reporters typically demand the right to look the material over and check or edit it for accuracy before turning the transcript over to the courts to become part of the official record. If the court wished to have the jury read the trial testimony before deliberation, producing the transcript for the jury could potentially delay the jury’s deliberations for several days or a week while the court reporter readied

it for submission. Judges would most likely balk at any delay that costs the court time and, therefore, money and inconvenience (B. Nelson, personal communication, September 2, 2017).

Finally, judges and attorneys are unlikely to be persuaded by the data found in this study, as it appears to be contrary to a number of strongly held beliefs common to the judiciary about how jurors make decisions. They will be convinced that the only way the jurors can effectively receive and apply information properly is to see and hear the testimony as it is given. The accepted belief of many legal professionals is that it is harmful to the process to allow jurors to read testimony, reports, or exhibits submitted by experts, as they believe that having the jury read these materials will give them greater weight in the minds of the jury than is warranted (B. Nelson, personal communication, September 2, 2017).

Written format methodologies may or may not overestimate juror sensitivity to testimony detail, but certainly lack the visual and auditory cues available in live testimony. Those using written materials should note the possibility that participants may believe that written material is more likely to be deceptive, as seen in Bond and DePaulo (2006).

Limitations of Study

Confidence was not manipulated in this study, as sensitivity to eyewitness confidence, and its effect on verdicts and belief in accuracy have been well documented. Cutler and Penrod (1995), along with many others (e.g., Cutler et al., 1988; Cutler et al., 1989; Kennedy & Haygood, 1992; Bradfield & Wells, 2000) have found significant

correlations between witness confidence and guilty verdicts, although for decades witness confidence was not considered diagnostic of witness accuracy (Cutler & Penrod, 1995). This view regarding the weak relationship between confidence and accuracy has been revised through more recent analyses that show very clearly that confidence is a strong predictor of accuracy (Wixted et al. 2015). Wixted and Wells (2017) stipulated that this strong relationship holds at least for identifications made under pristine testing conditions, which should include "initial, uncontaminated memory tests using fair lineups, with no lineup administrator influence, and… an immediate confidence statement" (p. 10).

It is probable that experimental control of extraneous variables, in particular the attempt to keep witness demeanor, affect, and non-verbal cues consistent across video and audio testimony conditions, while varying only the facts of the testimony, made those factors non-diagnostic to judgments made about the testimony. This is a limitation shared with a majority of studies of this type; it is inherent in the principles of experimental control. However, in real trials, such information may actually be highly diagnostic of accuracy, as the non-verbal behaviors of witnesses whose conditions of observation were good may vary from those of witnesses whose conditions of observation were poor.

Because this was a scripted scenario, albeit from actual preliminary hearing testimony, using actors rather than real witnesses, no judgments of actual eyewitness accuracy were possible. The only bases for judging participant responses were differences in comprehension, recall, and judgments about one of two different versions

of testimony, presented in one of four formats. That is the nature of scenario studies; there is no ground truth to compare to judgments of accuracy.

The scenario used for this experiment was preliminary hearing testimony with direct and cross examination, but no additional trial elements. There was no jury selection process, opening statements, other witness or expert testimony, attorney objections, no defendant was visible, there was no audience presence or noise, exhibits entered into evidence, no opening or closing statements, judge's instructions, or jury deliberation. This lack of embedding information and post evidence conversation about the facts of the case makes it difficult to determine whether those embedding factors would hinder cognitive processing of information and reduce sensitivity, or whether deliberation could increase application of knowledge to verdicts.

The demographic was limited to university students, rather than a representative sample of jury eligible individuals, which could reflect range limitations in age, SES, education, occupation, background, and life experience. This limitation of jury research was noted in both Weiten and Diamond (1979) and Diamond (1997). Some research examined in these two critical reviews of jury simulation literature asserted that the decision-making behaviors of students differ from those of actual jurors, whereas other research focused on the premise that students are less authoritarian and conservative than the average jury-eligible adult population. Other researchers, such as Bornstein et al (2017), found little difference between student and more diverse jury-eligible populations in either knowledge or verdicts. The meta-analysis by Bornstein et al. found that differences between student and non-student populations were statistically non-

significant, except for a slightly greater tendency for students, versus non-students, to vote guilty when presented with written testimony.

Our participants tendered their verdicts, rated witness observation and accuracy, and subsequently answered open-ended recall questions immediately after exposure to the testimony. The short interval between testimony and response may have not given the participants an opportunity to have other information cause post-event interference with their memory of the information. Also, the question order in this study simulated a verdict-driven deliberation model and was not counterbalanced. The order of the questions may have influenced the pattern of responses in a particular way that cannot be determined without counterbalanced responses for comparison.

Consistent with the actual preliminary trial transcript used as the basis for the stimulus materials for this experiment, the witness was visibly non-Latino Caucasian (in the video format), visibly and audibly female (in the video/audio format) and identified in the written formats as such, and her name was ethnically neutral (Jennifer May Denauer), while the name and description of the male defendant (who was neither visible nor audible in the video/audio formats) made it evident that he was African-American (Deshaun Brown). There could have been some effect of the ethnic casting of the witness and defendant on the outcome of the study, as they effect of the relative ethnicities could be stronger in one format than another. Also, the witness in this experiment was also the victim of the crime, which might have generated greater sympathy or empathy for the witness.

Future Directions

The limitations of the study provide clear directions for future research. In order to examine issues of ground truth and the natural relationships between witnessing conditions and witness demeanor, future studies should use a staged crime methodology, with a strong manipulation of good versus poor viewing conditions (lighting, clear vs. obstructed view, length of encounter), resulting in accurate and inaccurate witness identifications. The testimony should then be presented in one of four stimulus material formats to participants acting as jurors, as was done in this study. Presumably, by using a staged crime to create real witnesses to a crime, witness accuracy, confidence, and demeanor will vary somewhat between viewing conditions and individual jurors, allowing a more comprehensive examination of participant sensitivity and ability to discern accurate versus inaccurate identifications.

It would be useful to embed that testimony in other trial elements, such as opening statements, other testimony, closing arguments, and judge's instructions to more accurately assess juror understanding, knowledge, and application to verdict vary across the various formats. Adding juror deliberation would allow an examination of whether deliberation can increase application of knowledge to verdict by means of a discussion or debate of the facts of the case with others. Additionally, using multiple samples, allowing a comparison of college students to jury eligible community members would be beneficial to see if the more diverse life experience and educational background of the larger pool of community members creates different results than a sample of undergraduate psychology college students. It would also be advisable to vary the

ethnicities of the witness and the defendant, perhaps having the defendant present in the courtroom. Creating a more complex, perhaps multi-day trial scenario, and building in more of a delay before verdict, ratings, and recall questions would allow greater realism in terms of the juror's cognitive processing of the testimony and memory for detail. It would also be highly desirable to counterbalance the order of the questions to simulate an evidence-based deliberation strategy of decision-making (observation rating, ID accuracy rating, verdict) as well as a verdict-driven strategy (verdict, observation rating, ID accuracy rating) to compare.

A study examining the previously proposed procedure (questioning witnesses in the regular manner, but with the jury absent, then allowing the jurors to read, then watch, the testimony) could be designed. It would then be possible to compare the juror judgments resulting from that procedure with the standard judicial procedure of live witness testimony before jurors, jurors receiving only videotaped testimony, and jurors receiving only transcribed testimony. This could serve to disambiguate some of the factors (e.g., over-belief, non-verbal cues, etc.) affect juror decision-making.

There are still issues to be parsed apart by future experimentation and research, such as what factors are driving the increased application of knowledge in the written format? Is there a truthfulness bias creating a trend to over-believe the identification in the video and audio formats? Is there a sense of social obligation to the witness-victim that is triggered in the video and audio formats? Is there a greater tendency to suspend disbelief, or suppress knowledge of the poor conditions of observation, in the video and audio formats?

Clearly, more research is needed to understand how jury research methodologies and simulations map onto actual juror decision-making. The results found in the current experiment could be, in part, an artifact of experimental control (i.e., how people conduct experimental research). However, despite the limitations mentioned and the need for future investigation, the current study has clarified the problem and provided evidence to suggest that participants, across formats, are not unaware of the factors in testimony that pertain to the witness observation, nor are they insensitive to how those factors affect the quality of the observation (i.e., whether the witness got a good look at the perpetrator during the crime, or did not). The issue of importance is not whether the participant understands the information presented and what that information means – they do – but whether to believe the assertion made by the witness that, regardless of the magnitude of the potential for error, the person they identified is the perpetrator of the crime they witnessed. That decision to believe the identification was accurate varied by format, and experimental methodologies may not be able to fully replicate and capture the nuances of the actual juror experience which occurs between knowing and understanding the facts and the choice to believe the identification and apply that belief to the verdict. The inescapable conclusion from this study is, based on these data, jurors are more sensitive to the details in eyewitness testimony pertaining to eyewitness accuracy than previously believed, and the difference seen in video compared to written formats is not one of comprehension or knowledge, but of applying that knowledge to the verdict. When it comes to effectively using the knowledge contained in testimony, jurors would do well to "skip the movie, and read the book!"

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Appendix A

Testimony Transcripts and Condensed Summaries

Full Testimony Transcript – Good Viewing Condition

SUPERIOR COURT OF CALIFORNIA
COUNTY OF RIVERSIDE

PEOPLE OF THE STATE OF CALIFORNIA (Plaintiff)
vs.
DESHAUN L. BROWN (Defendant),
CASE NO. RIF-084376

REPORTER'S PRELIMINARY HEARING TRANSCRIPT

Before the Honorable JOHN M. BARNHARDT, Judge
Department 24
May 22, 2006

APPEARANCES:

For the People:
OFFICE OF THE DISTRICT ATTORNEY
BY: Helen Bonner, Deputy

For the Defendant:
MUNICIPAL CONFLICTS PANEL
BY: Barnard S. Williams, esq.

Reported by:
MARY R. MILLER, C.S.R. No. 9942
Official Court Reporter
Riverside County Superior Court

FILED APR 7, 2006

PEOPLES WITNESSES:
JENNIFER MAY DENAUER
Direct Examination by Ms. Bonner
Cross-Examination by Mr. Williams

WEDNESDAY, APRIL 14, 1999; RIVERSIDE, CALIFORNIA
Before the HON. JOHN M. BARNHARDT, Judge, Dept. 64

THE COURT:
We are back on the record in the case of People versus Deshaun Brown. Is counsel ready?

MR. WILLIAMS:
Barnard Williams for Mr. Brown. We're ready.

MS. BONNER:
Helen Bonner for the People. We're ready.

THE COURT:
That's fine.

THE COURT:
The first witness will be?

MS. BONNER:
Jennifer Denauer.

THE COURT:
Okay. Are there any motions?

MS. BONNER:
None by the People.

MR. WILLIAMS:
None, Your Honor.

THE COURT:
All right. Counsel for the People, you may call your first witness.

MS. BONNER:
Thank you, Your Honor. People call Jennifer Denauer.

THE COURT:
Okay. Come forward, please.

THE CLERK:
Go ahead and come on up to the witness stand, please. Before taking your seat, if you'll raise your right hand to be sworn.

You do solemnly swear the testimony you are about to give shall be the truth, the whole truth, and nothing but the truth, so help you God?

THE WITNESS:
Yes.

THE CLERK:
Please be seated. Please state your name and spell your name for the record.

THE WITNESS:
Jennifer May Denauer. J-e-n-n-i-f-e-r, M-a-y, D-e-n-a-u-e-r.

THE COURT:
All right. Counsel, you may inquire.

MS. BONNER:
Thank you, Your Honor.

JENNIFER MAY DENAUER
Was called as a witness by and on behalf of the People, having been first duly sworn, was examined and testified as follows:

DIRECT EXAMINATION BY MS. BONNER:

MS. BONNER:
Miss Denauer on October 11, 1998, a little after 2 o'clock in the afternoon, were you in your car at Union Bank on Van Buren Avenue in the City of Riverside?

MS. DENAUER:
Yes.

MS. BONNER:
And that's in the County of Riverside too?

MS. DENAUER:
Yes.

MS. BONNER:
And what were you doing at that time?

MS. DENAUER:
I was pulling up to go through the drive-thru ATM.

MS. BONNER:
And you were the driver?

MS. DENAUER:
Yes.

MS. BONNER:
Was there a front passenger?

MS. DENAUER:
Yes.

MS. BONNER:
Who was that?

MS. DENAUER:
Bethany Fischer.

MS. BONNER:
And was there a rear passenger?

MS. DENAUER:
Yes.

MS. BONNER:
And who was that?

MS. DENAUER:
James Denauer.

MS. BONNER:
And that's your brother?

MS. DENAUER:
Yeah.

MS. BONNER:
Was he seated behind you or behind Bethany?

MS. DENAUER:
Behind Bethany.

MS. BONNER:
And when you pulled up to the ATM machine drive-thru, what did you do?

MS. DENAUER:
Um, I opened up the door to get out to put the card in.

MS. BONNER:
And what happened then?

MS. DENAUER:
There was a gun in the car.

MS. BONNER:
Okay. Where did you see this gun?

MS. DENAUER:
On Bethany's passenger side window.

MS. BONNER:
Was her window all the way up or down?

MS. DENAUER:
It was cracked (indicating).

MS. BONNER:
Okay. Do you know how far down it was?

MS. DENAUER:
About this far (indicating).

MS. BONNER:
That's about 7, 8 inches?

MR. WILLIAMS:
Yes.

THE COURT:
That will be fine.

MS. BONNER:
And you could clearly see the gun through the window?

MS. DENAUER:
Yeah.

MS. BONNER:
How far was it through the window?

MS. DENAUER:
Um, halfway.

MS. BONNER:
Could you see who was holding it?

MS. DENAUER:
Um, yes. You could see.

MS. BONNER:
Could you see the face of the person holding it?
MS. DENAUER:
Yes. The face.

MS. BONNER:
Could you tell what race that person was?

MS. DENAUER:
Yes.

MS. BONNER:
And what race was that person?

MS. DENAUER:
Black.

MS. BONNER:
Did the person have anything over their head or on their head?

MS. DENAUER:
No.

MS. BONNER:
So you could see his hair?

MS. DENAUER:
Yes, it was long, past his shoulders.

MS. BONNER:
Was there anything unusual about his hair?

MS. DENAUER:
Yes. It was that style, you know… dreadlocks.

MS. BONNER:
Now, when you looked and you saw this person with the gun halfway through the cracked window, did this person say anything to you?

MS. DENAUER:
Yes.

MS. BONNER:
And what did he say?

MS. DENAUER:
He said, "Give me the F’n money."

MS. BONNER:
And when he said that, what did you do?

MS. DENAUER:
Nothing. I was shocked.

MS. BONNER:
Were you scared?

MS. DENAUER:
Yeah.

MS. BONNER:
Did anyone else in the car at that time respond to this demand for money?

MS. DENAUER:
Yes. Bethany said she didn't have any money. And my brother handed money over the seat.

MR. WILLIAMS:
Objection... Hearsay what Bethany said.

THE COURT:
Sustained.

MR. WILLIAMS:
Move to strike.

THE COURT:
Motion granted.

MS. BONNER:
Okay. And your brother -- did he do something?

MS. DENAUER:
He handed money over from his wallet.

MS. BONNER:
To the man standing at the window?

MS. DENAUER:
Yeah.

MS. BONNER:
After he got the money from your brother, what did the man with the gun do?

MS. DENAUER:
He went to the front of the car, and stopped with his hand on the front of the car, pointing the gun at us, and then came to my side.

MS. BONNER:
When he got to your side, were you still seated in the driver's seat?

MS. DENAUER:
Yes.

MS. BONNER:
And was your door shut at that point?

MS. DENAUER:
-Yeah.

MS. BONNER:
When he got to your side, what did he do?

MS. DENAUER:
He told me to give him my money. And I said I didn't have any. But I threw my whole backpack with my purse in it.

MS. BONNER:
Now, was your window rolled all the way down because you were going to use the ATM machine?

MS. DENAUER:
-Yeah.

MS. BONNER:
Did he put the gun inside the car or he was standing with it outside?

MS. DENAUER:
He put it halfway in the car, towards my head.

MS. BONNER:
Now, you said that he took your backpack. What did your backpack look like?

MS. DENAUER:
It was black.

MS. BONNER:
How large was it, roughly?

MS. DENAUER:
Um, like the regular school bag, backpacks.

MS. BONNER:
Approximately 12 inches?

MS. DENAUER:
To carry books in.

MS. BONNER:
By about 18, roughly.

MS. DENAUER:
-- yeah.

MS. BONNER:
And do you know what it was made of?

MS. DENAUER:
Nylon, the -- yeah.

MS. BONNER:
Inside your backpack, what items did you have?

MS. DENAUER:
I had a purse in there.

MS. BONNER:
And what was it made of?

MS. DENAUER:
I had makeup, my checkbook.

MS. BONNER:
Let me stop you. What was the purse made of?

MS. DENAUER:
Oh. It was another smaller backpack purse type.

MS. BONNER:
Okay. Was it --

MS. DENAUER:
It was green.

MS. BONNER:
Was it also nylon? It was green. Was it --

MS. DENAUER:
It was vinyl.

MS. BONNER:
Vinyl. And the property that was inside that you're going to be telling me, was it inside the backpack loose or was it in the purse?

MS. DENAUER:
It was in the purse. And also there was loose stuff in there --

MS. BONNER:
Okay.

MS. DENAUER:
-- in the backpack.

MS. BONNER:
What did you have inside?

MS. DENAUER:
I had a curling iron, eyeglasses, lotion, my checkbook, my I.D.'s. I had a pepper spray. Um, makeup. Did I say makeup?

MS. BONNER:
Did you have your high school diploma?

MS. DENAUER:
Yeah.

MS. BONNER:
Did you have anything with addresses?

MS. DENAUER:
My driver's license, my -- also had a Florida driver's license.

MS. BONNER:
Did you have a book with other people's addresses?

MS. DENAUER:
Yeah, I did.

MS. BONNER:
And any keys?

MS. DENAUER:
Yes. My house keys.

MS. BONNER:
Now, you indicated, I believe you said, that you threw the backpack?

MS. DENAUER:
Yeah.

MS. BONNER:
What exactly did you do?

MS. DENAUER:
I threw it out the window at him.

MS. BONNER:
Okay. Did he catch it or did it fall?

MS. DENAUER:
Yeah. He grabbed it.

MS. BONNER:
Okay. As he grabbed it --

MS. DENAUER:
There was stuff falling out of it.

MS. BONNER:
It wasn't zipped up, then?

MS. DENAUER:
No.

MS. BONNER:
Okay. You could see stuff falling out of it?

MS. DENAUER:
Yes.

MS. BONNER:
At the time, could you see what was falling out of it?

MS. DENAUER:
Um, no.

MS. BONNER:
Okay. After you threw the backpack out and he caught it and things were falling out, what did he do?

MS. DENAUER:
He told us to get the F out of the car.

MS. BONNER:
And did you try to get out?

MS. DENAUER:
Yes.

MS. BONNER:
Did you try to get out the driver's door?

MS. DENAUER:
No.

MS. BONNER:
You tried to get out the passenger door?

MS. DENAUER:
Yes.

MS. BONNER:
What did you do?

MS. DENAUER:
I jumped over onto Bethany's lap.

MS. BONNER:
And did you then get out of the car?

MS. DENAUER:
Um, she was a little hesitant to get out.

MS. BONNER:
No. Did you get out?

MS. DENAUER:
Oh, yeah. We all got out.

MS. BONNER:
Did you get out first?

MS. DENAUER:
Yes.

MS. BONNER:
And James?

MS. DENAUER:
James got out.

MS. BONNER:
Did he get out after you?

MS. DENAUER:
Um, I think we probably mainly got out at the same time.

MS. BONNER:
Okay. After you all got out, what happened to the man with the gun?

MS. DENAUER:
Um, he took off.

MS. BONNER:
Did he ever attempt to get in the car?

MS. DENAUER:
Um, I'm not sure.

MS. BONNER:
Could you open your driver's door fully --

MS. DENAUER:
No.

MS. BONNER:
-- where your car was sitting at the ATM?

MS. DENAUER:
No.

MS. BONNER:
Why not?

MS. DENAUER:
There was a pole in the way.

MS. BONNER:
How far could you open the driver's door?

MS. DENAUER:
Probably just cracked

MS. BONNER:
Um --

MS. DENAUER:
-- About that much (indicating)

MS. BONNER:
About 9 inches?

MS. DENAUER:
Yeah. And the seat was pulled up. I sit close.

MS. BONNER:
Okay. And so it would be hard with the door not open for you to get out -- or him to get in -- because the back of the seat was kind of in the way too?

MS. DENAUER:
Mm-hmm.

MS. BONNER:
You have to answer out loud.

MS. DENAUER:
Yeah.

MR. WILLIAMS:
Objection, Your Honor. That's not relevant.

THE COURT:
I agree. Sustained.

MS. BONNER:
Did you get a good look at the man who was holding the gun on you?

MS. DENAUER:
Yes, I saw his face.

MS. BONNER:
So you saw his face pretty well?

MS. DENAUER:
Yeah, but I was really afraid because I didn't want him to shoot in case -- you know -- witnesses.

MS. BONNER:
So how would you say you felt?

MS. DENAUER:
Scared.

MS. BONNER:
After he left -- strike that.
After you all got out, you testified that he left?

MS. DENAUER:
Yes.

MS. BONNER:
Was he walking?

MS. DENAUER:
No. He ran.

MS. BONNER:
Did any of you follow him?

MS. DENAUER:
Bethany did.

MS. BONNER:
She started chasing him?

MS. DENAUER:
Yes.

MS. BONNER:
You -- did you get back into the car to try and find a police officer?

MS. DENAUER:
Yes.

MS. BONNER:
All of you?

MS. DENAUER:
Um, yes.

MS. BONNER:
And how soon until you found the police officer?

MS. DENAUER:
Like 2 minutes.

MS. BONNER:
As you were getting in the car, did you locate any items that had fallen out when you first threw your backpack?

MS. DENAUER:
Yes.

MS. BONNER:
And what items were those?

MS. DENAUER:
My eyeglasses and my curling iron.

MS. BONNER:
Later, were you shown a lineup? That is, were you asked to look pictures and say, "Is this the person who robbed you?"

MS. DENAUER:
Yes

MS. BONNER:
And a Riverside police officer wearing a blue uniform was present when you did that?

MS. DENAUER:
When they came to my house?

MS. BONNER:
When you looked at the lineup, was there an RPD officer there?

MS. DENAUER:
Yes.

MS. BONNER:
And one of the pictures that was shown to you, was that the person who robbed you?

MS. DENAUER:
Yes.

MS. BONNER:
Do you see anyone in court this afternoon that you recognize?

MS. DENAUER:
Yes.

MS. BONNER:
And who do you recognize?

MS. DENAUER:
(Indicating.)

MS. BONNER:
You have to explain where the person is seated.

MS. DENAUER:
Right over there (indicating) in the orange suit.

MS. BONNER:
If the record could reflect that she's identified the defendant, Mr. Brown?

THE COURT: Yes.

MS. BONNER:
How – do you recognize him as looking the same as he did that afternoon?

MS. DENAUER:
No.

MS. BONNER:
How does he look different?

MS. DENAUER:
Well, his hair was longer. But I recognize him.

MS. BONNER:
So you saw his hair that afternoon?

MS. DENAUER:
Yes.

MS. BONNER:
Okay. And so you recognize him. So it's the person that demanded the money from you who had a gun?

MS. DENAUER:
Yes.

MS. BONNER:
I have no further questions.

THE COURT:
Mr. Williams?

CROSS-EXAMINATION BY MR. WILLIAMS:

MR. WILLIAMS:
Now, when you saw the man at the teller -- the ATM, how close -- this was at 2 o'clock in the afternoon; is that right?

MS. DENAUER:
Yes.

MR. WILLIAMS:
Was it pretty well lit there?

MS. DENAUER:
Yes, daylight.

MR. WILLIAMS:
Okay. And you saw this person's face?

MS. DENAUER:
Yes.

MR. WILLIAMS:
How close were you when you saw this person's face?

MS. DENAUER:
I was sitting in the driver's seat of the car, and he had his -- the gun and his head looking into the window in the passenger side.

MR. WILLIAMS:
So he was across the car looking into the window? And how long was he there?

MS. DENAUER:
I'd say for a minute or two.

MR. WILLIAMS:
I have no further questions.

THE COURT:
Anything else?

MS. BONNER:
No.

THE COURT:
Thank you very much. You may step down.

Full Testimony Transcript – Poor Viewing Condition

SUPERIOR COURT OF CALIFORNIA
COUNTY OF RIVERSIDE

PEOPLE OF THE STATE OF CALIFORNIA (Plaintiff)
vs.
DESHAUN L. BROWN (Defendant),
CASE NO. RIF-084376

REPORTER'S PRELIMINARY HEARING TRANSCRIPT

Before the Honorable JOHN M. BARNHARDT, Judge
Department 24
May 22, 2006

APPEARANCES:

For the People:
OFFICE OF THE DISTRICT ATTORNEY
BY: Helen Bonner, Deputy

For the Defendant:
MUNICIPAL CONFLICTS PANEL
BY: Barnard S. Williams, esq.

Reported by:
MARY R. MILLER, C.S.R. No. 9942
Official Court Reporter
Riverside County Superior Court

FILED APR 7, 2006

PEOPLES WITNESSES:
JENNIFER MAY DENAUER
Direct Examination by Ms. Bonner
Cross-Examination by Mr. Williams

WEDNESDAY, APRIL 14, 1999; RIVERSIDE, CALIFORNIA
Before the HON. JOHN M. BARNHARDT, Judge, Dept. 64

THE COURT:
We are back on the record in the case of People versus Deshaun Brown. Is counsel ready?

MR. WILLIAMS:
Barnard Williams for Mr. Brown. We're ready.

MS. BONNER:
Helen Bonner for the People. We're ready.

THE COURT:
That's fine.

THE COURT:
The first witness will be?

MS. BONNER:
Jennifer Denauer.

THE COURT:
Okay. Are there any motions?

MS. BONNER:
None by the People.

MR. WILLIAMS:
None, Your Honor.

THE COURT:
All right. Counsel for the People, you may call your first witness.

MS. BONNER:
Thank you, Your Honor. People call Jennifer Denauer.

THE COURT:
Okay. Come forward, please.

THE CLERK:
Go ahead and come on up to the witness stand, please. Before taking your seat, if you'll raise your right hand to be sworn.

You do solemnly swear the testimony you are about to give shall be the truth, the whole truth, and nothing but the truth, so help you God?

THE WITNESS:
Yes.

THE CLERK:
Please be seated. Please state your name and spell your name for the record.

THE WITNESS:
Jennifer May Denauer. J-e-n-n-i-f-e-r, M-a-y, D-e-n-a-u-e-r

THE COURT:
All right. Counsel, you may inquire.

MS. BONNER:
Thank you, Your Honor.

JENNIFER MAY DENAUER
Was called as a witness by and on behalf of the People, having been first duly sworn, was examined and testified as follows:

DIRECT EXAMINATION BY MS. BONNER:

MS. BONNER:
Miss Denauer on October 11, 1998, a little after 11 o'clock at night, were you in your car at Union Bank on Van Buren Avenue in the City of Riverside?

MS. DENAUER:
Yes.

MS. BONNER:
And that's in the County of Riverside too?

MS. DENAUER:
Yes.

MS. BONNER:
And what were you doing at that time?

MS. DENAUER:
I was pulling up to go through the drive-thru ATM.

MS. BONNER:
And you were the driver?

MS. DENAUER:
Yes.

MS. BONNER:
Was there a front passenger?

MS. DENAUER:
Yes.

MS. BONNER:
Who was that?

MS. DENAUER:
Bethany Fischer.

MS. BONNER:
And was there a rear passenger?

MS. DENAUER:
Yes.

MS. BONNER:
And who was that?

MS. DENAUER:
James Denauer.

MS. BONNER:
And that's your brother?

MS. DENAUER:
Yeah.

MS. BONNER:
Was he seated behind you or behind Bethany?

MS. DENAUER:
Behind Bethany.

MS. BONNER:
And when you pulled up to the ATM machine drive-thru, what did you do?

MS. DENAUER:
Um, I opened up the door to get out to put the card in.

MS. BONNER:
And what happened then?

MS. DENAUER:
There was a gun in the car

MS. BONNER:
Okay. Where did you see this gun?

MS. DENAUER:
On Bethany's passenger side window.

MS. BONNER:
Was her window all the way up or down?

MS. DENAUER:
It was cracked (indicating).

MS. BONNER:
Okay. Do you know how far down it was?

MS. DENAUER:
About this far (indicating).

MS. BONNER:
That's about 7, 8 inches?

MR. WILLIAMS:
Yes.

THE COURT:
That will be fine.

MS. BONNER:
And you could clearly see the gun through the window?

MS. DENAUER:
Yeah.

MS. BONNER:
How far was it through the window?

MS. DENAUER:
Um, halfway.

MS. BONNER:
Could you see who was holding it?

MS. DENAUER:
Um, yes. You could see.

MS. BONNER:
Could you see the face of the person holding it?

MS. DENAUER:
Some. The eyes.

MS. BONNER:
Could you tell what race that person was?

MS. DENAUER:
Yes.

MS. BONNER:
And what race was that person?

MS. DENAUER:
Black.

MS. BONNER:
Did the person have anything over their head or on their head?

MS. DENAUER:
Yes.

MS. BONNER:
What?

MS. DENAUER:
A hood.

MS. BONNER:
Do you know what color it was?

MS. DENAUER:
It was either navy blue or black.

MS. BONNER:
Now, when you looked and you saw this person with the gun halfway through the cracked window, did this person say anything to you?

MS. DENAUER:
Yes.

MS. BONNER:
And what did he say?

MS. DENAUER:
He said, "Give me the F’n money."

MS. BONNER:
And when he said that, what did you do?

MS. DENAUER:
Nothing. I was shocked.

MS. BONNER:
Were you scared?

MS. DENAUER:
Yeah.

MS. BONNER:
Did anyone else in the car at that time respond to this demand for money?

MS. DENAUER:
Yes. Bethany said she didn't have any money. And my brother handed money over the seat.

MR. WILLIAMS:
Objection... Hearsay what Bethany said.

THE COURT:
Sustained.

MR. WILLIAMS:
Move to strike.

THE COURT:
Motion granted.

MS. BONNER:
Okay. And your brother -- did he do something?

MS. DENAUER:
He handed money over from his wallet.

MS. BONNER:
To the man standing at the window?

MS. DENAUER:
Yeah.

MS. BONNER:
After he got the money from your brother, what did the man with the gun do?

MS. DENAUER:
He went quickly around the front of the car with his hand on the front of the car, pointing the gun at us, and came to my side.

MS. BONNER:
When he got to your side, were you still seated in the driver's seat?

MS. DENAUER:
Yes.

MS. BONNER:
And was your door shut at that point?

MS. DENAUER:
-Yeah.

MS. BONNER:
When he got to your side, what did he do?

MS. DENAUER:
He told me to give him my money. And I said I didn't have any. But I threw my whole backpack with my purse in it.

MS. BONNER:
Now, was your window rolled all the way down because you were going to use the ATM machine?

MS. DENAUER:
-Yeah.

MS. BONNER:
Did he put the gun inside the car or he was standing with it outside?

MS. DENAUER:
He put it halfway in the car, towards my head.

MS. BONNER:
Now, you said that he took your backpack. What did your backpack look like?

MS. DENAUER:
It was black.

MS. BONNER:
How large was it, roughly?

MS. DENAUER:
Um, like the regular school bag, backpacks

MS. BONNER:
Approximately 12 inches?

MS. DENAUER:
To carry books in.

MS. BONNER:
By about 18, roughly.

MS. DENAUER:
-- yeah.

MS. BONNER:
And do you know what it was made of?

MS. DENAUER:
Nylon, the -- yeah.

MS. BONNER:
Inside your backpack, what items did you have?

MS. DENAUER:
I had a purse in there.

MS. BONNER:
And what was it made of?

MS. DENAUER:
I had makeup, my checkbook.

MS. BONNER:
Let me stop you. What was the purse made of?

MS. DENAUER:
Oh. It was another smaller backpack purse type.

MS. BONNER:
Okay. Was it --

MS. DENAUER:
It was green.

MS. BONNER:
Was it also nylon? It was green. Was it --

MS. DENAUER:
It was vinyl.

MS. BONNER:
Vinyl. And the property that was inside that you're going to be telling me, was it inside the backpack loose or was it in the purse?

MS. DENAUER:
It was in the purse. And also there was loose stuff in there --

MS. BONNER:
Okay.

MS. DENAUER:
-- in the backpack.

MS. BONNER:
What did you have inside?

MS. DENAUER:
I had a curling iron, eyeglasses, lotion, my checkbook, my I.D.'s. I had a pepper spray. Um, makeup. Did I say makeup?

MS. BONNER:
Did you have your high school diploma?

MS. DENAUER:
Yeah.

MS. BONNER:
Did you have anything with addresses?

MS. DENAUER:
My driver's license, my -- also had a Florida driver's license.

MS. BONNER:
Did you have a book with other people's addresses?

MS. DENAUER:
Yeah, I did.

MS. BONNER:
And any keys?

MS. DENAUER:
Yes. My house keys.

MS. BONNER:
Now, you indicated, I believe you said, that you threw the backpack?

MS. DENAUER:
Yeah.

MS. BONNER:
What exactly did you do?

MS. DENAUER:
I threw it out the window at him.

MS. BONNER:
Okay. Did he catch it or did it fall?

MS. DENAUER:
Yeah. He grabbed it.

MS. BONNER:
Okay. As he grabbed it --

MS. DENAUER:
There was stuff falling out of it.

MS. BONNER:
It wasn't zipped up, then?

MS. DENAUER:
No.

MS. BONNER:
Okay. You could see stuff falling out of it?

MS. DENAUER:
Yes.

MS. BONNER:
At the time, could you see what was falling out of it?

MS. DENAUER:
Um, no.

MS. BONNER:
Okay. After you threw the backpack out and he caught it and things were falling out, what did he do?

MS. DENAUER:
He told us to get the F out of the car.

MS. BONNER:
And did you try to get out?

MS. DENAUER:
Yes.

MS. BONNER:
Did you try to get out the driver's door?

MS. DENAUER:
No.

MS. BONNER:
You tried to get out the passenger door?

MS. DENAUER:
Yes.

MS. BONNER:
What did you do?

MS. DENAUER:
I jumped over onto Bethany's lap.

MS. BONNER:
And did you then get out of the car?

MS. DENAUER:
Um, she was a little hesitant to get out.

MS. BONNER:
No. Did you get out?

MS. DENAUER:
Oh, yeah. We all got out.

MS. BONNER:
Did you get out first?

MS. DENAUER:
Yes.

MS. BONNER:
And James?

MS. DENAUER:
James got out.

MS. BONNER:
Did he get out after you?

MS. DENAUER:
Um, I think we probably mainly got out at the same time.

MS. BONNER:
Okay. After you all got out, what happened to the man with the gun?

MS. DENAUER:
Um, he took off.

MS. BONNER:
Did he ever attempt to get in the car?

MS. DENAUER:
Um, I'm not sure.

MS. BONNER:
Could you open your driver's door fully --

MS. DENAUER:
No.

MS. BONNER:
-- where your car was sitting at the ATM?

MS. DENAUER:
No.

MS. BONNER:
Why not?

MS. DENAUER:
There was a pole in the way.

MS. BONNER:
How far could you open the driver's door?

MS. DENAUER:
Probably just cracked

MS. BONNER:
Um --

MS. DENAUER:
-- About that much (indicating)

MS. BONNER:
About 9 inches?

MS. DENAUER:
Yeah. And the seat was pulled up. I sit close.

MS. BONNER:
Okay. And so it would be hard with the door not open for you to get out -- or him to get in -- because the back of the seat was kind of in the way too?

MS. DENAUER:
Mm-hmm.

MS. BONNER:
You have to answer out loud.

MS. DENAUER:
Yeah.

MR. WILLIAMS:
Objection, Your Honor. That's not relevant.

THE COURT:
I agree. Sustained.

MS. BONNER:
Did you get a good look at the man who was holding the gun on you?

MS. DENAUER:
Just his eyes.

MS. BONNER:
So you really didn't get to see his face too well?

MS. DENAUER:
No, I was really afraid because I didn't want him to shoot in case -- you know -- witnesses.

MS. BONNER:
So how would you say you felt?

MS. DENAUER:
Scared.

MS. BONNER:
After he left -- strike that.
After you all got out, you testified that he left?

MS. DENAUER:
Yes.

MS. BONNER:
Was he walking?

MS. DENAUER:
No. He ran.

MS. BONNER:
Did any of you follow him?

MS. DENAUER:
Bethany did.

MS. BONNER:
She started chasing him?

MS. DENAUER:
Yes.

MS. BONNER:
You -- did you get back into the car to try and find a police officer?

MS. DENAUER:
Yes.

MS. BONNER:
All of you?

MS. DENAUER:
Um, yes.

MS. BONNER:
And how soon until you found the police officer?

MS. DENAUER:
Like 2 minutes.

MS. BONNER:
As you were getting in the car, did you locate any items that had fallen out when you first threw your backpack?

MS. DENAUER:
Yes.

MS. BONNER:
And what items were those?

MS. DENAUER:
My eyeglasses and my curling iron.

MS. BONNER:
Later, were you shown a lineup? That is, were you asked to look pictures and say, "Is this the person who robbed you?"

MS. DENAUER:
Yes

MS. BONNER:
And a Riverside police officer wearing a blue uniform was present when you did that?

MS. DENAUER:
When they came to my house?

MS. BONNER:
When you looked at the lineup, was there an RPD officer there?

MS. DENAUER:
Yes.

MS. BONNER:
And one of the pictures that was shown to you, was that the person who robbed you?

MS. DENAUER:
Yes.

MS. BONNER:
Do you see anyone in court this afternoon that you recognize?

MS. DENAUER:
Yes.

MS. BONNER:
And who do you recognize?

MS. DENAUER:
(Indicating.)

MS. BONNER:
You have to explain where the person is seated.

MS. DENAUER:
Right over there (indicating) in the orange suit.

MS. BONNER:
If the record could reflect that she's identified the defendant, Mr. Brown?

THE COURT: Yes.

MS. BONNER:
How – do you recognize him as looking the same as he did that night?

MS. DENAUER:
No.

MS. BONNER:
How does he look different?

MS. DENAUER:
Well, he was wearing a hood. But it – I recognize his eyes.

MS. BONNER:
So you never saw his hair that night?

MS. DENAUER:
No.

MS. BONNER:
Okay. And so you recognize his eyes. So it's the person that demanded the money from you who had a gun?

MS. DENAUER:
Yes.

MS. BONNER:
I have no further questions.

THE COURT:
Mr. Williams?

CROSS-EXAMINATION BY MR. WILLIAMS:

MR. WILLIAMS:
Now, when you saw the man at the teller -- the ATM, how close -- this was at 11 o'clock at night; is that right?

MS. DENAUER:
Yes

MR. WILLIAMS:
Was it pretty well lit there?

MS. DENAUER:
No.

MR. WILLIAMS:
Okay. And you saw this person's face?

MS. DENAUER:
Only the eyes.

MR. WILLIAMS:
How close were you when you saw this person's eyes?

MS. DENAUER:
I was sitting in the driver's seat of the car, and he had his -- the gun and his head looking into the window in the passenger side.

MR. WILLIAMS:
So he was across the car looking into the window? And how long was he there?

MS. DENAUER:
I'd say for less than a minute.

MR. WILLIAMS:
I have no further questions.

THE COURT:
Anything else?

MS. BONNER:
No.

THE COURT:
Thank you very much. You may step down.

Summarized Testimony Transcript – Good Viewing Condition

SUPERIOR COURT OF CALIFORNIA, COUNTY OF RIVERSIDE

THE CASE OF PEOPLE OF THE STATE OF CALIFORNIA (Plaintiff) vs. ***DESHAUN L. BROWN (Defendant)****, CASE NO. RIF-084376, before the Honorable JOHN M. BARNHARDT, Judge, Department 24, May 22, 2006.*

For the People: Helen Bonner*, Deputy.* ***For the Defendant: Barnard S. Williams, esq.*** *Reported by: MARY R. MILLER, C.S.R. No. 9942, Official Court Reporter, Riverside County Superior Court*

PEOPLES WITNESS: JENNIFER MAY DENAUER

During direct examination by prosecuting attorney Bonner, Ms. Denauer stated the following: I pulled up in my car to the ATM at Union Bank, Van Buren Blvd., Riverside, at 2pm in the afternoon on October 11, 1998. A gun was pointed at me through the passenger window. I could see the face of the man holding the gun. He was black, with long hair in dreadlocks. He told me to give him the F'n money. He walked to the front of the car and stopped with his free hand on the hood of the car, pointing the gun at me, then came around to my side of the car. He told me to give him my money. I said I didn't have any, but I threw my whole backpack with my purse in it at him. He told us to get out of the car, and when we got out, he took off. I got a good look at his face. Later, I was shown a lineup and asked, "Is one of these people the person that robbed you?" I picked one of the pictures out as the man who had robbed me. I see him sitting in the courtroom, there (pointing to Deshaun Brown). He is the man who robbed me.

During cross-examination by defense attorney Williams, Ms. Denauer stated the following: I saw the man at the ATM at 2pm in the afternoon. The area was well lit, as it was daylight. I first saw his face across the width of the car, as he was looking in the passenger window, where he remained for a minute or two.

Summarized Testimony Transcript – Poor Viewing Condition

SUPERIOR COURT OF CALIFORNIA, COUNTY OF RIVERSIDE

THE CASE OF PEOPLE OF THE STATE OF CALIFORNIA (Plaintiff) vs. ***DESHAUN L. BROWN (Defendant)****, CASE NO. RIF-084376, before the Honorable JOHN M. BARNHARDT, Judge, Department 24, May 22, 2006.*

For the People: Helen Bonner*, Deputy.* ***For the Defendant: Barnard S. Williams, esq.*** *Reported by: MARY R. MILLER, C.S.R. No. 9942, Official Court Reporter, Riverside County Superior Court*

PEOPLES WITNESS: JENNIFER MAY DENAUER

During direct examination by prosecuting attorney Bonner, Ms. Denauer stated the following: I pulled up in my car to the ATM at Union Bank, Van Buren Blvd., Riverside, at 11pm at night on October 11, 1998. A gun was pointed at me through the passenger window. I could see only the eyes of the man holding the gun. He was black, and wearing a blue or black hoody. He told me to give him the F'n money. He walked quickly around the front of the car, pointing the gun at me, then came around to my side of the car. He told me to give him my money. I said I didn't have any, but I threw my whole backpack with my purse in it at him. He told us to get out of the car, and when we got out, he took off. I could not really see his face, only his eyes. Later, I was shown a lineup and asked, "Is one of these people the person that robbed you?" I picked one of the pictures out as the man who had robbed me. I see him sitting in the courtroom, there (pointing to Deshaun Brown). He is the man who robbed me.

During cross-examination by defense attorney Williams, Ms. Denauer stated the following: I saw the man at the ATM at 11pm at night. The area was very poorly lit, so it was very dark. I first saw just his eyes across the width of the car, as he was looking in the passenger window, where he remained for less than a minute.

Appendix B

Experimental Questions

"Please circle ONE, and only ONE, answer. Do not go back and look at a previous page."

Page 1.

Do you think Deshaun Brown, the defendant, is: Guilty Not Guilty

Page 2.

How confident do you feel about your verdict of guilty or not guilty?

Unsure Sure

1 2 3 4 5 6

Page 3.

How good of a look did the witness get at the perpetrator (robber)?

Extremely poor look

Very poor look

Somewhat poor look

Somewhat good look

Very good look

Extremely good look

Page 4.

Do you think the witness correctly identified the perpetrator (robber)?

(Did she identify the bad guy?)

Definitely Yes

Probably Yes

Maybe Yes

Maybe No

Probably No

Definitely No

Page 5.

What time of day did the crime occur, approximately? _____

Page 6.

What was the lighting like at the time of the crime? _____

Page 7.

What was the witness able to see of the perpetrator (robber)?_____

Page 8.

Was there a weapon? If so, what kind? _____

Page 9.

Was the perpetrator (robber) wearing a disguise? If so, what kind? _____

Page 10. Demographics: Age, Sex, Ethnic Background, First Language, English Proficiency, Have you been the victim of a crime? What kind?, Have you served on a jury? What kind?

Appendix C

Proportion of Guilty Verdicts, by Viewing Condition and Testimony Presentation Condition

No Participants Excluded

	Good	Poor	$\chi^2_{(1)}$	*p*	*r*	*N*
Video	.697	.520	4.99	.013	.182	151
Audio	.792	.534	11.22	.001	.274	150
Transcript	.722	.233	34.80	<.001	.490	145
Summary	.622	.270	22.83	<.001	.393	148

Note: All *p*-values are one-tailed.