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Sara Marie Locatelli  
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LOYOLA UNIVERSITY CHICAGO

PERSONAL BELIEFS AND PUBLIC PRINT:  
THE INFLUENCE OF PRE-EXISTING ATTITUDES AND  
PRETRIAL PUBLICITY INFORMATION ON FINAL VERDICTS

A DISSERTATION SUBMITTED TO  
THE FACULTY OF THE GRADUATE SCHOOL  
IN CANDIDACY FOR THE DEGREE OF  
DOCTOR OF PHILOSOPHY

PROGRAM IN SOCIAL PSYCHOLOGY

BY

SARA MARIE LOCATELLI

CHICAGO, IL

AUGUST 2011

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## ABSTRACT

Pretrial publicity (PTP), defined as any news story about a case not yet in trial, has been shown to affect trial outcomes. Results, however, are mixed, with studies finding strong effects, others weaker effects, and some no effects. These differences are sometimes attributed to methodology and study stimuli. In the present research, the effect of participant attitudes was explored. Participant attitudes can have a strong influence on perceptions about a piece of information, and could explain differences in use of PTP as well as findings that judicial remedies to alleviate PTP effects are ineffective. Participants were exposed to one of four news articles, containing information on confession, prior record, or resisting arrest, or only neutral information. They took a measure of legal attitudes, including questions relevant to the negative PTP information received, and then read a trial transcript of an assault on a middle-aged man. Participants then received standard jury instructions, a specific admonition to ignore PTP, or a specific admonition with the additional statement that PTP can be unreliable.

Results differed by content of PTP. Participants receiving confession PTP assigned higher guilt ratings overall; strong instructions increased guilt ratings and guilty verdicts among participants receiving confession PTP. Participants receiving resisting arrest PTP and specific instructions assigned higher guilt ratings than participants receiving neutral PTP and specific instructions. Finally, among participants receiving prior record PTP, a weak attitude toward a prior record as indicative of guilt and standard

instructions resulted in lower guilt ratings than participants receiving neutral PTP; the effects were reversed for participants with a strong attitude that having a prior record indicates guilt. Stronger instructions weakened the effects of attitude and PTP. Results suggest that instructions specifically addressing PTP information may only ameliorate PTP effects in certain situations, and that psychological reactance, thought suppression and ironic processes provide better explanations for use of PTP, despite instructions to discourage use, than belief perseverance. Additionally, the effects of other attitudes specific to the case and the relationship between guilt ratings and verdicts were explored. Future research should continue to explore relationship among attitudes, evidence, PTP, and outcomes.

## CHAPTER ONE

### INTRODUCTION

The study of social psychology began as a study of attitudes (McGuire, 1989). Though this subfield has expanded to include a variety of important research topics, including the ways in which psychological processes can influence legal cases, attitudes remain an important aspect of many social psychological inquiries (Ottati, Edwards, & Krundick, 2005). The present project seeks to expand on past research, by examining the effect of attitudes on processing of legal information; reactions to and influences of pretrial publicity may depend in part on an individual's attitudes about the legal information contained in the media. These attributes can influence an individual's behavior in this legal context, the most important behavior being the individual's verdict preference. These issues have never been studied empirically, so the current research project will study these variables.

Though the eventual verdict from a deliberating jury is the only aspect that concerns the legal system, social psychological research has shown that individual verdict preferences can have a strong influence on eventual verdict, even if not all jurors are in total agreement at the beginning of deliberation. Kalven and Zeisel (1966) for instance found that the majority verdict at the beginning of deliberation was the final verdict in over 90% of trials, a finding that has been confirmed by both experimental and field studies (for a review, see Devine, Clayton, Dunford, Seying, & Pryce, 2001). Where

individual jurors start out is a good indication of where the entire jury will end. Any information that may bias jurors individually before they step into the jury room, then, is of great concern.

One issue that can bias jurors before they enter the jury room, or even the courtroom, is pretrial publicity (PTP). One of the first instances of PTP was the Aaron Burr treason trial in 1807 (*United States v. Burr*, 1807). Burr argued that an impartial jury could not be seated due to the prejudicial stories published by a local newspaper. This was the first time the court had dealt with these issues, and with no research or precedent to guide them, determined that knowledge of a case does not exempt a potential juror. It must be shown that the juror is biased against the defendant and that this bias would prevent the juror from being able to decide the case based on the evidence. Though PTP has come before the court many times since then, the “actual bias” ruling has rarely changed (Curtner & Kassier, 2005).

The issue of PTP is in the middle of a tug-of-war between two Constitutional rights: the right to a free press and the right of defendants to a fair trial – which is why PTP is often referred to as the free press-fair trial debate. Neither of these rights is qualified in any way, though some (e.g. Chesterman, 1997) have argued that the First Amendment is given greater weight than the Sixth; there is nothing in the Constitution stating that the freedom of the press can be suspended in certain cases, leading many to argue that censorship of the press is unconstitutional, regardless of the motive. Therefore, those involved with the legal system are called to maintain a defendant’s 6th

amendment rights despite the freedom of the press to print information about the defendant.

Once a jury has been seated, they are asked by the court to avoid any press coverage of the trial, and judges can make it more difficult for jurors to obtain information from the media during the trial by using such options as sequestering the jury or gag orders. But jurors do not enter the courtroom as blank slates; if the biasing information has already been acquired by an individual before he or she becomes a juror, these options are not likely to have any noticeable effect. The court has dealt with this issue in a variety of ways. One way is with judicial remedies (Kramer, Kerr, & Carroll, 1990). The court may delay a trial to allow the publicity to subside, which is referred to as a continuance. The danger, however, is that the media will simply resume coverage once the trial begins. Further, certain types of publicity, such as emotionally arousing information, may be less easily forgotten than more factual PTP (Honesty, Charman, & Levi, 2003; Kramer, Kerr, & Carroll, 1990).

Once the jury pool (known as the *venire*) has been drawn, prospective jurors are asked various questions, including questions about their knowledge of and beliefs about a case, which is called *voir dire*. Often, *voir dire* is done in groups, in which the judge asks general questions, such as, "Has anyone heard anything about this case?" and jurors respond with yes or no (Hans & Dee, 1991). When the pretrial publicity associated with a case is great, the court can elect to question each juror individually and even separately from the rest of the jury pool. Obviously, this procedure is more time-consuming, and would not be part of the regular questioning procedure. The court may also instruct



jurors to ignore any pretrial information they have obtained when deciding a case, referred to as a judicial admonition (Thompson & Fuqua, 1998). Many researchers and theorists question whether jurors are able to ignore such information (Diamond & Vidmar, 2001).

If PTP is so great that it is reasonable to assume an unbiased jury cannot be seated, the court may draw a jury pool from another community – referred to as a “change of *venire*” – and bring them to the jurisdiction in which the crime took place (Studebaker & Penrod, 2005). Another option is to move the trial to another location and carry out jury selection there, which is called a “change of venue.” This means that, rather than moving the jurors, the judge, attorneys, and defendant must go to the different jurisdiction instead. Either of these options is expensive and, therefore, rarely used. The judge may also be hesitant to move a trial because of strong community reactions to the crime and a desire to try a case in the community (Moran & Cutler, 1991). For example, Finkel (1995) discusses the Rodney King trial, which was moved to another venue, and the police officers accused of assaulting King were acquitted; this relocation led to riots and vocal critics, who believed that moving the trial to a predominantly White community had been a miscarriage of justice. Well-known cases like this one can influence other judges’ willingness to grant change of venue motions because of fear of community reaction (Finkel, 1995).

The research examining continuance, *voir dire*, and judicial admonitions have not always found strong conclusions, though Studebaker and Penrod (1997) argue that our lack of complete understanding of PTP effects may make it difficult to uncover remedies

that will actually have an effect. If the remedies do not work, the final option is to overturn a verdict. The court has only exercised this option a few times (e.g., *Irvin v. Dowd*, 1961; *Rideau v. Louisiana*, 1963; *Shepherd v. Maxwell*, 1966). In such cases, the amount of PTP has been extreme and the information highly inflammatory. Some research suggests, however, that PTP does not need to reach the level seen in these cases to be biasing to jurors.

Legal psychologists have examined the issue of PTP from many angles. Two of the most important aspects of this research are 1) how to determine whether a prospective juror has been biased so that he or she may be excused from service and 2) how to use remedies effectively so that, if any biased jurors were identified, the right to a fair trial by an impartial jury may be ensured. The goal of this project is to examine these two issues, by comparing individuals receiving biasing information in PTP to individuals receiving control information, and by comparing levels of judicial admonishment. In addition, the present research will measure and integrate participants' attitudes toward certain pieces of legal information to determine if and how these attitudes affect verdicts and effectiveness of admonitions. Attitudes have been examined before in the PTP research (e.g. *Arbuthnot, Myers, & Leach*, 2002; *Butler*, 2007; *Chrzanowski*, 2006; *Constantini & King*, 1980/1981; *De Luca*, 1979; *Kovera*, 2002), with often mixed results, but the present research will expand on prior attitude research by measuring the effect of specific attitudes instead of general attitude toward crime, which, according to theory on attitude-behavior correspondence (*Ajzen & Sexton*, 1999; *Ronis, Yates, & Kirscht*, 1989), should have a stronger relationship to verdict preferences.

## CHAPTER TWO

### LITERATURE REVIEW

Before discussing the present research, it is necessary to examine what has been found in the past. First, the findings of research on PTP will be summarized and discussed, including research that has examined the use of one or more remedies, along with commentary on the methodology utilized in these studies. Next, research and theory on attitudes will be reviewed.

#### **Pretrial Publicity**

PTP became a topic of research in the 1960s, shortly after the American Bar Foundation (ABA) released recommendations for what information law enforcement and court personnel should not give to the press. The most recent guidelines (ABA, 1991) included information on a confession or similar statement, results of tests, opinions on guilt, information on defendant's character, and information on prior arrests or convictions. A handful of researchers have performed content analyses of the media to determine whether these guidelines have been followed and what type of information is actually being included in PTP, as well as to provide estimates of how many stories a given defendant receives. Often, such content analyses are performed as part of a change of venue motion (e.g. Moran & Cutler 1991, 1997; Studebaker, Robbenolt, Pathak-Sharma, & Penrod, 2000) or as part of an experimental study using a certain trial (e.g., Chrzanowski, 2006), meaning these analyses examine only one case. Other content

analyses have examined the media in general, and have found that PTP is predominantly pro-prosecution and the recommendations of the ABA are routinely violated (Imrich, Mullin, & Linz, 1995). Further, certain defendants, such as racial minorities, may be the subject of more negative PTP (Dixon & Linz, 2002).

Bruschke and Loges (1999) examined all federal first-degree murder trials in a 2-year period and found that approximately 46% of the cases received no publicity. The remaining cases varied in the amount of PTP received, ranging from cases covered in one article to one case that received 141 articles. The more sensational the crime, the more publicity it will receive. For instance, Stuebaker, Robbennolt, Pathak-Sharma, and Penrod (2000) content-analyzed over 1500 articles for the Timothy McVeigh trial; these articles were randomly selected from specific venue locations, meaning that this number does not represent the entire population of articles on this case.

### **Non-Experimental Studies of PTP Effects**

Obviously, PTP does not occur in all cases, but it does occur in a sizeable proportion of cases, and, because it is very likely to be negative toward the defendant, it is a cause for concern. Various researchers have studied the effect of naturally occurring PTP on the tendency to prejudge guilt-innocence, as well as a variety of other variables, such as sentencing preference and the ability to be impartial. Some were performed as part of a motion for a change of venue, while others were performed to examine a new methodology in performing public opinion surveys. These studies provide information on how PTP affects verdict preferences pretrial.

Though these studies differ in how PTP exposure is operationalized, PTP exposure does appear to affect beliefs about guilt, especially pretrial. Nietzel and Dillehay (1983) presented the results of 5 community surveys, four of which compared respondents in the trials' current venues (where PTP exposure was high) to one or more alternative venues. Results in general show that respondents from the venue counties, where PTP tends to be greater, were more likely to have heard about the cases, read stories about the cases, and believe the defendants were guilty than respondents from other counties. Vidmar and Judson (1981) and Moran and Cutler (1991, 1997), on the other hand, operationalized PTP exposure by whether a respondent was aware of a case, and found that respondents who had heard about a case were more likely to believe the defendant was guilty.

Other researchers have tested respondents' knowledge of the case, and examined guilt based on specific pieces of content remembered (Chrzanowski, 2006), categorized participants by level of knowledge (Constantini & King, 1980/1981; Simon & Eimmermann, 1971), or correlated number of correct responses with guilt (Arbuthnot, Myers, & Leach, 2002; Studebaker, Robbennolt, Penrod, Pathak-Sharma, Groscup, & Devenport, 2002). Regardless of how knowledge was measured, it was found to relate to belief in guilt; respondents who were more knowledgeable were more likely to believe the defendant was guilty.

These surveys show that respondents who are knowledgeable about the trial are more likely to prejudge, though there are some exceptions. Brown, Duane, and Fraser (1997) found that respondents with increased exposure to O.J. Simpson through the

media were more likely to believe Simpson was innocent, though this was because participants with increased exposure tended to form a parasocial relationship, a psychological connection people may develop with celebrities, with Simpson.

Obviously, when the defendant is a celebrity, PTP may have different effects on potential jurors and may even create sympathy for the defendant.

Some studies have used naturally occurring PTP, and then presented participants with trial information. Freedman and Burke (1996) measured knowledge of the case, and then gave participants a brief summary. Because the case had not yet come to trial, the researchers created the trial summary based on similar cases. Though knowledgeable participants believed, pretrial, that the defendant was guilty, this effect went away after reading the summary. Participants were informed that this trial information was fabricated and that the researchers did not have inside information on the trial; the trial information was also very weak, yielding approximately a 31 percent conviction rate overall.

Honesty, Charman, and Levi (2003) also studied naturally occurring PTP in a fraud case in England. Participants reported what they could remember about the case, and their responses were classified as being either emotional (such as information on the effect the crime had on the victims) or factual (such as the amount of money involved) PTP. Over a period of days, participants viewed a six-hour video re-enactment and provided their thoughts and verdict preferences throughout. At the end of the trial, participants rendered a final verdict and evaluated the evidence. Results showed an effect for emotional PTP, but not for factual PTP; participants who had recalled emotional

information pretrial were more likely to render a guilty verdict. In addition, participants' reasoning about the trial was found to mediate the effect of PTP on verdict. The PTP had affected the way they considered the evidence throughout the trial, which in turn, influenced their verdict.

In another examination of naturally occurring PTP, Chrzanowski (2006) conducted an internet study in which she measured the effect of naturally occurring PTP on pretrial, midtrial, and post-trial verdicts in an acquaintance rape trial and found an effect for PTP. Participants completed a questionnaire of what they remembered from PTP, then over a period of months, completed guilt ratings and evaluations of various pieces of evidence after each trial summary. Participants who reported viewing pro-prosecution PTP, which was coded based on what information they recalled from the media, rated the defendants as significantly more guilty throughout the trial than participants who had recalled neutral or pro-defense PTP information. Participants who had recalled pro-defense PTP, on the other hand, rated the defendant as significantly less guilty. Pro-defense PTP, however, is far less common than pro-prosecution PTP.

One study examined the effect of naturally occurring PTP by accident. While studying the effect of eyewitness testimony in a trial summary, Greene and Loftus (1984) suddenly saw a drop in conviction rates. On the debriefing questionnaires, many participants mentioned that they had acquitted the defendant because of a recent case in the media (the Titus case) in which a man had been released after it was established that the victim in his trial had been mistaken in identifying him as the perpetrator. This study was one of the first to examine general PTP – media information on crime in general or

on a different trial that influences verdicts in the target trial. In a second study, Greene and Loftus recruited a community sample, asking if they had heard about another case involving a mistaken eyewitness, and measured their verdict preference in the case from their first study. Once again, participants who had heard about the case of mistaken identification were less likely to convict.

While these results from non-experimental research on PTP are mixed, they generally show that PTP biases participants, even after exposure to trial information. Such research, however, does not allow control over other aspects that may affect trial outcome, such as facts of the case, characteristics of the parties involved, timing of the PTP, and contents of the PTP. Such research also does not clearly show why trial information counteracts the effects of PTP in some cases, but not others. Therefore, in addition to the study of naturally occurring PTP, many researchers have performed experiments in which they manipulate the presence of PTP and measure the effects on verdicts and guilt ratings.

### **Experimental Studies of PTP Effects**

Though there are many contradictory findings in the experimental PTP literature, there are some factors that have been demonstrated to affect study outcome. One commonly studied factor is the type of information presented in PTP. The ABA guidelines included a variety of information that should not be published in PTP, and content analyses (Dixon & Linz, 2002; Grady, 1972; Imrich, Mullin, & Linz, 1995) have shown that PTP can include many different pieces of information - from hearsay to



prior convictions to confession information. Research suggests that the content of PTP may have an effect on guilt.

Two of the most biasing pieces of information, a confession and prior convictions, are often ruled inadmissible in court; the former is ruled inadmissible if it was coerced, and the latter is generally not allowed in court except when the defendant testifies. De Luca (1979) compared several news stories, to determine how different pieces of information affected perceptions of guilt. Participants who read about a confession, either alone or in combination with information on prior arrest and/or failure of a lie detector test, were more likely to believe the defendant was guilty of embezzlement than control participants. Unfortunately, he did not include conditions in which prior convictions or performance on a lie detector test were presented alone, so it is difficult to tell specifically what information biased participants.

Padawer-Singer and Barton (1975) conducted two studies, with the cooperation of the Supreme Court of the State of New York, on the negative effects of confession and prior record information, using deliberating juries. Participants were randomly selected from the court's jury pools and were assigned to 12-person juries. Half of the juries received a news story stating that the defendant had a prior record and had confessed to the crime. Participants met in a courtroom, where they listened to a recording of a trial, then deliberated to a verdict. The first experiment ended with many hung juries, but individual jurors who had received PTP were more likely to vote guilty. In the second experiment, more time was given for deliberation, meaning there were fewer hung juries, especially among exposed jurors. Juries receiving negative PTP were more likely to

convict. Once again, because prior record and confession were always presented together, it cannot be ascertained whether one is more biasing than the other, or what effect each piece of information would have alone. However, this study does support the contention that such information is biasing to juries. Tans and Chaffee (1966) also compared different pieces of favorable or unfavorable information, and examined the effect confession alone or when combined with other information. Once again, information on a confession increased perceptions of guilt, especially when this information was presented alone.

Prior record alone is also believed to be very biasing to jurors. Otto, Penrod, and Dexter (1994) examined prior record, along with other biasing pieces of information: negative statements on the defendant's character, information on the defendant's low status job, and strong or weak inadmissible statements from the defendant's neighbor. Participants were randomly assigned to receive one of these pieces of information, rendered a pretrial verdict, viewed a videotaped trial, and completed a measure of verdict and perceptions of the defendant. Character information and inadmissible statements were found to influence pretrial verdicts, which, in turn, influenced final verdicts. Character information and strong statements biased the participants toward guilt, while weak statements had the opposite effect. In addition, prior record affected final verdict indirectly; knowledge of a prior record made participants more likely to view the defendant as a "typical criminal" which led to more guilty verdicts.

Placement of the information did not have an effect. Hvistendahl (1979) also studied prior arrest, as well as other negative information, examining whether placement

moderated the effect of PTP on verdict. Participants were randomly assigned to receive control information, prior arrest, gang membership, or defendant race. He found that prejudicial information presented in the last paragraph was just as biasing as prejudicial information presented in the lead paragraph, and participants who had received information on prior arrest or gang membership were more likely to believe the defendant was guilty than participants receiving control or race information.

In addition, though not part of the ABA's guidelines, some researchers have examined the effect of reports on evidence, such as witness information or physical evidence to be used in trial. Even if the participants eventually hear the evidence in trial, such information presented in PTP can be one-sided. Shaw and Skolnick (2004) compared PTP containing witness testimony information to PTP summarizing physical evidence, and found that participants were more likely to find the defendant guilty in the physical PTP condition. This effect was only significant, however, after participants had deliberated. Burke (2000) also examined reports on evidence. Participants then viewed a trial and deliberated in groups of 4 to 7. After deliberation, participants rendered individual verdicts. Only participants receiving eyewitness information in PTP rendered any guilty verdicts; all other participants found the defendant not guilty.

Another way content has been studied is by classifying PTP as emotional or factual in nature. Kramer and Kerr (1989) compared participants receiving control information, factual information (prior convictions and inadmissible physical evidence), emotional information (a hit-and-run charge), or both factual and emotional information in PTP. Participants receiving any biasing information were more likely to convict. It

was argued, however, that emotional information may be more memorable and that emotional PTP may still have an effect on verdicts after a delay.

To study this possibility, Kramer, Kerr, and Carroll (1990) performed an elaborate study using the same case and stimuli as Kramer and Kerr (1989). Some participants saw the case immediately after seeing PTP, while others saw the case after a delay. In addition, some participants were allowed to deliberate. The majority of participants were jury-eligible community members, with a few student participants included only in sessions without enough participants to create complete juries. They found that, while a delay reduced the number of guilty verdicts among participants receiving factual PTP, it had no effect on participants receiving emotional PTP, who were more likely to find the defendant guilty than participants not receiving emotional PTP. They also found that PTP effects were stronger for jurors allowed to deliberate than for participants asked to decide the case alone.

Another factor that may affect study outcome is type of media. Many researchers use newspaper stimuli, but some research (e.g., Rollings & Blascovich, 1977) suggests that PTP viewed in other media, such as on TV, may have different effects. Ogloff and Vidmar (1994) compared control participants to participants receiving newspaper only, video only, or both newspaper and video, to determine if media had an influence on study outcome. Participants receiving any media were more likely to believe the defendant was guilty than control participants. The media groups did not significantly differ, but the authors argued that the results showed a definite trend over all dependent variables, and that this trend was in the hypothesized direction: belief in guilt and preferred sentence

were highest in the video and newspaper condition, and lowest in the newspaper only condition.

Deliberation has also been identified as a factor that can affect study outcomes (Bruschke & Loges, 2004). A handful of studies, including a few of the studies discussed previously, included deliberation, with some finding that deliberation reduced the effect of PTP (e.g., Burke, 2000), and with others finding no effect (e.g., Padawer-Singer & Barton, 1975) or even an increased effect of PTP (e.g., Davis, 1986; Dexter, Cutler, & Moran, 1992; Kramer, Kerr, & Carroll, 1990; Ruva, 2006; Shaw & Skolnick, 2004). These mixed results have led some researchers to examine possible explanations. One explanation for the mixed effects of deliberation is source-monitoring. It has been argued that PTP has an influence on post-deliberation verdicts because jurors forget where the information came from, and believe the PTP information was actually presented in trial evidence. Ruva, McEvoy, and Bryant (2007) compared participants receiving PTP or unrelated news stories who either deliberated or individually listed as many trial facts as they could remember. Participants exposed to PTP were more likely to find the defendant guilty than participants who read unrelated stories; deliberating jurors were less likely to find the defendant guilty than non-deliberating jurors, but deliberation did not interact with PTP. Even after deliberating, exposed jurors were more likely to vote guilty than non-exposed jurors. Ruva et al. also found that after deliberation, exposed jurors were more likely to mistake PTP for trial evidence. Non-deliberating jurors did not make such source-monitoring errors.

Another explanation is case strength. Kerr, Niedermeier, and Kaplan (1999) compared jurors receiving negative PTP to jurors receiving positive PTP after reading either a moderate-strength case or a weak case. They found that participants receiving negative PTP and those reading a moderate-strength case were more likely to find the defendant guilty, but found no interaction between PTP and case strength. In addition, jurors who deliberated were less likely to find the defendant guilty than non-deliberating jurors in the weak case, but when reading the moderate case, deliberating jurors were more likely to find the defendant guilty. Essentially, studies that use moderate-strength cases should be more likely to find increased PTP effects after deliberation than studies using weaker evidence or studies that do not allow deliberation.

Strength of the evidence has been examined in other PTP studies, as well. In fact, some researchers (e.g., Dexter, Cutler, & Moran, 1992; Fein, Morgan, Norton, & Sommers, 1997; Riedel, 1993; Ruva et al., 2007; Ruva & McEvoy, 2008) have noted that a moderate-strength case was specifically chosen for their research, because PTP is more likely to have an influence. This is based on the liberation hypothesis (Kalven & Zeisel, 1966), which states that jurors will decide a case based on the evidence in strong and weak cases, but will be more likely to use outside information when case strength is moderate.

A recent field study (Devine, Buddenbaum, Houp, Studebaker, & Stolle, 2009) examined the liberation hypothesis in real cases. The study was conducted using court cases from courts in Indiana. Data on strength of the evidence and extraevidentiary variables (such as PTP) were gathered through questionnaires from judges and attorneys,

and final verdicts were measured with questionnaires from jurors. The authors analyzed the moderating effect of strength of the evidence in two ways. First, they correlated extraevidentiary variables with verdict while partialling out variance due to strength of the evidence. Second, they divided cases into weak, moderate, and strong evidence, and separately conducted correlations between extraevidentiary variables and verdict at each level of evidence strength. PTP still had a significant effect on verdict even after controlling for evidence strength, but when cases were classified as weak, moderate, or strong, PTP only had a significant effect on verdict for moderate cases.

### **Methodological Issues with Studying PTP**

Several researchers (Bruschke & Loges, 2004; Steblay, Besirevic, Fulero, & Jimenz-Lorente, 1999; Wilson & Bornstein, 1998) have noted that the method used to study PTP effects may influence the outcome. Though people tend to evaluate targets in terms of good-bad naturally, perhaps even automatically (Ajzen & Sexton, 1999), it appears that when someone is being asked to perform this process as a juror, the outcome may be different from a person who has been asked to do something else. The role people are asked to play in a study can affect how they evaluate their target, the defendant (Freedman, Martin, & Mota, 1998). For instance, in Frame's (2001) study, participants told to evaluate the defendant were more likely to evaluate the defendant negatively than participants asked to read and evaluate the comprehensibility of the trial transcript. It is unclear, however, whether the different outcome is because the participant employed a different process when being asked to serve as a juror versus being asked to evaluate the comprehensibility of the transcript, or whether being asked to

evaluate something else utilized cognitive resources that would have otherwise been used to evaluate the defendant. In any case, the instructions given, whether serving as a cover story or simply to create a control condition, can have profound effects on the study outcome.

A study by Simon (1966) also shows the effect of instructions. Simon compared news stories written in different styles. Participants received negative PTP written in a conservative style or a sensational, tabloid style. Those who read the sensational article were more likely to believe the defendant was guilty than participants reading the conservative article. The effect went away, however, after hearing the trial and being admonished by the judge to ignore PTP information. Some (Lieberman & Arndt, 2000) question whether these findings are due to demand characteristics; participants were recruited for a study on the “problem of trial publicity” (p. 41). In addition, all participants received the same information pretrial, so comparisons could not be made to participants who received no information; though trial information reduced guilty verdicts, it is possible that participants receiving PTP were still more likely to vote guilty than participants receiving no PTP.

Lack of trial information can also explain differing results. Public opinion surveys aimed at measuring the tendency to prejudge usually do not present information from the trial; this is understandable, because in many of these cases, the trial has not yet begun. Some experimental studies, however, use PTP as their only stimulus, and even ask participants to take on the role of juror and decide the case based on the news articles they received. Obviously, this would maximize the chances that participants base their



decisions on prejudicial information and may even cause them to “fill in the blanks” left by the lack of trial information with other information gleaned from the media and personal stereotypes in order to form a cohesive story (Finkel, 1995). In addition, some studies use only brief summaries. Kramer and Kerr (1989) found a difference between participants viewing a short trial and participants viewing a longer trial; conviction rates were higher in the short trial. They did not, however, find a significant interaction between trial length and PTP exposure.

Freedman, Martin, and Mota (1998) argue that procedure and method of measurement explain PTP effects. In their first study, participants received negative or neutral PTP, viewed a video-taped trial, and then received either neutral instructions from the judge or an admonition to ignore PTP information. The authors found no effect of PTP. In their second study, participants followed the same procedure, except half of the participants gave an additional rating of their verdict preference prior to viewing the trial. The authors found that negative PTP led to higher guilt ratings posttrial only among those participants who gave their opinion pretrial.

Source of the sample has also received criticism, with some questioning whether research based on student samples is generalizable to the courts. While most of the studies discussed previously used student samples, Sue and colleagues have shown that these effects are also found in community samples. Sue, Smith, and Gilbert (1974) compared student participants to participants recruited throughout the community. All participants received a newspaper article on the case that mentioned a gun found in the defendant’s possession; in half of the articles, the gun was the murder weapon but was

inadmissible in court (due to an illegal search), and in the other half of the articles, the gun was not the murder weapon. The researchers found no difference between students and community members. Participants who were told the gun was the murder weapon were more likely to believe the defendant was guilty; this effect was stronger among female participants.

Bornstein (1999) also examined sample source, in an analysis of all jury simulation studies published in *Law and Human Behavior* between 1977 and 1996. He found that 65% of the studies used undergraduate samples and 55% used written trial stimuli, and that the proportion of studies using undergraduate samples is increasing over time. His review of the literature and its findings, however, uncovered few differences between studies using written stimuli and those using more realistic trial presentations. Though Bornstein states that there is not much cause for concern over the validity of less realistic studies, he does warn that the court is often suspicious of such studies (a sentiment that has been echoed by others: Studebaker & Penrod, 1997; Studebaker, Robbennolt, Penrod, Pathak-Sharma, Groscup, & Devenport, 2002), and recommends that researchers performing simulation research using student samples and/or written stimuli follow these studies with replications that use more diverse samples and realistic stimuli to demonstrate the robustness of the findings.

It should be noted, though, that there are a handful of field studies – studies that have examined PTP and trial outcomes in real cases. Even these studies have found mixed results. For example, Brusckie and Loges (1999, 2004) studied first-degree murder and robbery cases in the federal courts and found that PTP generally had no effect

on trial outcome. Devine et al. (2009) on the other hand, found that PTP did have an effect on trial outcomes, even after controlling for strength of the evidence. The effect was strongest, however, in cases of moderate strength. There were key differences in how these researchers measured PTP; Brushke and Loges searched for defendant names in Lexis/Nexis and counted any article that mentioned the crime, defendant, victim, or (in some cases) the defendant's gang, but did not measure whether the PTP was negative or what information it contained. Devine et al., on the other hand, asked the judge on the case whether there had been "significant pretrial publicity" (p. 140) or if any jurors had been removed from the panel due to media exposure. These studies, as well as the others discussed, should make it clear that the method through which a researcher studies PTP can have an effect on the outcome of the study, even outside of the lab.

### **Meta-Analysis on PTP**

Recently, I completed a meta-analysis on the subject of PTP (Locatelli, Fulero, & Kluwe, 2011); many of the previously discussed effects, including the effects of remedies, were investigated. Searches for published studies were conducted using online databases, the American Psychological-Law Society newsletter, manual inspection of several applied and forensic psychology journals, and reference lists. Unpublished studies were solicited by contacting PTP researchers and with a post to the PSYCH-LAW list-serv. The results of 99 studies provided 37 pretrial effect sizes and 68 posttrial effect sizes of negative PTP on guilt. These two sets were meta-analyzed separately from each other, due to the fact that some studies measured PTP effects both pretrial and posttrial. In both cases, a random effects model was used; this model assumes that there is not a

single true effect size, but a distribution of possible effect sizes depending on characteristics of the study.

Many moderators were examined, including source of the sample, crime of which defendant was accused, strength of the evidence, deliberation, trial presentation format, judicial admonition, judicial instructions, and *voir dire*. Pretrial, no moderators were significant. The grand mean effect size was  $r = 0.323$ ,  $SE = 0.042$ ,  $Z = 7.96$ ,  $p < 0.001$ , with a 95% confidence interval of 0.245 to 0.397. This interval is expected to contain the true grand mean of pretrial effect sizes 95% of the time. In addition, because the assumption of a random effects model is that the grand mean is not representative of all studies on a topic, these analyses produced a 95% prediction interval of -0.223 to 0.893, which reflects the distribution of possible individual study effect sizes depending on the study's characteristics. As this prediction interval suggests, it is not unusual for a single study to find no effect or even a negative effect of PTP, due to the distribution of true effect sizes, though more studies should find a positive effect of PTP.

Posttrial, several moderators were significant. Strength of the evidence was found to affect study outcome, with studies using moderate strength cases finding the largest effect sizes. Crime also had a significant impact; studies using sex offenses and non-violent crimes tended to have smaller effect sizes, and studies using homicide cases tended to have larger effect sizes. Finally, trial presentation format had a significant effect, with studies using more realistic trial media finding smaller effect sizes. Source of the sample and deliberation had no significant effects on study outcome. In addition, whether participants were asked about guilt pretrial, which Freedman, Martin, and Mota

(1998) argue explains why studies find an effect of PTP, had no effect of study outcome when examined across all 68 studies. The grand mean effect size was  $r = 0.163$ ,  $SE = 0.023$ ,  $Z = 7.14$ ,  $p < 0.001$ , 95%  $CI = 0.118, 0.207$ , with a 95% prediction interval of -0.222 to 0.550. Once again, it is not unusual for a study to find no effect or even a negative effect of PTP, due to the distribution of true effect sizes and the characteristics of a given study, but more studies should find positive effects.

Essentially, PTP does have an effect on verdicts and guilt ratings, both pre- and posttrial, and many factors explain the variability in study findings. One issue that could not be fully explored in meta-analysis was the effect of remedies. There was a great deal of variation in combinations of remedies, and in some cases, only one study used a particular type of remedy or combination of remedies. Some research suggests, however, that remedies can be effective in alleviating PTP effects.

### **Using Remedies to Alleviate PTP**

Information that is not refuted in trial can bias individual verdicts (Burke, 2000). If information has been ruled inadmissible prior to seating a jury, such as in a pretrial hearing, the jury may not even hear that the information is not to be used. A general judicial admonition may or may not be effective at discounting such information. Many studies (e.g. Fein, McCloskey, & Tomlinson, 1997; Sue, Smith, & Caldwell, 1973; Sue, Smith, & Gilbert, 1974) suggest general admonitions are not effective.

Admonitions specifically mentioning the forbidden information have not been very effective, either. Lee, Krauss, & Lieberman (2005) studied strong judicial admonitions in the context of inadmissible hearsay evidence in a civil case. Participants

received no admonition, a weak admonition that hearsay evidence is not admissible, or a strong admonition that jurors are prohibited from using such evidence. The authors found no difference in verdicts, but also found that participants receiving the strong admonition were more confident in their verdicts. Wolf and Montgomery (1977) found that stronger and more specific admonitions increased dependence on the inadmissible information, and also increased participants' self-reported desire to use such information. Similarly, Bornstein, Whisenhunt, Nemeth, and Dunaway (2002, Study 2) examined whether a specific judicial admonition to ignore pretrial publicity would alleviate pretrial publicity effects in a civil trial. They found a main effect for instruction. Participants receiving instructions to ignore PTP were less likely to find the defendant liable, including participants who did not receive any PTP. Unfortunately, instructions did not interact with PTP.

Another method of admonishing the jury involves giving a reason to ignore the forbidden information. Freedman, Martin, and Mota (1998, Study 1) examined a judicial admonition, which not only contained an instruction to ignore PTP, but also included the statement that PTP is at "risk of being unreliable" (p. 260). They found that this admonition decreased guilt votes overall and, the authors note, even appeared to have a stronger effect on participants who had received negative PTP, though the interaction term was not significant.

Kassin and Sommers (1997) also studied the effect of explanations for judicial admonitions, and found that some reasons may be more effective than others. In trial, some participants heard a taped conversation, in which the defendant confessed to the

crime. In one condition, the tape was ruled inadmissible because it had been obtained illegally. In another, the tape was ruled inadmissible because it was of poor quality and difficult to hear. When participants were told the tape was illegally obtained, they were more likely to use the evidence. When they were told the tape was barely audible, they followed the admonition and decided the case similarly to the control participants.

Many psychological theorists as well as legal personnel (e.g., Curtner & Kassier, 2005) have offered reasons for why these remedies are ineffective, such as psychological reactance and ironic process theory (Lee, Krauss, & Lieberman, 2005), which will be discussed in detail later. It is possible that the remedies do not work, and that courts should consider their “last-resort” remedies, such as change of *venire* and change of venue more often. This has been recommended before (e.g., Fulero, 1987). However, others suggest that studying remedies in experimental isolation does not actually demonstrate how these remedies work in real-life. Brusckhe & Loges (2004) discuss the cumulative remedies hypothesis, which states that single remedies that appear ineffective alone may be effective when combined with each other. Not only could the effects of remedies be additive or even multiplicative, but remedies are rarely used in isolation in actual courtrooms; judges frequently utilize more than one remedy. In addition, certain elements that are referred to as remedies by the psychology literature are often fixed parts of the trial: *voir dire* and deliberation, for instance.

Studies utilizing multiple remedies are rarely performed, though they have been recommended by many. Carroll, Kerr, Alfini, Weaver, MacCoun, and Feldman (1986) recommend that researchers perform a study using three groups: a control group who

views no PTP, a treatment group who views PTP, and a second treatment group who views PTP and is then subjected to a set of judicial remedies. Another possibility is that certain remedies are more effective on some people, especially people with certain attitudes. Specifically attitudes toward PTP information may affect participants' inclination to use such information in their verdict decisions and may also influence the effectiveness of remedies.

### **Legal Attitudes**

A handful of studies of PTP have examined participants' attitudes. In general, these studies use an attitude measure to categorize participants. One way of categorizing individuals is as authoritarian or non-authoritarian (also known as egalitarian). Sue, Smith, and Pedroza (1975) found that, although authoritarianism affected perceived strength of the prosecution's case, there was no significant effect on verdict or sentence.

Another way involves classifying individuals as either "due process" or "crime control." Arbuthnot, Myers, and Leach (2002) asked their participants three questions designed to measure crime control attitudes and three questions designed to measure due process attitudes. The last three items were reverse-scored to create a composite score of what the authors called "pro-prosecution attitudes." These attitudes were not significant predictors of guilt ratings.

Though PTP researchers have examined attitudes in their research, only a few have examined the interactions between specific attitudes and PTP. Butler (2007) examined how PTP interacts with death qualification in a murder trial. Prior research suggests that participants who are "death-qualified" tend to be qualitatively different



from and more conviction-prone than “Witt excludables” – jurors whose beliefs on the death penalty are so strong (either for or against) that they could not carry out their duties as a juror in a death penalty case. Butler’s (2007) research suggests that death-qualified jurors may be more susceptible to PTP effects; death-qualified participants were more likely to attend to PTP and were more likely to remember information obtained from media sources. This, in turn, made death-qualified participants more likely to believe the defendant was guilty. Death-qualified participants were also more likely to believe the defendant would receive a fair trial, suggesting that they may also be more likely to state they could be impartial jurors than “excludable” participants.

Chrzanowski (2006) also examined interactions between PTP and attitudes. In her path analyses, she incorporated interactions between specific attitudes toward rape and PTP bias (the degree to which the information received/recalled was pro-prosecution or pro-defense), finding several significant effects on final verdicts. She did not, however, examine the relationship between specific attitude and specific pieces of information from the PTP. In addition, her results cannot be generalized to the effect of specific attitudes on cases other than rape. Regardless, Chrzanowski’s approach is much different than the approach other researchers have taken to studying the effect of attitudes; most have combined scores on attitude measures to create an overall composite. Many argue that the category into which a person falls (due process or crime control, authoritarian or egalitarian, and so on) should affect verdict, but these categories are based on the scores a person receives and not necessarily on their feelings about individual pieces of information. An individual may be “due process” overall, but if he

or she has an attitude that only guilty people run from the police, finding out that a defendant resisted arrest would bias him or her against that defendant, perhaps regardless of other attitudes. This may explain some researchers' null findings when examining attitudes.

### **Theories Explaining or Predicting These Effects**

There are a variety of theories that explain why specific attitudes, and the strength of those attitudes, could affect verdict preferences, regardless of the remedies used in trial. Though PTP researchers have examined attitudes in their research, few have integrated attitude theory into their research or used such theory to devise hypotheses.

According to Ajzen and Sexton (1999), the expectancy-value model is the most accepted theory of how attitudes are formed. This theory states that attitudes are a function of the strength of beliefs about the attitude object and the evaluation (good-bad, desirable-undesirable, and so on) of that belief. Attitudes develop as more beliefs are formed and evaluated, which occurs as more information is acquired about the attitude object. Though an individual would not have a pre-made attitude about the defendant before encountering the PTP, an individual would likely have attitudes about the behavior of defendants, based on knowledge of past cases, media portrayals of crimes, and stereotypes (Finkel, 1995). Information provided in PTP should affect a person's beliefs about a defendant and the likelihood of guilt, and the evaluation of these beliefs should be influenced by pre-existing beliefs, attitudes, and biases about such pieces of information. The stronger the attitude, the more quickly the attitude is activated in the relevant situation (Fazio, 2000). For instance, if I discover that a defendant has confessed to a

crime, this would affect my belief about the defendant. The amount of weight I ascribe to this belief, however, depends on my evaluation of this information. If I believe that confessions occur for other reasons than guilt, such as coercion by the police or a desire to protect someone else, then the weight I ascribe to this piece of information in generating my overall attitude toward the defendant is low. On the other hand, if I strongly believe that a person who confesses is guilty, this information would be activated more quickly when encountering the current defendant, receive more weight, and strongly affect my overall attitude toward the defendant.

The question becomes whether this attitude toward the defendant in reference to his or her guilt affects behavior, which is, in this situation, selecting a verdict. According to Ronis, Yates, and Kirscht (1989), a great deal of research has found a weak relationship between attitudes and behavior. Ajzen and Fishbein (1977) argue, however, that such weak relationships are found when the attitude being measured is very general and the behavior, very specific. When more specific attitudes are measured, they are more predictive of specific behavior. In addition, Ajzen and Sexton (1999) argue that strong attitudes are less likely to vary by situation, and are thus, more predictive of behavior than weak attitudes. This means that, when specific attitudes toward PTP information are measured, they should relate to verdict preferences, particularly when the attitudes are strong.

When PTP is an issue in a case, the judge will often try to reduce the biasing effects of PTP information by issuing an admonition to jurors to ignore any information obtained outside of the trial. As has been discussed previously, research on judicial

admonition has found little support that this remedy reduces bias, at least when used in isolation. Various researchers have offered explanations and theories for why the admonition is ineffective, including ironic processes (Lee, Krauss, & Lieberman, 2005), thought suppression (Studebaker & Penrod, 1997), and reactivity (Lee, Krauss, & Lieberman, 2005).

According to Lee, Krauss, and Lieberman (2005), ironic process theory states that when people are told to ignore certain information, two types of mental processes are activated. The first type, conscious processes, aims at reaching the desired mental state, by thinking about what the person has been told to think about, such as the evidence. The second type, unconscious processes, monitors thoughts on the undesired mental state, by looking for thoughts on what the person has been asked to ignore, such as pretrial publicity. In trying to not think about something, the person ends up thinking about it more. This means that, when jurors are told to ignore certain information, they are motivated to oblige, but their efforts to follow the judge's instructions backfire. Another possibility, thought suppression, is similar to ironic processes. According to Studebaker and Penrod (1997), this phenomenon occurs when a person actively tries to suppress a particular thought, causing the thought to become more prominent, especially if the information is emotionally arousing. In both of these cases, the individual is trying to avoid thinking about something, but ironic processes are unconscious while thought suppression is conscious.

Psychological reactance, on the other hand, occurs when a juror does not attempt to suppress a thought, but instead ignores the judge's instructions. When being told to

ignore certain information, the juror may react by believing his free will has been threatened (Lee, Krauss, & Lieberman, 2005), leading to arousal (Lieberman & Arndt, 2000). In order to reaffirm one's sense of free will, and decrease this negative arousal, the juror purposely uses the discredited information. It is difficult to determine which of these general processes is at work, because the juror may be unaware of how she is being affected by the judge's instructions (Hammerl & Fulcher, 2005), making these processes difficult, if not impossible, to measure. Wolf and Montgomery (1977), however, found that participants receiving a strong admonition reported greater desire to use such information than others, suggesting that psychological reactance was occurring. This was only the case when the admonition was strong, as opposed to the standard admonition. In addition, a more recent study (Jonas, 2007) found that trait reactance only influenced verdict and sentencing when the inadmissible information was emotional in nature.

Regardless of which of these processes is occurring, they all lead to the same outcome – that participants will use discredited information. In addition, psychological reactance research suggests that reactance is more likely to occur when the admonition is strong and when it specifically threatens the participant's choice; it also suggests that reactance can be thought of as a personality trait (Jonas, 2007; Pavey & Sparks, 2009), rather than a universal process. Some studies find some positive effects of admonition, however, meaning that there may be some individual differences operating to clarify why admonitions are effective for some people but not others. One explanation that has received some attention is belief perseverance (Lieberman & Arndt, 2000). This phenomenon occurs when an individual continues to use a piece of information to make a

decision, even though that information has been discounted. Lieberman and Arndt (2000) argue that belief perseverance is not a good explanation for PTP, because it does not explain the backfire effect, which occurs when people exposed to a judicial admonition are more likely to use the discredited information. The study on instructions by Freedman et al. (1998), however, suggests that jurors are not blindly using biasing information. When participants were told a piece of evidence was inadmissible because it was illegally obtained, they still used it, but when a piece of evidence was considered inadmissible because it was of poor quality, participants were able to suppress that information.

It is possible, however, that by not examining attitude toward the information in past research, that belief perseverance effects were missed. Research suggests it is more likely to happen in people with strong attitudes (Anderson, 1995). Belief perseverance may explain why certain individuals continue to use biasing information even though the judge has admonished them to ignore it. In addition, it may be strong attitudes, not psychological reactance, that explain the backfire effect. Therefore, if belief perseverance is operating, participants with strong attitudes about a piece of information and its probative value should be more likely to continue to use it after being admonished by the judge and may ascribe greater weight to this information than participants with weaker attitudes. This is much different from the explanations discussed previously, because ironic process, thought suppression, and reactivity effects are related to the instructions as well as the biasing information, not an individual's attitudes toward that information. By also examining attitudes, then, a researcher can make a comparison

between these general processes that should affect people with strong and weak attitudes alike to processes like belief perseverance that should interact with attitude strength. This allows a researcher to use theories to create and test competing hypotheses – something which, reviewers argue, is lacking in PTP research (Bruschke & Loges, 2004).

## CHAPTER THREE

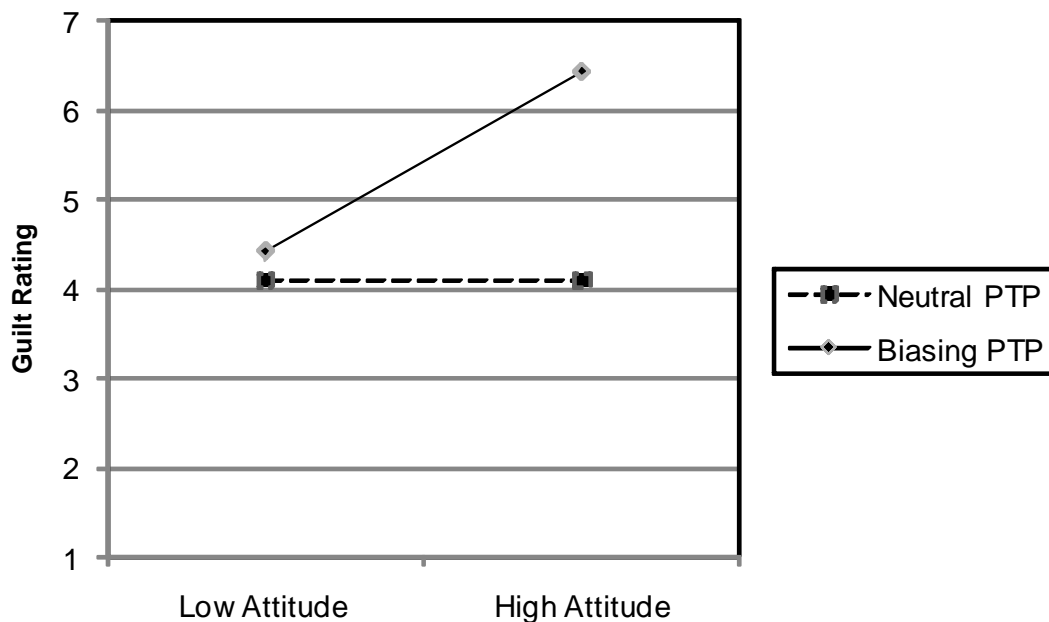
### OVERVIEW OF THE STUDY

The study will examine three variables. The first variable is participant attitudes on specific pieces of legal information: confession, prior record, and resisting arrest. The way a participant feels about this specific information and its probative value should be more predictive of verdict preference when this information is included in PTP than general attitude toward crime. The second variable is biasing information. Participants will be randomly assigned to receive one of 3 pieces of biasing information – confession, prior record, and behavior during arrest – or to receive a control article with no biasing information. Finally, the third variable is a judicial admonition manipulation. Participants will be randomly assigned to receive standard trial instructions, an admonition specifically mentioning PTP, or a specific admonition along with a statement meant to attack the credibility of the media information.

The following attitude hypotheses will be tested. First, attitudes are expected to interact with PTP information to influence verdict preference. That is, an individual with a strong attitude toward a certain piece of information will be more likely to find the defendant guilty if PTP contains that piece of information. Participants with weak attitudes toward the PTP information will not be influenced by negative PTP, and should show similar verdict preferences as participants receiving neutral PTP. If this hypothesis is confirmed, the results should look like Figure 1 below.



Figure 1. Expected Interaction Effect of PTP and Attitude.



Hypotheses dealing with the judicial admonitions will also be tested. Based on prior research, it is expected that stronger, more specific judicial admonitions should have a stronger effect than more general admonitions, and should reduce guilty verdicts. Of particular interest in this study, however, is how judicial admonitions interact with PTP information and prior attitudes. Based on belief perseverance literature, it is expected that people with strong attitudes should be less affected by judicial admonitions when presented with biasing information; the expected results will look like Figures 2, 3, and 4 below for Standard, Specific, and Strong Instructions, respectively. Alternatively, if one of the other more general processes is operating, attitude will not interact with admonition among participants receiving biasing information.

Figure 2. Expected Interaction Effect of PTP and Attitude (Instructions = Standard).

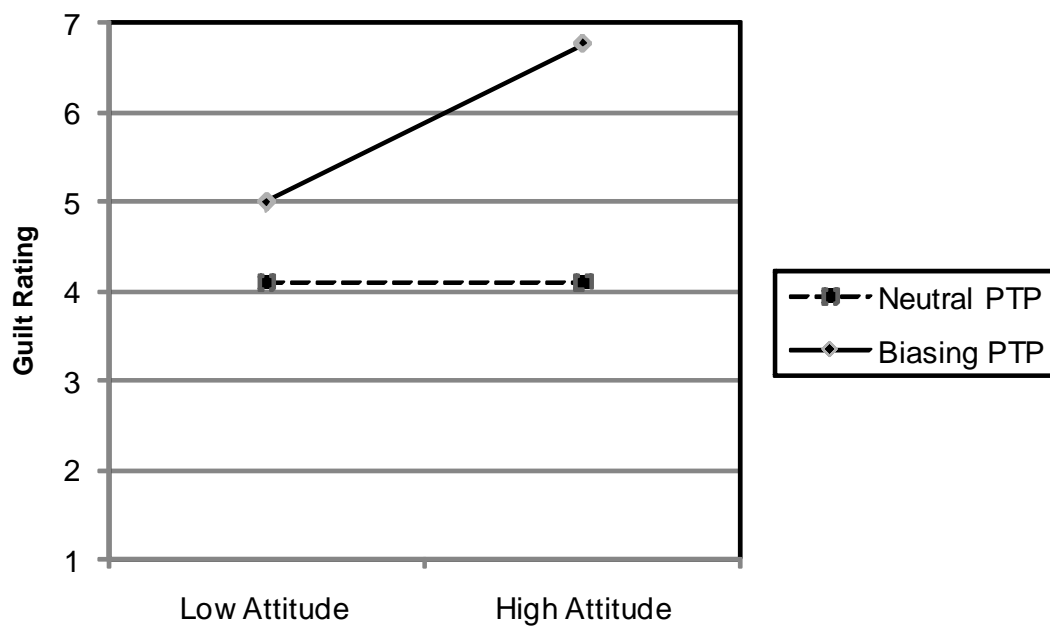


Figure 3. Expected Interaction Effect of PTP and Attitude (Instructions = Specific).

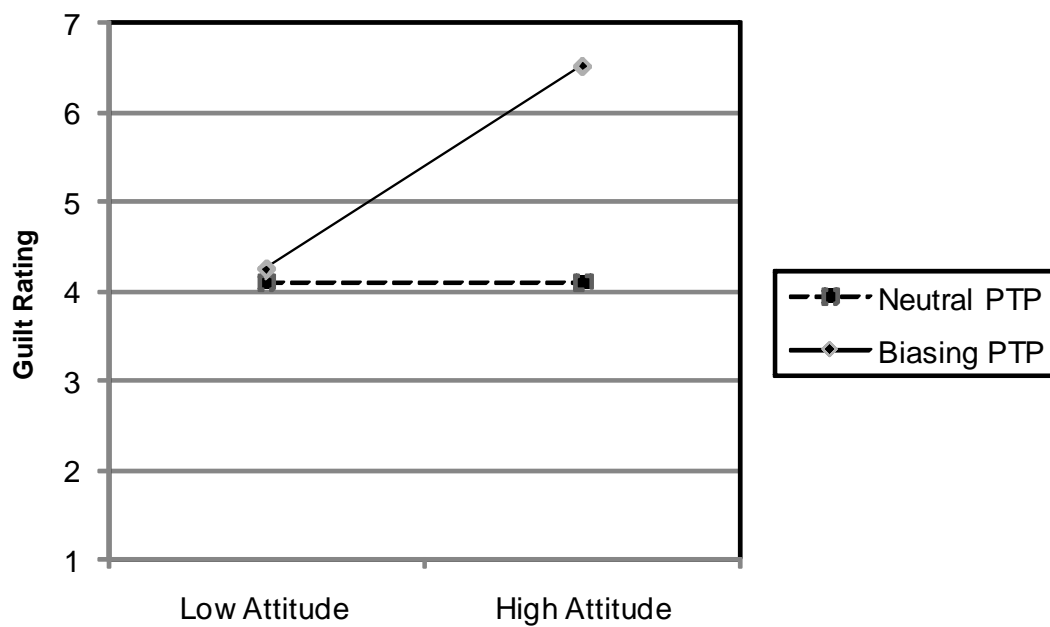
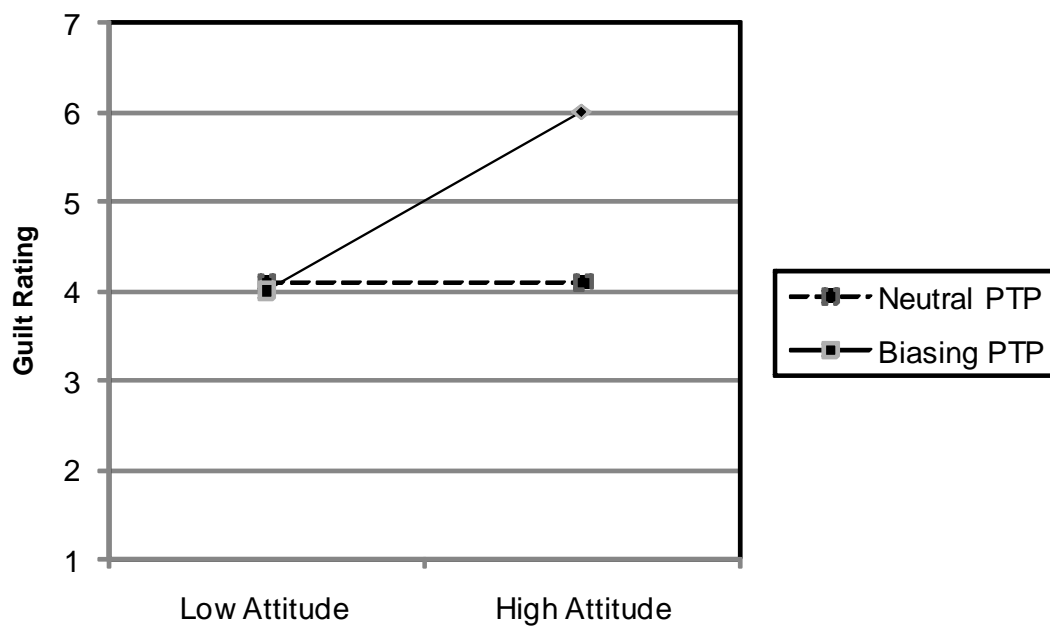


Figure 4. Expected Interaction Effect of PTP and Attitude (Instructions = Strong).



## CHAPTER FOUR

### DESIGN AND ANALYSIS TECHNIQUE

This study uses a quasi-experimental design. There are two manipulated and randomly assigned variables – PTP and instructions – and one variable, attitude strength, as a participant variable. PTP is considered the independent variable, and instructions and attitude strength are moderators. To use the terminology established by Jaccard (2001), attitude is a first-order moderator, which is expected to influence the relationship between PTP, the independent variable, and the outcome variables, verdict and continuous guilt rating. Instructions are second-order moderators, expected to interact with the first-order moderator, attitude strength, and change its effect on the relationships between PTP and outcomes.

Because the present study uses both categorical and continuous independent variables, as well as interactions between and among those variables, multiple regression was used. Interactions were tested using the techniques of Aiken and West (1991). This means first-order terms were entered into the equation first, and then interactions were entered second, to determine if they explain significant variance after first-order terms are in the equation. Some of the hypotheses can only be tested with three-way interactions; according to Aiken and West, to test three-way interactions, all first- and second-order terms must be included in the regression equation. Because of this requirement, a large number of variables are required in both equations, many of which are not being used to

test a hypothesis. In order to determine *a priori* which variables are believed to be significant, a table was created (see Table 1), stating all variables included in the regression model and which hypothesis, if any, that variable tests.

Table 1. Variables and Interactions Used to Test Hypotheses.

<b>First Order</b>	<b>Hypothesis</b>
Confession (Conf.) PTP	PTP
Prior Record (Priors) PTP	PTP
Resisting Arrest (Res.) PTP	PTP
Conf. Attitude	
Priors Attitude	
Res. Attitude	
Specific Admonition	Admonition
Strong Admonition	Admonition
<b>Second Order</b>	<b>Hypothesis</b>
Conf. PTP X Conf. Attitude	Attitude by Information
Priors PTP X Prior Attitude	Attitude by Information
Res. PTP X Res. Attitude	Attitude by Information
Conf. PTP X Specific Admonition	Admonition x Information
Priors PTP X Specific Admonition	Admonition x Information
Res. PTP X Specific Admonition	Admonition x Information
Conf. PTP X Strong Admonition	Admonition x Information
Priors PTP X Strong Admonition	Admonition x Information
Res. PTP X Strong Admonition	Admonition x Information
Conf. Attitude X Specific Admonition	
Priors Attitude X Specific Admonition	
Res. Attitude X Specific Admonition	
Conf. Attitude X Strong Admonition	
Priors Attitude X Strong Admonition	
Res. Attitude X Strong Admonition	
<b>Third Order</b>	<b>Hypothesis</b>
Conf. PTP X Conf. Attitude X Specific Admonition	Belief Perseverance vs. Alternative
Priors PTP X Prior Attitude X Specific Admonition	Belief Perseverance vs. Alternative
Res. PTP X Res. Attitude X Specific Admonition	Belief Perseverance vs. Alternative
Conf. PTP X Conf. Attitude X Strong Admonition	Belief Perseverance vs. Alternative
Priors PTP X Prior Attitude X Strong Admonition	Belief Perseverance vs. Alternative
Res. PTP X Res. Attitude X Strong Admonition	Belief Perseverance vs. Alternative

In addition to including all first- and second-order terms in the equation, continuous predictor variables should be centered, by subtracting the variable mean. This is to prevent issues with multicollinearity. According to Aiken and West (1991), the regression equation including a three-way interaction should be:

$$Y = b_1X + b_2Z + b_3W + b_4XZ + b_5XW + b_6ZW + b_7XZW + b_0$$

If  $b_7$  is significant, there is a three-way interaction. In this case, the interaction would be probed as follows. Aiken and West recommend rearranging the equation to show the regression of Y on X, which is accomplished by factoring X out of the equation:

$$Y = (b_1 + b_4Z + b_5W + b_7ZW)X + (b_2Z + b_3W + b_6ZW + b_0)$$

The first expression is the simple slope of X and the second expression is the Y-intercept. Then, X should be plotted at all possible combinations of ZH, ZL, WH, and WL, where H refers to a value one standard deviation above the variable mean and L refers to a value one standard deviation below the variable mean, or, if either Z or W is a dichotomous variable, L refers to the 0 category and H to the 1 category. This will produce 2 graphs (WL and WH), each with two regression lines (ZL and ZH).

The researcher should then test the simple slopes, to determine if the slope of the regression of Y on X is significantly different from 0 at a given value of Z and W. The slope is tested by calculating the first expression in the equation above at the selected values of Z and W, then dividing the resulting slope by its standard error, computed as:

$$s_b = [s_{11} + Z^2s_{44} + W^2s_{55} + Z^2W^2s_{77} + 2Zs_{14} + 2Ws_{15} + 2ZW s_{17} + 2ZW s_{45} + 2ZW^2s_{47} + 2W^2Zs_{57}]^{1/2}$$

where  $s_{nn}$  refers to the value in row  $n$ , column  $n$  of the regression variance-covariance matrix.

If the higher-order term is not significant, Aiken and West (1991) give recommendations on how to proceed. Keeping the terms in the equation will not substantially bias the estimates of regression coefficients for lower order terms, but will influence the power to test the significance of lower-order regression coefficients. Therefore, the researcher should make a decision about whether these terms should be kept in the equation or whether they should be dropped. If there is a strong theoretical basis for an interaction, the terms should be kept in the equation. Otherwise, the researcher should use a step-down procedure, where the higher-order terms are dropped, and the regression analyses re-performed.

There is some theoretical basis for the three-way interactions being significant, but these analyses are still somewhat exploratory in nature. In the event the three-way interactions are not significant, these terms, as well as all unnecessary two-way interactions (terms not being used to test a hypothesis), should be dropped. This will lend more power to the remaining tests and will also produce a more parsimonious model. Full and reduced models can also be compared:

$$F(m, n - k - 1) = \frac{(R^2_{in} - R^2_{out}) / (m)}{(1 - R^2_{in}) / (n - k - 1)}$$

where  $R^2_{in}$  is the total variance explained in the full model,  $R^2_{out}$  is the total variance explained in the reduced model,  $m$  is the number of terms being tested (in “in” but not “out”) and  $k$  is the total number of predictors in the full model. If  $F$  is significant, then

the two models differ in the amount of variance explained. A non-significant  $F$  means the two models do not differ and the simpler model should be adopted.

### **Power Analysis**

Many of the studies performed on PTP have used small sample sizes, and/or have involved a large number of experimental conditions. In fact, some of the studies finding null effects used few participants. It was common to see factorial studies with only 20 participants, or fewer, in each condition; this suggests that many researchers were using general rules on sample size, as opposed to power analysis. When studying a phenomenon with a small to moderate effect size, like PTP, this number may not be sufficient. No study reviewed made any mention of *a priori* power analysis, even though some researchers have tried to convince others of the importance of examining statistical power (e.g., Cohen, 1992). Obviously, a nonsignificant result does not necessarily mean power was low (Hoenig & Heisey, 2001), but analyzing power before beginning an experiment will maximize the chances that one will find a significant result, if such an effect exists.

For multiple regression, power is computed using  $R^2$  and its effect size,  $f^2$ , which is computed as  $R^2$  divided by  $1 - R^2$  (Cohen, 1988). More specifically, because the present study is testing interactions, the effect size of interest is the change in  $R^2$  after controlling for a set of variables. This first set of variables will be referred to as the “control” variables, while the additional set will be referred to as the “treatment” variables.



Power analysis was performed using PASS 2008. The current study uses 8 control variables, with an additional 21 treatment variables, though it is important to mention that only 15 of these treatment variables are being used to test hypotheses; the additional variables are entered in the equation because they are necessary to test a three-way interaction in multiple regression (Aiken & West, 1991) and are not expected to be significant. Based on results from a meta-analysis, the control variables are expected to produce an  $R^2$  of approximately 0.1681, which was entered in the power analysis as 0.16 to provide a more conservative estimate. If each of the treatment variables explains less than 0.01 of the variance in  $Y$  ( $R^2 = 0.06$ , a small effect size, Cohen, 1988), a sample of 352 would be needed. This is probably an overly conservative estimate, but this number would also provide some padding to account for any missingness or attrition in the dataset. A sample size of 310 was selected as the minimum sample size needed (based on power analysis with treatment  $R^2 = 0.08$ ), and 352 was selected as the final sample size to account for potential missingness.

## CHAPTER FIVE

### PILOT TESTING

In the recent meta-analysis (Locatelli, Fulero, & Kluwe, 2011), it was found that PTP studies most frequently use either a homicide case (29 studies, or 42.9%) or a sexual assault case (9 studies or 13.2%). In order to generalize PTP findings to actual cases, it is important to conduct studies using different crimes. Therefore, the current study utilized an assault trial. Pilot testing was conducted in order to find a case with a 50% conviction rate; PTP effects are strongest in moderate cases (Locatelli, Fulero, & Kluwe, 2011).

Further, when this project was originally proposed, an additional variable of witness characteristics was included (see Appendix A for a brief review of the literature on witness characteristic effects). It was expected that a crime committed against an elderly woman would elicit more negative affect than the same crime committed against a middle-aged man. As part of pilot testing, this hypothesis was tested.

Finally, it was originally planned that this project would be conducted online. Though other PTP studies have been conducted via the internet (e.g., Chrzanowski, 2006; Studebaker et al., 2002), no studies have been performed comparing results obtained via the internet to those obtained in the lab. Therefore, half of participants completed the study through an online survey, and the other completed the study in the lab. It was determined *a priori* that if any differences were found between lab and online results, the main study would be conducted in the lab.

## Pilot 1 Methods

### Participants

Participants were 259 undergraduates from the Loyola University Introductory Psychology participant pool. About 59 percent of the participants were women. Seventeen participants did not provide a verdict, and were dropped from analysis. Of the remaining 242 participants, 121 completed the study in person and 121 completed the study as an online survey.

### Materials

**Trial transcripts.** All transcripts were based on the case of *Brunner v. Mississippi* (2007). This case involved an assault on an elderly woman by a former employee of the woman's husband. In order to find a case with a conviction rate of 50%, six transcripts of the same trial were created, each with a different combination of the following evidence:

Favoring conviction

- First neighbor, who reports seeing the defendant's car in the area before assault
- Second neighbor, who reports seeing the defendant's car in front of the victim's home
- Victim's identification of defendant
- Case recovered from defendant's home (which was stolen from victim's home)
- Disgruntled employee argument (defendant was former employee of victim's brother, and threatened the victim's brother when he was fired)

### Favoring acquittal

- Alibi
- Poor ID by victim (not wearing glasses at the time of the attack and the fact that the brother mentioned the defendant as the perpetrator before the victim gave identification)
- Poor ID by second neighbor (unsure if make of the car matches defendant's car)
- Explanation for recovered case

In addition, for each combination of evidence, two copies of each version were created. One named the victim as Joseph Washington, a 45-year-old man, and the other stated the victim was Janice Washington, a 70-year-old woman. The names and genders of all witnesses were kept the same in all versions of the trial. In order to keep all aspects of the trial constant, the defendant was made the former employee of the victim's brother (rather than the victim's spouse as in the original case), who was living with the victim at the time of the attack due to renovations on his home.

**Posttest.** Following the trial transcript, participants completed a posttest. First, participants were asked to render a verdict, using the options "guilty" or "not guilty." After that, participants completed the Positive and Negative Affect Schedule (PANAS: Watson, Clark, & Tellegen, 1988). This scale contains 20 items, 10 of which are negative emotions such as "distressed" or "ashamed", and 10 or which are positive emotions such as "enthusiastic" or "proud."

Participants were asked to rate the degree to which the trial made them experience each emotion on a scale of 1 ("Very slightly or not at all") to 5 ("Extremely").

## **Procedure**

Participants completing the study online accessed an Opinio survey. The first screen contained a consent script (see Appendix B). Participants were then taken to one of 12 trial transcripts. Each major section of the trial (opening statements, witness testimony, closing statements, jury instructions) was on its own page. Participants completed the posttest, and were then taken to a debriefing screen (see Appendix C).

Participants completing the study in person were greeted by the experimenter; they then received the same consent script as the online survey. If they consented to be in the study, they approached the experimenter to obtain an envelope containing one of 12 trial transcripts followed by the post-test. Each participant was allowed to proceed through the packet at his or her own pace. After completing the measure, participants returned their packet to the experimenter and were debriefed in the same manner as online participants.

## **Pilot 1 Results**

All pilot study analyses were conducted using SPSS Version 16.

### **Verdicts**

Percentage of guilty verdicts was computed by version, victim characteristics, and study location (lab or online). Though certain case versions did reach the needed 50% conviction rate in some instances, there was a great deal of variation in conviction rates; victim characteristics and study location appeared to influence conviction rate. The percentages are displayed in Table 2 below.

Table 2. Conviction Rates by Trial Version, Victim, and Study Location.

<i>Version</i>	<i>Victim</i>	<i>Online</i>	<i>Lab</i>
1	Middle-aged man	30.00%	8.33%
1	Elderly woman	20.00%	20.00%
2	Middle-aged man	20.00%	60.00%
2	Elderly woman	0.00%	18.18%
3	Middle-aged man	27.27%	41.67%
3	Elderly woman	20.00%	30.00%
4	Middle-aged man	50.00%	25.00%
4	Elderly woman	20.00%	36.36%
5	Middle-aged man	20.00%	50.00%
5	Elderly woman	80.00%	10.00%
6	Middle-aged man	20.00%	20.00%
6	Elderly woman	40.00%	14.29%

Victim characteristics were coded as 0 (middle-aged man) and 1 (elderly woman); study location was also coded as 0 (online) and 1 (lab). Interaction terms were created with the three variables: version x victim, version x location, victim x location, and version x victim x location. A logistic regression was performed, using all variables and interactions; version and any interaction using version was entered into the logistic regression as a categorical variable<sup>1</sup>.

The abbreviated results of the logistic regression can be found in Table 3 below (complete results for all variables, including dummy variables, can be found in Appendix D). The overall model was significant,  $\chi^2 (df = 23) = 37.979, p = 0.026$ , McFadden's  $R^2 = 0.151$ , and correctly classified 74.8% of participants. Though none of the individual variables reached conventional levels of significance, the overall test of the three-way interaction term was significant,  $Wald (df = 5) = 11.90, p = 0.036$ .

<sup>1</sup> When a logistic regression predictor is entered as "categorical," SPSS creates a series of dummy codes for analysis. This means that for a variable with  $k$  categories, the number of dummy variables, and corresponding beta coefficients, is  $k - 1$ .

Table 3. Overall Effects of Location, Version, and Victim on Verdicts.

<i>Predictor</i>	<i>B</i>	<i>SE</i>	<i>Wald</i>
Location	0.000	1.118	0.000
Overall Version			3.178
Victim	0.981	1.021	0.924
Overall Location X Version			7.520
Overall Victim X Version			9.169
Victim X Location	-1.386	1.683	0.678
Overall Victim X Location X Version			11.896*
Constant	-1.386	0.791	3.075 <sup>+</sup>

Notes. +,  $p < 0.10$ , \*  $p < 0.05$

### **Emotional Reactions**

The 10 negative items were combined to create a single negative composite. In addition, the 10 positive items were combined to create a positive composite. If the victim gender is having the desired effect, there should be an increase in negative emotion, meaning a significant difference between victim conditions on the negative composite. Because positive emotion is considered to be independent of negative emotion (Watson, Clark, & Tellegen, 1988), and participants in either condition should experience little to no positive emotion, there should be no difference on the positive composite.

A 2 (victim) x 6 (version) x 2 (location) ANOVA was performed for the negative affect composite. There was a significant effect of victim,  $F(1,218) = 8.801, p = 0.003$ . Participants reported experiencing more negative affect when the victim was an elderly woman ( $M = 16.3, SD = 5.48$ ) than when the victim was a middle-aged man ( $M = 14.3, SD = 4.4$ ). In addition, there was a significant interaction between location and version,  $F(5,218) = 2.265, p = 0.049$ . Additional simple effects testing revealed that for

participants completing the study online, there were no differences in reported negative affect by version,  $F(5, 115) = 1.056, p = 0.389$ . For participants completing the study in the lab, there was a significant effect of version,  $F(5, 115) = 2.879, p = 0.017$ ; participants receiving version 1 ( $M = 13.0, SD = 3.295$ ) experienced significantly less negative affect than participants receiving version 4 ( $M = 18.11, SD = 6.54$ ). No other differences were significant.

The same  $2 \times 6 \times 2$  ANOVA was performed using the positive affect composite. There was a significant main effect of version,  $F(5, 218) = 2.291, p = 0.047$ ; post-hoc tests revealed that the only significant difference was between version 4 and version 5. No other effects were significant.

### **Pilot 1 Discussion**

The method in which one completed the study had an influence on study results. Specifically, method interacted with victim and the facts of the case to influence whether a participant chose to convict. It is unclear as to the reason for these effects. There are a variety of possible explanations for the differences, but none that can be tested with the present data. As one can see from the conviction rates in Table 1 above, in some cases, online participants were more likely to convict, but in other cases, they were less likely to convict. Without further research, it is difficult to infer why case information was viewed so differently online versus in the lab. It is possible that these differences were a product of this specific case or even the sample selected. It may be that participants who choose to complete a study online are qualitatively different from participants who choose to complete a study in the lab.



Whatever the reason for the differences, however, there were differences, which could mean that jury research performed online may find different results in the lab. Because lab settings are more similar to (though arguably still quite different from) actual jury trials than online settings, one could conclude that lab studies will provide more generalizable results. At the very least, these differences suggest that there are differences for this specific case tested with this specific sample. Therefore, since the next phase of this project will use the same case and sample source, additional pilot testing and the project itself will be performed in the lab.

Because a case of moderate strength was not found, additional pilot testing was necessary. Using the results of the first pilot test, version 4 was selected for additional work; this is because version 4 had the most consistent conviction rate across the two victims. Certain aspects of this case were strengthened. The victim's identification of the attacker was strengthened. In previous versions, the victim contacted his/her brother after the attack, and the brother asked if the attacker had been the defendant; instead, the victim clearly identified the defendant to his/her brother first. The second neighbor's identification was also strengthened.

## **Pilot 2 Methods**

### **Participants**

Participants were 24 undergraduates from the Loyola University Introductory Psychology participant pool. About 54 percent were women. All participants completed the study in the lab.

## Materials

**Trial transcript.** All participants received the strengthened version 4 transcript. In one half of the transcripts, the victim was the middle-aged man; in the other half, the victim was the elderly woman.

**Posttest.** The same posttests were used.

## Procedure

Participants followed the same procedure as lab participants in Pilot 1. Upon arriving at the lab, participants were randomly assigned to receive the middle-aged man version of the transcript, or the elderly woman version of the transcript.

## Pilot 2 Results

All analyses were conducted using SPSS Version 16.

## Verdicts

Percentages of guilty verdicts were computed for both victim conditions. When the victim was a middle-aged man, 41.7% of participants convicted; when the victim was an elderly woman, 58.3% of participants convicted. Both of these cases would be considered of moderate strength.

## Emotional Reactions

Once again, two composites were created, one negative and one positive. Differences were tested with two independent sample *t*-tests. No significant differences were found for either composite,  $t_{negative} (22) = -0.785, p = 0.456$  and  $t_{positive} (22) = -1.497, p = 0.149$ . Means and standard deviations are displayed in Table 4 below.

Table 4. Means (and Standard Deviations) of Affect by Condition.

<i>Victim</i>	<i>Negative Affect</i>	<i>Positive Affect</i>
Elderly woman	17.75 (6.384)	26.42 (4.699)
Middle-aged man	19.75 (6.538)	30.08 (7.064)

### **Pilot 2 Discussion**

By strengthening certain aspects of the case, two moderate versions of the trial were found. Unfortunately, this version yielded no significant difference between victim conditions on reported negative affect. Because it was expected that the effect of victim on conviction in the actual study would be mediated by negative affect, further pilot testing would be necessary to find a suitable combination of evidence. It was decided that for the present project, the victim variable would be dropped from the study. Future research should examine the present variables and relationships with different victims and different cases.

## CHAPTER SIX

### METHODS

#### Participants

Participants were 358 Loyola students, obtained through the Introductory Psychology participant pool. They were recruited for a study on decision making about court cases. Participants were predominantly freshmen, women, and Caucasian; only 2 percent had ever served on a jury before. See Table 5 below for complete information about the sample.

Table 5. Sample Demographic Information.

<i>Variable</i>		<i>M (SD)</i>	<i>Range</i>
Age		19.1 (1.72)	18 – 38
<i>Variable</i>		<i>Frequency</i>	<i>Percent</i>
Gender	Male	108	30.2%
	Female	249	69.6%
	Missing	1	0.3%
Race / Ethnicity	Caucasian	245	68.4%
	Hispanic/Latino	43	12.0%
	Asian-American	35	9.8%
	African-American/Black	13	3.6%
	Other	18	5.0%
	Prefer Not to Answer	4	1.1%
Year in School	Freshman	198	55.3%
	Sophomore	107	29.9%
	Junior	26	7.3%
	Senior	27	7.5%
Prior Jury Experience	No	347	96.9%
	Yes	7	2.0%
	Missing	4	1.1%

## Materials

### News Article

A news article was written about the case; this article was based on similar crime news articles (see Appendix E). Four versions of the article were created. All versions stated that an arrest had been made in a recent assault on Joseph Washington, and gave some background on the crime. The article then named the defendant, and gave some information on his upcoming court appearances. The neutral version did not contain any additional information; this version was received by 83 participants. The three PTP versions contained an additional line, between the statement about arrest and information on court appearances. This line said “According to a police source,” followed by one of three pieces of biasing information: 1) “the suspect has confessed to the crime” (received by 92 participants), 2) “the suspect did not cooperate during arrest” (received by 88 participants), and 3) “the suspect has been convicted of a similar crime in the past” (received by 95 participants).

### Questionnaire

A *voir dire* questionnaire was also created (see Appendix F). This questionnaire first gathered basic demographic information: age, gender, race, and whether the participant has served on a jury before. Next, the questionnaire contained 34 attitude questions, which were drawn from the Legal Attitudes of Prospective Jurors scale (Kravitz, Culter, & Brock, 1993) and General Attitudes Toward the Legal System scale (Schiffhauer & Wrightsman, 1995, both reprinted in Wrightsman, Batson, & Edkins, 2004). Thirty-two of the questions were received by all participants. The remaining two

questions varied. Participants who received biasing information in their news article received two attitude questions that dealt with the specific information they received; those receiving confession information received two confession questions, those receiving prior conviction information received two prior record questions, and those receiving information that the suspect did not cooperate received two questions on resisting arrest. Participants in the neutral group were randomly assigned to receive one of these three question pairs.

### **Trial Transcript**

All participants received the trial transcript from Pilot 2, with the middle-aged man as the victim (see Appendix G).

### **Jury Instructions**

Jury instructions were included at the end of the trial transcript (see Appendix H). Participants received one of three instructions; 120 received standard instructions. One hundred twenty-two received standard instructions that also warned jurors not to use information from the media (specific instructions). Finally, 116 participants received the specific instructions with the additional caution that media information can be inaccurate (strong instructions).

### **Posttest**

Finally, a two-question posttest was used to obtain participant verdicts, with the choices being guilty and not guilty, as well as a rating of guilt, ranging from 1 – definitely not guilty to 7 – definitely guilty (see Appendix I).

### **Procedure**

The study was conducted in the lab. Participants were greeted by the experimenter, and received the consent script (see Appendix J). Those who agreed to participate were given a packet containing all necessary materials. Participants were allowed to proceed through the packet at their own pace.

Participants first received a news article, which they were told is to introduce them to the case. After reading the article, the participant completed the questionnaire, which they were told is common procedure for potential jurors. Participants next read the abbreviated trial transcript about an assault on a middle-aged man followed by standard, specific, or strong instructions. Participants then selected a verdict and rated defendant guilt. After completing the materials, they returned the materials to the experimenter and were debriefed (see Appendix K).

## CHAPTER SEVEN

### RESULTS

All data were analyzed using R version 2.12.1 (R Development Core Team, 2010); in addition, the QuantPsyc (Fletcher, 2010) and psych (Revelle, 2010) R libraries were used. Six participants were dropped from analysis because they were missing one or both of the key attitude items that corresponded to PTP condition (confession, resisting arrest or prior record), leaving 353 participants. Of these participants, 121 responded to confession attitude items ( $n = 92$  receiving confession PTP,  $n = 29$  receiving neutral PTP), 115 responded to resisting arrest items ( $n = 87$  receiving resistance PTP,  $n = 28$  receiving neutral PTP), and 117 responded to prior record items ( $n = 92$  receiving prior record PTP,  $n = 25$  receiving neutral PTP).

The two confession attitude items were averaged together to create a confession composite; a resisting arrest composite and prior record composite were created in the same way. To prepare variables for analysis, attitude composites were centered by subtracting the mean, dummy variables were created for PTP and instructions, and interaction terms were created by multiplying variables together, in accordance with the recommendations of Aiken and West (1991). Means and standard deviations were performed on all variables under study (see Table 6 below). Additionally, correlations between predictor variables and the outcomes were performed (see Table 7 below).



Table 6. Means and Standard Deviations of Variables and Interaction Terms.

<i>Variable</i>	<i>n</i>	<i>Mean</i>	<i>SD</i>
<i>Attitude Items</i>			
Confession Attitude Item 1	121	2.88	1.06
Confession Attitude Item 2	121	2.42	0.96
Overall Confession Attitude	121	2.65	0.77
Overall Confession Attitude (Centered)	121	0.00	0.77
Resisting Arrest Attitude Item 1	115	2.25	1.07
Resisting Arrest Attitude Item 2	115	2.63	1.02
Overall Resisting Arrest Attitude	115	2.44	0.88
Overall Resisting Arrest Attitude (Centered)	115	0.00	0.88
Prior Record Attitude Item 1	117	3.49	1.09
Prior Record Attitude Item 2	117	3.27	0.93
Overall Prior Record Attitude	117	3.38	0.77
Overall Prior Record Attitude (Centered)	117	0.00	0.77
<i>Interaction Terms</i>			
Confession Attitude x PTP	121	-0.02	0.67
Confession Attitude x Specific Instructions	121	-0.03	0.46
Confession Attitude x Strong Instructions	121	-0.05	0.39
Confession Attitude x PTP x Specific Instructions	121	-0.05	0.41
Confession Attitude x PTP x Strong Instructions	121	-0.04	0.33
Resisting Arrest Attitude x PTP	115	-0.03	0.78
Resisting Arrest Attitude x Specific Instructions	115	0.05	0.51
Resisting Arrest Attitude x Strong Instructions	115	-0.04	0.40
Resisting Arrest Attitude x PTP x Specific Instructions	115	0.01	0.42
Resisting Arrest Attitude x PTP x Strong Instructions	115	-0.03	0.33
Prior Record Attitude x PTP	117	0.00	0.67
Prior Record Attitude x Specific Instructions	117	0.02	0.42
Prior Record Attitude x Strong Instructions	117	0.03	0.48
Prior Record Attitude x PTP x Specific Instructions	117	0.04	0.34
Prior Record Attitude x PTP x Strong Instructions	117	0.01	0.43
<i>Dependent Variables</i>			
Verdict	353	0.47	0.50
Guilt	353	4.81	1.20

*Notes.* Interaction terms use centered attitude composite variables.

Table 7. Correlations between Predictors and Outcome Variables

<i>Variable</i>	<i>Verdict</i>	<i>Guilt Rating</i>
<i>PTP</i>		
Confession	0.04	0.10 <sup>+</sup>
Resisting Arrest	-0.06	-0.01
Prior Record	0.03	0.03
<i>Instructions</i>		
Specific (vs. Standard)	0.00	-0.06
Strong (vs. Standard)	0.04	0.09 <sup>+</sup>
<i>Attitude</i>		
Confession	0.12	0.01
Resisting Arrest	0.15	0.17 <sup>+</sup>
Prior Record	0.34 <sup>**</sup>	0.32 <sup>**</sup>
<i>PTP X Instruction</i>		
Confession x Specific	0.03	0.00
Confession x Strong	0.08	0.18 <sup>*</sup>
Resist x Specific	0.06	0.02
Resist x Strong	-0.09 <sup>+</sup>	0.01
Prior x Specific	-0.01	0.00
Prior x Strong	0.03	0.01
<i>Attitude x PTP</i>		
Confession	0.10	0.05
Resisting Arrest	0.12	0.15 <sup>+</sup>
Prior	0.39 <sup>**</sup>	0.39 <sup>**</sup>
<i>Attitude x Instruction</i>		
Confession x Specific	0.21 <sup>*</sup>	0.05
Confession x Strong	-0.08	-0.06
Resist x Specific	0.01	-0.01
Resist x Strong	0.16 <sup>+</sup>	0.19 <sup>*</sup>
Prior x Specific	0.22 <sup>*</sup>	0.22 <sup>*</sup>
Prior x Strong	0.23 <sup>*</sup>	0.23 <sup>*</sup>
<i>PTP x Instructions x Attitude</i>		
Confession x Specific	0.15	0.06
Confession x Strong	-0.04	-0.06
Resist x Specific	0.05	0.07
Resist x Strong	0.06	0.07
Prior x Specific	0.18 <sup>+</sup>	0.23 <sup>*</sup>
Prior x Strong	0.25 <sup>*</sup>	0.24 <sup>*</sup>
<i>Dependent Variables</i>		
Verdict		0.61 <sup>**</sup>

Notes. Attitude variables are centered composites; +  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ .

### Regression Analyses on Guilt

Guilt data were analyzed with linear multiple regressions. One regression was performed to compare effects of the four types of PTP (confession, resisting arrest, prior record, and neutral) and three types of instructions (standard, specific, and strong) on guilt ratings. This model included three dummy variables to reflect PTP (with neutral PTP as the comparison group) and two dummy variables to reflect instructions (with standard instructions as the comparison group). The overall model was marginally significant,  $R^2 = 0.03$ ,  $F(5,347) = 1.89$ ,  $p = 0.10$  (see Table 8 for regression coefficients). One predictor was significant; participants receiving confession PTP assigned higher guilt ratings than participants receiving neutral PTP ( $\beta = 0.16$ ,  $t = 2.44$ ,  $p = 0.02$ ). Additionally, the effect of prior record PTP was marginally significant; participants receiving prior record PTP assigned higher guilt ratings than participants receiving neutral PTP ( $\beta = 0.11$ ,  $t = 1.69$ ,  $p = 0.09$ ).

Table 8. Predicting Guilt Rating with Type of PTP and Jury Instructions.

<i>Predictor</i>	<i>B</i>	<i>SE</i>	$\beta$	<i>t</i>
Constant	4.52	0.16		28.56 <sup>**</sup>
Confession PTP	0.44	0.18	0.16	2.44 <sup>*</sup>
Resisting Arrest PTP	0.23	0.18	0.08	1.22
Prior Record PTP	0.31	0.18	0.11	1.69 <sup>+</sup>
Specific Instructions	-0.06	0.16	-0.03	-0.40
Strong Instructions	0.20	0.16	0.08	1.27
$R^2$	0.03 <sup>+</sup>			
$F$	$F(5,347) = 1.89, p = 0.095$			

Notes. <sup>+</sup>  $p < 0.10$ , <sup>\*</sup>  $p < 0.05$ , <sup>\*\*</sup>  $p < 0.01$ .

Additionally, separate regressions were performed on each group to examine the effects of PTP, attitudes, and instructions, and their interactions. Three regressions

included participants who received confession PTP and answered confession attitude items, and the neutral participants responding to the confession attitude questions ( $n = 121$ ); three regressions included participants receiving resisting arrest PTP and attitude questions, and neutral participants responding to the resisting arrest items ( $n = 115$ ); two regressions included participants receiving prior record PTP and attitude questions, and their corresponding neutral participants ( $n = 117$ ).

### **Confession**

Three regressions were performed to test hypotheses pertaining to confession PTP, confession attitudes, and jury instructions. The first regression only included main effects. The overall model was not significant,  $R^2 = 0.05$ ,  $F(4, 116) = 1.53$ ,  $p = 0.20$  (see Table 9 for regression coefficients). One predictor was significant; participants receiving strong jury instructions assigned higher guilt ratings than participants receiving standard instructions ( $\beta = 0.23$ ,  $t = 2.13$ ,  $p = 0.04$ ).

The second model, in which interaction terms were added, also was not significant,  $R^2 = 0.10$ ,  $F(11, 109) = 1.14$ ,  $p = 0.34$  (see Table 9 for regression coefficients). The main effect of strong jury instructions was no longer significant; instead, the interaction between confession PTP and strong instructions was significant ( $\beta = 0.53$ ,  $t = 2.21$ ,  $p = 0.03$ ). As previously discussed, when higher-order interaction terms are not significant, Aiken and West (1991) recommend using a step-down procedure, in which the three-way interaction terms and any two-way terms not being used to test a hypothesis are dropped, and the regression re-performed.

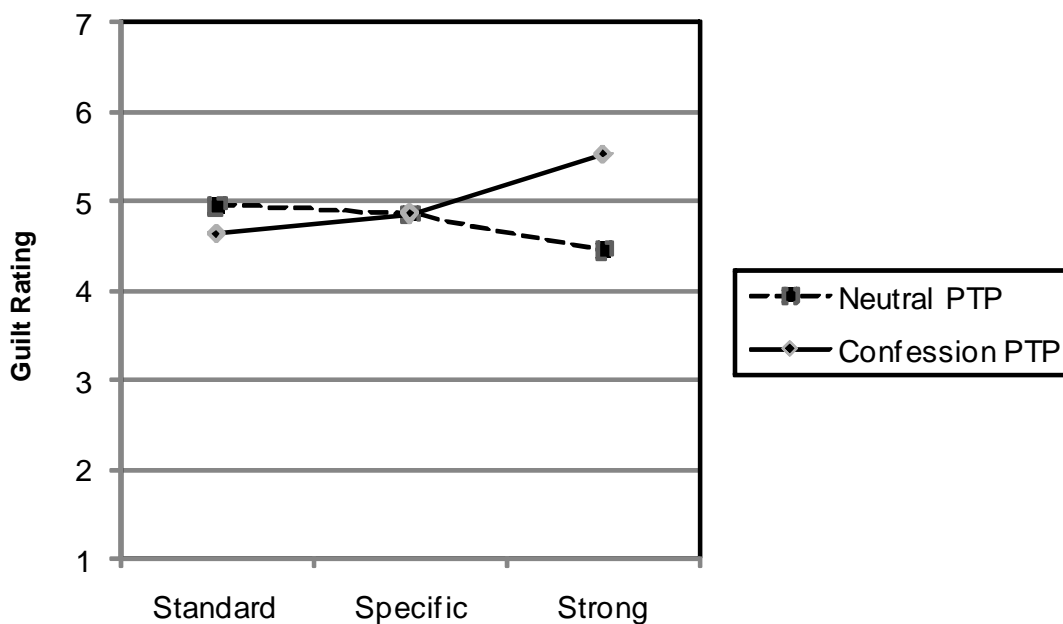
Table 9. Predicting Guilt Rating with Confession PTP, Attitude, and Instructions

Predictor	Model 1			Model 2			Model 3		
	B	SE	t	B	SE	t	B	SE	t
Constant	4.55	0.27	16.90**	4.98	0.38	13.27**	4.95	0.36	13.62**
PTP	0.22	0.26	0.86	-0.39	0.44	-0.89	-0.32	0.42	-0.75
Specific Instructions	0.11	0.27	0.69	-0.19	0.55	-0.34	-0.10	0.52	-0.19
Strong Instructions	0.58	0.27	2.13*	-0.55	0.59	-0.95	-0.50	0.57	-0.89
Attitude	0.08	0.15	0.56	-0.42	0.53	-0.80	-0.25	0.31	-0.81
PTP X Specific				0.46	0.64	0.72	0.33	0.61	0.54
PTP X Strong				1.46	0.66	2.21*	1.40	0.65	2.16*
PTP X Attitude				0.77	0.61	1.27	0.42	0.35	1.20
Attitude X Specific				0.47	0.78	0.60			
Attitude X Strong				0.09	0.75	0.12			
PTP X Attitude X Specific				-0.67	0.87	-0.77			
PTP X Attitude X Strong				-0.45	0.87	-0.52			
$R^2$			0.05			0.10			0.09
F			$F(4,116) = 1.53, p = 0.20$			$F(11,109) = 1.14, p = 0.34$			$F(7,113) = 1.68, p = 0.12$
$\Delta R^2$						0.05			0.04
$\Delta F$						$\Delta F(7,109) = 0.92, p = 0.49$			$\Delta F(4,113) = 1.82, p = 0.13$

Notes. PTP = Confession PTP (Received = 1, Neutral = 0); Attitude = Overall Confession Attitude (Centered); Specific = Specific Jury Instructions compared to Standard; Strong = Strong Jury Instructions compared to Standard; Model 2 adds all interaction terms; Model 3 includes only two-way terms used to test hypotheses; change in  $R^2$  and  $F$  compares the current model to Model 1; \*  $p < 0.05$ , \*\*  $p < 0.01$ .

A third regression was performed, dropping the two three-way interaction terms, and two attitudes x instructions terms. This model was also not significant,  $R^2 = 0.09$ ,  $F(7,113) = 1.68$ ,  $p = 0.12$  (see Table 9 for regression coefficients). Once again, the interaction between confession PTP and strong instructions was significant ( $\beta = 0.51$ ,  $t = 2.16$ ,  $p = 0.03$ ). To probe this effect, simple slopes were computed and tested using a web-based program by Preacher, Curran, and Bauer (2004a). Tests of the slopes showed that the difference between Confession PTP-Strong Instructions and Neutral PTP-Strong Instructions was significant, simple slope = 1.078,  $SE = 0.477$ ,  $t = 2.26$ ,  $p = 0.026$ . No other differences were significant. Predicted values are plotted below (see Figure 1).

Figure 5. Effect of Confession PTP and Instructions (Attitude = Mean).



### Resisting Arrest

Three regressions were performed to test hypotheses pertaining to resisting arrest PTP, resisting arrest attitudes, and jury instructions. The first regression only included

main effects. The overall model was not significant,  $R^2 = 0.05$ ,  $F(4,110) = 1.43$ ,  $p = 0.23$  (see Table 10 for regression coefficients). One predictor, attitudes, was significant; participants with stronger beliefs that resisting arrest indicates guilt assigned higher guilt ratings ( $\beta = 0.19$ ,  $t = 2.01$ ,  $p = 0.05$ ).

The second model, in which interaction terms were added, also was not significant,  $R^2 = 0.14$ ,  $F(11,103) = 1.57$ ,  $p = 0.12$  (see Table 10 for regression coefficients). The main effect of attitude was no longer significant, but the interaction between resisting arrest PTP and specific instructions was marginally significant ( $\beta = 0.47$ ,  $t = 1.91$ ,  $p = 0.06$ ). Because the higher order (three-way interaction) terms were not significant, these terms and two-way terms not being used to test hypotheses were dropped, and the regression was performed again.

The third model was marginally significant,  $R^2 = 0.10$ ,  $F(7,107) = 1.77$ ,  $p = 0.10$  (see Table 10 for regression coefficients). The main effect of specific instructions was significant ( $\beta = -0.54$ ,  $t = -2.35$ ,  $p = 0.02$ ). Overall, participants receiving specific jury instructions assigned lower guilt ratings than participants receiving standard instructions. This was qualified, however, by a significant interaction between resisting arrest PTP and specific instructions ( $\beta = 0.61$ ,  $t = 2.53$ ,  $p = 0.01$ ). To probe this effect, simple slopes were calculated and tested using a web-based program (Preacher, Curran, & Bauer, 2004a). The difference between Resisting Arrest PTP-Specific Instructions and Neutral PTP-Specific Instructions was significant, with participants receiving specific instructions and biasing PTP assigning higher guilt ratings than participants receiving specific instructions and neutral PTP, simple slope = 1.156,  $SE = 0.453$ ,  $t = 2.55$ ,

Table 10. Predicting Guilt Rating with Resisting Arrest PTP, Attitude, and Instructions.

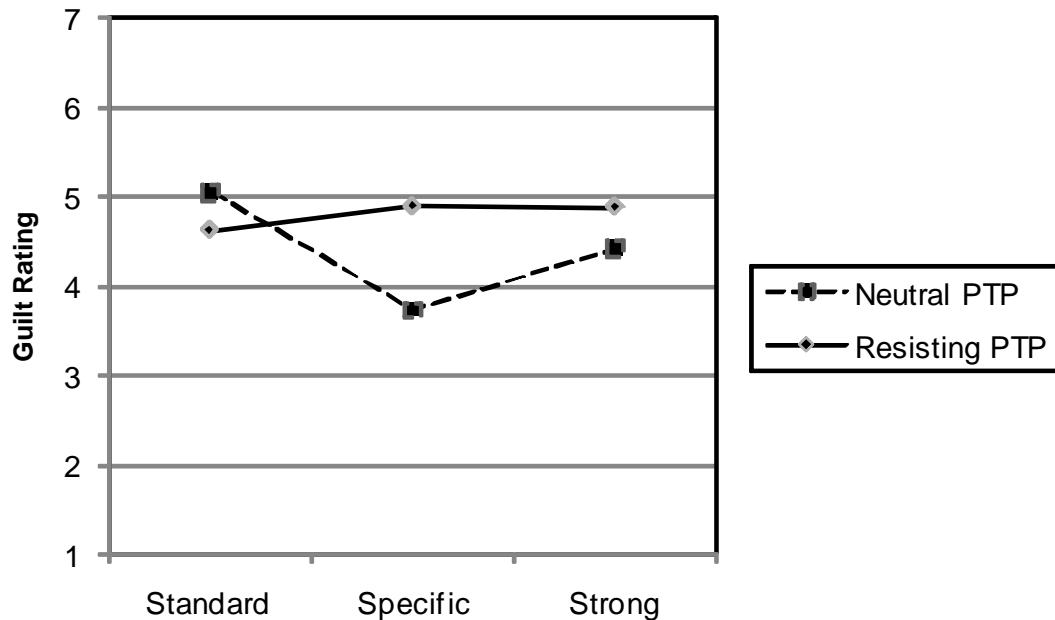
Predictor	Model 1			Model 2			Model 3		
	B	SE	t	B	SE	t	B	SE	t
Constant	4.43	0.26	16.80**	5.08	0.38	13.45**	5.05	0.38	13.48**
PTP	0.37	0.25	1.47	-0.46	0.43	-1.07	-0.43	0.43	-1.02
Specific	-0.05	0.26	-0.20	-0.96	0.58	-1.65 <sup>+</sup>	-1.31	0.56	-2.35*
Strong	0.06	0.26	0.23	-0.63	0.52	-1.23	-0.63	0.52	-1.23
Attitude	0.25	0.12	2.01*	0.71	0.60	1.20	0.47	0.27	1.72 <sup>+</sup>
PTP X Specific				1.23	0.65	1.91 <sup>+</sup>	1.59	0.63	2.53**
PTP X Strong				0.91	0.60	1.52	0.90	0.60	1.50
PTP X Attitude				-0.48	0.63	-0.77	-0.22	0.30	-0.77
Attitude X Specific				-0.93	0.73	-1.27			
Attitude X Strong				0.36	0.74	0.49			
PTP X Attitude X Specific				0.89	0.79	0.27			
PTP X Attitude X Strong				-0.28	0.83	-0.34			
$R^2$			0.05			0.14			0.10 <sup>+</sup>
F			$F(4,110) = 1.43, p = 0.23$			$F(11,103) = 1.57, p = 0.12$			$F(7,107) = 1.77, p = 0.10$
$\Delta R^2$						0.09			0.05 <sup>+</sup>
$\Delta F$						$F(7,103) = 1.62, p = 0.14$			$F(3,107) = 2.17, p = 0.10$

Notes. PTP = Resisting Arrest PTP (Received = 1, Neutral = 0); Attitude = Overall Resisting Arrest Attitude (Centered); Specific = Specific Jury Instructions compared to Standard; Strong = Strong Jury Instructions compared to Standard; Model 2 adds all interaction terms; Model 3 includes only two-way terms used to test hypotheses; change in  $R^2$  and  $F$  compares the current model to Model 1; \*  $p < 0.05$ , \*\*  $p < 0.01$ .



$p = 0.012$ . No other differences were significant. (Predicted values are plotted in Figure 2.)

Figure 6. Effect of Resisting Arrest PTP and Instructions (Attitude = Mean).



### Prior Record

Two regressions were performed to test hypotheses pertaining to prior record PTP, prior record attitudes, and jury instructions. The first regression only included main effects. The overall model was significant,  $R^2 = 0.14$ ,  $F(4,112) = 4.46$ ,  $p = 0.002$  (see Table 11 for regression coefficients). One predictor was significant; participants with stronger attitudes toward prior record information indicating guilt gave higher guilt ratings ( $\beta = 0.33$ ,  $t = 3.71$ ,  $p = 0.003$ ). Additionally, the effect of PTP was marginally significant, with participants receiving prior record PTP assigning higher guilt rating than participants receiving neutral PTP ( $\beta = 0.14$ ,  $t = 1.64$ ,  $p = 0.10$ ).

Table 11. Predicting Guilt Rating with Prior Record PTP, Attitude, and Instructions.

Predictor	Model 1			Model 2		
	B	SE	t	B	SE	t
Constant	4.62	0.28	16.56**	4.48	0.37	12.05**
PTP	0.43	0.27	1.64 <sup>+</sup>	0.62	0.43	1.45
Specific	-0.38	0.27	-1.42	-0.75	0.56	-1.34
Strong	-0.14	0.27	-0.51	0.46	0.56	0.83
Attitude	0.53	0.14	3.71**	-1.62	0.59	-2.72**
PTP X Specific				0.35	0.63	0.56
PTP X Strong				-0.76	0.63	-1.19
PTP X Attitude				2.37	0.66	3.62**
Attitude X Specific				1.56	0.74	2.10*
Attitude X Strong				1.80	0.761	2.37*
PTP X Attitude X Specific				-1.48	0.85	-1.74 <sup>+</sup>
PTP X Attitude X Strong				-1.86	0.85	-2.20*
$R^2$		0.14**			0.27**	
F		$F(4,112) = 4.46, p = 0.002$			$F(11,105) = 3.56, p = 0.0003$	
$\Delta R^2$					0.13	
$\Delta F$					$F(7,105) = 2.77, p = 0.01$	

Notes. PTP = Prior Record PTP (Received = 1, Neutral = 0); Attitude = Overall Prior Record Attitude (Centered); Specific = Specific Jury Instructions compared to Standard; Strong = Strong Jury Instructions compared to Standard; Model 2 adds all interaction terms; \*  $p < 0.05$ , \*\*  $p < 0.01$ .

The second model, in which interaction terms were added, was also significant,  $R^2 = 0.27$ ,  $F(11, 105) = 3.56$ ,  $p = 0.0003$  (see Table 11 for regression coefficients). The main effect of attitudes was still significant, but was in the opposite direction ( $\beta = -1.01$ ,  $t = -2.72$ ,  $p = 0.01$ ). This effect was qualified by several significant interactions, including a significant interaction between PTP and attitude, a significant three-way interaction with PTP, attitude, and strong jury instructions ( $\beta = -0.64$ ,  $t = -2.20$ ,  $p = 0.03$ ), and a marginally significant three-way interaction with PTP, attitude, and specific jury instructions ( $\beta = -0.41$ ,  $t = -1.74$ ,  $p = 0.09$ ).

To probe these interactions, simple slopes were calculated and tested using a web-based program for three-way interactions (Preacher, Curran, & Bauer, 2004b). Analyses show that participants who have a strong attitude that a prior record indicates guilt assign a higher guilt rating when they learned of a prior record in PTP, and assign a lower guilt rating when PTP was neutral, simple slope =  $-1.204$ ,  $SE = 0.626$ ,  $t = -1.922$ ,  $p = 0.057$ . On the other hand, participants who do not believe a prior record indicates guilt assign a lower guilt rating when PTP contains prior record and a higher guilt rating when PTP is neutral, simple slope =  $2.451$ ,  $SE = 0.698$ ,  $t = 3.513$ ,  $p = 0.0007$ . This effect is plotted in Figure 3.

This effect is qualified, however, by significant three-way interactions between PTP, attitude, and instructions. When standard instructions are used, attitude has a strong influence on the verdicts preferences of both participants receiving neutral and biasing PTP. When specific instructions are used, attitude no longer has an effect for those

receiving neutral PTP, but has the expected effect among those receiving prior record PTP. Finally, when strong instructions are used, attitude has a small effect on verdict. These results confirm that attitude interacts with PTP information, but only when standard instructions are used. Stronger instructions appear to reduce this effect. Predicted values for combinations of PTP (Priors or Neutral), Instructions (Specific or Strong), and Attitudes ( $-1 SD$  or  $+1 SD$ ) are plotted below; see Figure 4 for the interaction when specific instructions are used, and Figure 5 for the interaction when strong instructions are used.

Figure 7. Effect of Prior Record (Priors) PTP and Attitude (Instructions = Standard).

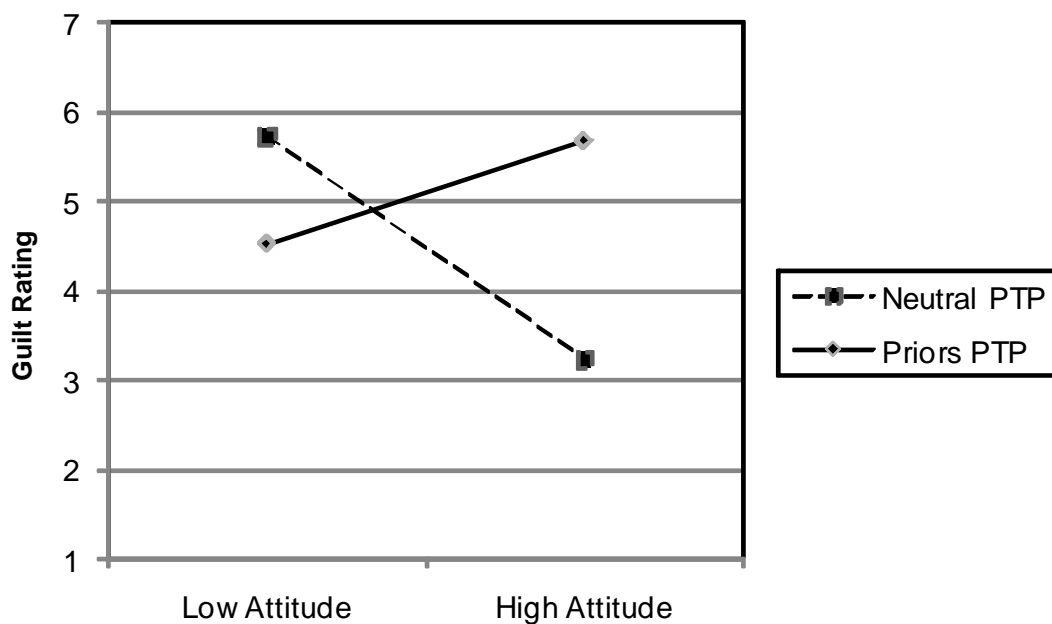


Figure 8. Effect of Prior Record (Priors) PTP and Attitude (Instructions = Specific).

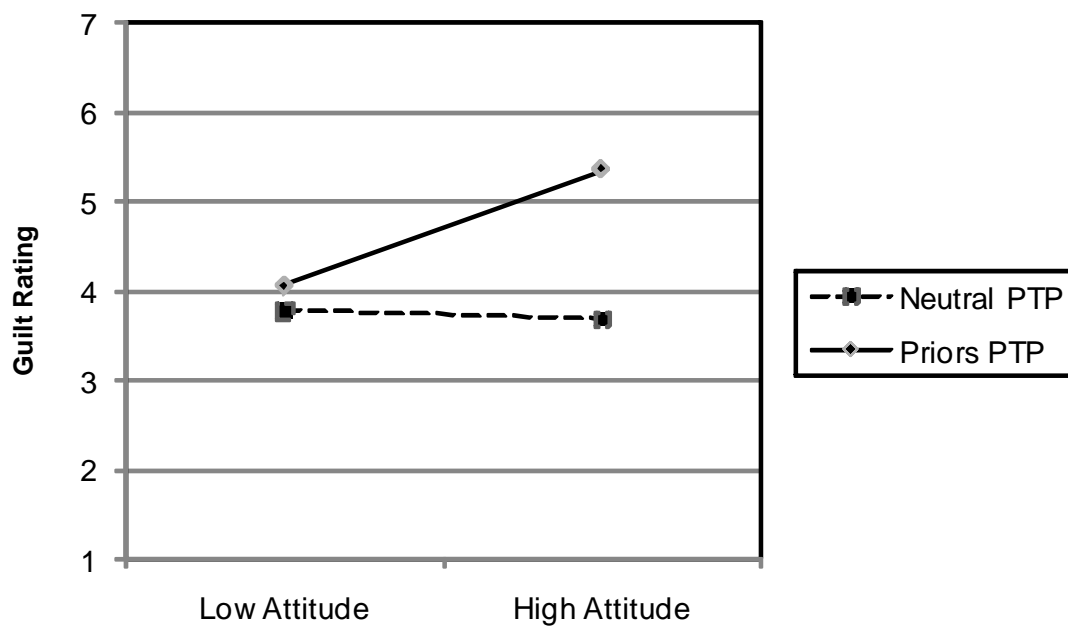
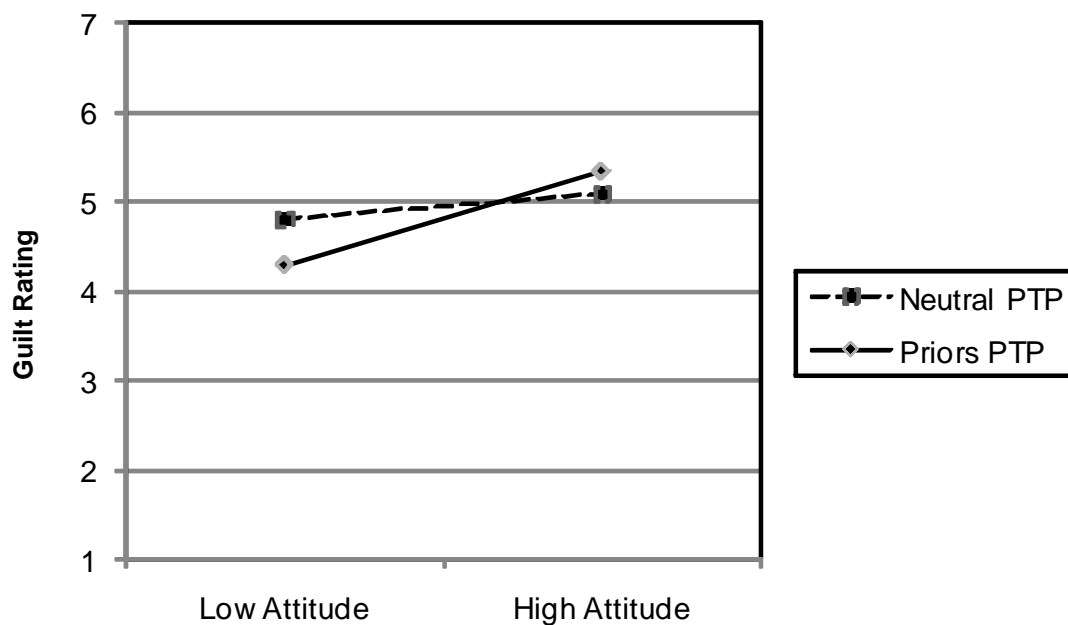


Figure 9. Effect of Prior Record (Priors) PTP and Attitude (Instructions = Strong).



### Logistic Regression Analyses on Verdict

Verdict data were analyzed in logistic multiple regressions. First, the overall effects of PTP and instructions were analyzed, with three dummy variables reflecting PTP (with neutral as the comparison group) and two dummy variable reflecting instructions (with standard as the comparison group). The model provided a poor fit for the data,  $\chi^2 (df = 347) = 485.54, p < 0.0001$ , correctly predicting 67.4 percent of verdicts, and yielded no significant predictors (see Table 12 for coefficients).

Table 12. Predicting Verdict with Type of PTP and Jury Instructions.

<i>Predictor</i>	<i>B</i>	<i>SE</i>	<i>Wald</i>
Constant	-0.26	0.266	-0.98
Confession PTP	0.14	0.305	0.47
Resisting Arrest PTP	-0.20	0.311	-0.64
Prior Record PTP	0.10	0.305	0.31
Specific Instructions	0.13	0.261	0.51
Strong Instructions	0.22	0.263	0.85

*Notes.* No predictors were significant.

Additional logistic regressions were performed to examine PTP, attitudes, and instructions: three regressions to include “confession” participants; three regressions including “resisting arrest” participants; and three regressions including “prior record” participants.

#### Confession

The first regression included the main effects of PTP, instructions, and attitude. Overall, the model provided a poor fit for the data,  $\chi^2 (df = 116) = 163.92, p = 0.0023$ , correctly predicting 80.2 percent of verdicts; none of the variables were significant predictors of verdict (see Table 13 for coefficients).

Table 13. Predicting Verdict with Confession PTP, Attitude, and Instructions.

<i>Predictor</i>	<u>Model 1</u>			<u>Model 2</u>			<u>Model 3</u>		
	<i>B</i>	<i>SE</i>	<i>Wald</i>	<i>B</i>	<i>SE</i>	<i>Wald</i>	<i>B</i>	<i>SE</i>	<i>Wald</i>
Constant	-0.45	0.46	-0.97	0.17	0.62	0.27	0.14	0.61	0.22
PTP	0.08	0.44	0.18	-0.77	0.74	-1.03	-0.73	0.73	-1.01
Specific	0.48	0.46	1.04	-0.90	1.14	-0.79	-0.19	0.88	-0.22
Strong	0.65	0.46	1.40	-1.08	1.07	-0.94	-0.59	0.97	-0.61
Attitude	0.40	0.25	1.57	0.07	0.89	0.08	0.29	0.53	0.54
PTP X Specific				1.75	1.28	1.37	0.95	1.04	0.91
PTP X Strong				1.97	1.20	1.64 <sup>+</sup>	1.64	1.11	1.47
PTP X Attitude				0.40	1.02	0.39	0.17	0.61	0.28
Attitude X Specific				3.89	2.64	-0.90			
Attitude X Strong				-1.23	1.36	-0.90			
PTP X Attitude X Specific				-3.52	2.74	-1.29			
PTP X Attitude X Strong				0.66	1.56	0.42			

*Notes.* PTP = Confession PTP (Received = 1, Neutral = 0); Attitude = Overall Confession Attitude (Centered); Specific = Specific Jury Instructions compared to Standard; Strong = Strong Jury Instructions compared to Standard; Model 2 adds all interaction terms; Model 3 includes only two-way terms used to test hypotheses; +  $p < 0.10$

In the second model, two- and three-way interactions were added. Once again, this model provided poor fit for the data,  $\chi^2 (df = 109) = 152.93, p = 0.004$ , predicting 87.6 percent of verdicts. The interaction between PTP and strong instructions was marginally significant; participants receiving PTP and strong instructions were over 3 times more likely to vote guilty than participants receiving neutral PTP and strong instructions (see Table 13 for coefficients). Because the overall model provided a poor fit, and neither of the three-way interactions was significant, three-way interactions and two-way interactions between attitude and instructions were removed, and the logistic regression was re-performed. This third model still provided poor fit for the data,  $\chi^2 (df = 113) = 161.65, p = 0.002$ , correctly predicting 82.6 percent of verdicts, and yielded no significant predictors (see Table 13 for regression coefficients).

## Resisting Arrest

The first logistic regression included only main effects. This model provided poor fit for the data,  $\chi^2$  ( $df = 110$ ) = 152.01,  $p = 0.005$ , predicting 70.4 percent of verdicts, and yielded no significant predictors (see Table 14 for regression coefficients).

Table 14. Predicting Verdict with Resisting Arrest PTP, Attitude, and Instructions.

<i>Predictor</i>	<u>Model 1</u>			<u>Model 2</u>			<u>Model 3</u>		
	<i>B</i>	<i>SE</i>	<i>Wald</i>	<i>B</i>	<i>SE</i>	<i>Wald</i>	<i>B</i>	<i>SE</i>	<i>Wald</i>
Constant	-0.49	0.48	-1.03	-0.15	0.70	-0.21	-0.15	0.69	-0.22
PTP	-0.02	0.45	-0.05	-0.46	0.80	-0.58	-0.45	0.79	-0.57
Specific	0.51	0.46	1.11	-0.38	1.07	-0.36	-1.01	1.11	-0.91
Strong	-0.03	0.48	-0.07	0.35	1.09	0.32	0.19	0.95	0.20
Attitude	0.31	0.22	1.41	0.79	1.12	0.70	0.71	0.55	1.30
PTP X Specific				1.25	1.20	1.05	1.88	1.23	1.53
PTP X Strong				-0.51	1.24	-0.42	-0.36	1.11	-0.32
PTP X Attitude				-0.45	1.18	-0.38	4163.00 <sup>2</sup>	0.60	-0.69
Attitude X Specific				-1.11	1.38	-0.80			
Attitude X Strong				1.49	1.76	0.85			
PTP X Attitude X Specific				0.98	1.49	0.66			
PTP X Attitude X Strong				-1.56	1.90	-0.82			

*Notes.* PTP = Resisting Arrest PTP (Received = 1, Neutral = 0); Attitude = Overall Resisting Arrest Attitude (Centered); Specific = Specific Jury Instructions compared to Standard; Strong = Strong Jury Instructions compared to Standard; Model 2 adds all interaction terms; Model 3 includes only two-way terms used to test hypotheses; no predictors were significant

The second model, which added interaction terms, also provided poor fit,  $\chi^2$  ( $df = 103$ ) = 144.41,  $p = 0.004$ , predicting 79.1 percent of verdicts; once again, there were no significant predictors (see Table 14 for regression coefficients). The third model, which dropped the three-way and attitude by instructions interaction terms, provided poor fit,  $\chi^2$  ( $df = 107$ ) = 147.97,  $p = 0.005$ , predicted 76.5 percent of verdicts, and yielded no significant predictors (see Table 14 for regression coefficients).

<sup>2</sup> According to Tabachnick & Fidell (2001), large parameter estimates and standard errors occur when there are too few cases to adequately test the number of predictors.



## Prior Record

The first logistic regression included only main effects. This model provided poor fit for the data,  $\chi^2 (df= 112) = 144.38, p = 0.021$ , predicting 83.8 percent of verdicts. One variable, attitude, was a significant predictor (coefficient = 1.05, *Wald test* = 3.575,  $p = 0.004$ ); individuals with a strong attitude (1 *SD* above the mean) are over 2 times more likely to vote guilty than individuals with a neutral (mean level) attitude and over 5 times more likely to vote guilty than individuals with a weak attitude (1 *SD* below the mean; see Table 15 for regression coefficients).

Table 15. Predicting Verdict with Prior Record PTP, Attitude, and Instructions.

<i>Predictor</i>	<u>Model 1</u>			<u>Model 2</u>			<u>Model 3</u>		
	<i>B</i>	<i>SE</i>	<i>Wald</i>	<i>B</i>	<i>SE</i>	<i>Wald</i>	<i>B</i>	<i>SE</i>	<i>Wald</i>
Constant	0.16	0.51	0.32	0.16	0.84	0.19	0.20	0.68	0.30
PTP	0.06	0.50	0.13	0.11	0.95	0.12	0.04	0.80	0.05
Specific	-0.77	0.50	-1.56	18.83	1499.00	-0.01	-2.40	1.33	-1.80 <sup>+</sup>
Strong	-0.02	0.50	-0.04	1.19	1.30	0.92	1.11	1.12	0.99
Attitude	1.05	0.29	3.58 <sup>**</sup>	-2.79	1.91	-1.46	-0.53	0.64	-0.83
PTP X Specific				18.17	1499.00	0.01	1.75	1.46	1.20
PTP X Strong				-1.48	1.43	-1.03	-1.37	1.27	-1.07
PTP X Attitude				4.46	2.04	2.18 <sup>*</sup>	2.02	0.74	2.72 <sup>**</sup>
Attitude X Specific				31.79	2419.08	0.01			
Attitude X Strong				2.17	2.17	1.00			
PTP X Attitude X Specific				-32.15	2419.08	-0.01			
PTP X Attitude X Strong				2.35	2.36	-0.99			

*Notes.* PTP = Prior Record PTP (Received = 1, Neutral = 0); Attitude = Overall Prior Record Attitude (Centered); Specific = Specific Jury Instructions compared to Standard; Strong = Strong Jury Instructions compared to Standard; Model 2 adds all interaction terms; Model 3 includes only two-way terms used to test hypotheses; +  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$

The second model, which added interaction terms, provided acceptable fit,  $\chi^2 (df = 105) = 127.60, p = 0.066$ , and predicted 85.5 percent of verdicts; additionally, the interaction between attitude and PTP was significant (see Table 15 for regression coefficients). Though this model provided acceptable fit for the data, dropping the non-

significant three-way interaction and attitude x instructions interactions would provide more power.

The third model, which dropped the three-way and attitude by instructions interaction terms, also provided acceptable fit,  $\chi^2 (df = 109) = 133.39, p = 0.056$ , and predicted 87.2 percent of verdicts. Additionally, the interaction between attitude and PTP was still significant (see Table 15 for regression coefficients). This effect was probed using the web-based program for two-way interactions (Preacher, Curran, and Bauer, 2004a). Though none of the simple slopes were significant, the following trends were observed. Participants with a weak attitude (1 *SD* below the mean) who received neutral PTP were 4.5 times more likely to vote guilty than participants with low attitude who received prior record PTP, simple slope = -1.513, *SE* = 0.962, *t* = -1.572, *p* = 0.119, *OR* = 4.54. On the other hand, the effect was reversed for participants with strong attitude (1 *SD* above the mean); participants who received prior record PTP were almost 5 times more likely to vote guilty than participants with a strong attitude who received neutral PTP, simple slope = 1.600, *SE* = 1.012, *t* = 1.580, *p* = 0.117, *OR* = 4.95.

### Overall Results

Because this study examined so many variable, some clarification of study findings, and how they align with hypotheses is necessary. Table 16 below details for each hypothesized effect what was found and a brief interpretation of the finding. As the table shows, more significant effects were found for guilt rating than verdict. Attitude only showed a significant interaction with prior record PTP, but was in the expected

direction. Additionally, instructions weakened the effects of resisting arrest PTP and prior record PTP, but actually strengthened the effect of confession PTP.

Table 16. Chart of Hypothesized Effects and Significant Results.

<b>Hypothesis</b>	<b>Effect on Guilt Rating?</b>	<b>Effect on Verdict?</b>
<b>Effect of PTP Information</b>		
Confession	Confession PTP resulted in higher guilt ratings	None
Prior Record	Prior record PTP results in marginally higher guilt ratings	None
Resisting Arrest	None	None
<b>PTP x Attitude Interaction</b>		
Confession	None	None
Prior Record	Strong attitude increased effect of PTP	Strong attitude increased effect of PTP
Resisting Arrest	None	None
<b>PTP x Admonition Interaction</b>		
Confession	Strong instructions strengthened effect of PTP – Psychological Reactance	Similar to guilt rating effect but marginally significant
Prior Record	None	None
Resisting Arrest	Specific instructions strengthened effect of PTP – Psychological Reactance, Ironic Process, or Thought Suppression	None
<b>PTP x Attitude x Admonition Interaction</b>		
Confession	None	None
Prior Record	Specific and strong instructions weakened effect of PTP and attitude	None
Resisting Arrest	None	None

### **Exploratory Analyses**

Though some hypotheses were supported, only small proportions of total variance in guilt ratings and verdicts were explained. Other variables, including participant attitudes not examined in previous analyses, may explain these outcomes. According to Finkel (1995), individual attitudes and knowledge influence legal decisions and provide basic guidelines on the weighting of specific case facts. Though it was expected that attitudes directly relevant to PTP information would determine whether PTP is used in

determining guilt, other attitudes relevant to case evidence and information may also determine guilt. Therefore, exploratory analyses using other attitude variables relevant to the specific facts of the case were conducted.

In addition to the attitude questions relevant to the PTP information received, participants provided a variety of attitudes that may be relevant to the specific case, including whether a defendant should be required to testify, and whether illegal or circumstantial evidence should be used in court. Furthermore, it has been argued that simply learning that a person has been arrested for a crime may be biasing and may lead people to believe the arrestee is guilty (Bruschke & Loges, 1999); as with PTP information, however, learning that a person has been arrested would only be biasing if an individual believes that police do not make mistakes and only arrest guilty people. Therefore, attitudes about police or defendant behavior during arrest would also be relevant to the current case, where all participants (including those in the neutral PTP group) received a statement about the arrest prior to reading the court transcript.

Multiple linear regression was performed, regressing guilt rating onto attitude questions about evidence, police behavior, and suspect behavior. Overall, the model was significant,  $R^2 = 0.07$ ,  $F(7,343) = 3.42$ ,  $p = 0.002$  (see Table 17 for regression coefficients). One predictor was significant; participants who more strongly believed that police should be able to arrest people who “look suspicious” gave higher guilt ratings,  $\beta = 0.15$ ,  $t = 2.77$ ,  $p = 0.006$ . Additionally, participants who more strongly believed that illegal evidence should be allowed in court gave marginally higher guilt ratings,  $\beta = 0.10$ ,  $t = 1.81$ ,  $p = 0.07$ .

These variables were also used in a logistic regression with verdict. Though overall the model provided a poor fit for the data,  $\chi^2 (df = 343) = 440.38, p = 0.0003$ , predicting only 55 percent of verdicts, three predictors were significant (see Table 17 for regression coefficients). As with guilt rating, a belief that police should be able to arrest “suspicious” people increased guilty verdicts. Participants who more strongly believed circumstantial evidence should never be used to convict were 1.5 times less likely to find the defendant guilty than participants with a neutral attitude. Finally, participants who strongly believed upstanding citizens have nothing to fear from the police were 1.3 times more likely to find the defendant guilty than participants with a neutral attitude.

Table 17. Regressions Using Evidence, Police Behavior, and Suspect Behavior Attitudes.

<i>Predictor</i>	DV: Guilt Rating				DV: Verdict		
	<i>B</i>	<i>SE</i>	$\beta$	<i>t</i>	<i>B</i>	<i>SE</i>	<i>Wald</i>
Constant	3.96	0.57		6.90	-0.94	1.04	-0.90
Allow Illegal Evidence	0.11	0.06	0.10	1.81 <sup>+</sup>	0.14	0.11	1.35
Allow Circumstantial Evidence	-0.09	0.07	-0.07	-1.33	-0.45	0.13	-3.50 <sup>**</sup>
Defendant Required to Testify	0.01	0.07	0.01	0.17	0.11	0.13	0.87
Arrest Suspicious People	0.18	0.07	0.15	2.77 <sup>**</sup>	0.37	0.12	3.03 <sup>**</sup>
Upstanding Citizens	0.09	0.06	0.08	1.40	0.26	0.11	2.38 <sup>*</sup>
Police Overzealous	-0.07	0.07	-0.05	-0.93	0.07	0.13	0.54
Police Abuse Power	0.08	0.09	0.05	0.96	-0.14	0.16	-0.86

Notes. <sup>+</sup>  $p < 0.10$ , <sup>\*</sup>  $p < 0.05$ , <sup>\*\*</sup>  $p < 0.01$ .

Another finding that should be further examined is explanations for verdict selections and how verdict relates to guilt rating. Though guilt rating and verdict were strongly correlated ( $r = 0.61$ ), variables that significantly predicted guilt did not also predict verdict, and vice versa. The guilt rating ranged from 1 to 7, with the endpoints “Definitely Not Guilty” and “Definitely Guilty,” respectively. One would expect then,

that participants selecting “7” on the guilt rating would also select “guilty”; the table below (Table 18) shows that this is not the case.

Table 18. Frequencies (and Percentages) of Guilt Rating by Verdict.

<i>Guilt Rating</i>	<i>Not Guilty</i>	<i>Guilty</i>	<i>Total</i>
1	0 (0.0%)	0 (0.0%)	0
2	13 (100.0%)	0 (0.0%)	13
3	53 (98.1%)	1 (1.9%)	54
4	43 (93.5%)	3 (6.5%)	46
5	57 (47.1%)	64 (52.9%)	121
6	21 (18.9%)	90 (81.1%)	111
7	1 (12.5%)	7 (87.5%)	8
<i>Total</i>	188 (53.3%)	165 (46.7%)	353

One possibility is that, as Finkel (1995) suggests, attitudes change the way one considers the evidence, and alters the threshold one sets for guilt. Additionally, participants who believe that the defendant is definitely or probably guilty, but still select a not guilty verdict, may be responding to their own beliefs about justice, such as a belief that it is better to free a guilty defendant than convict an innocent defendant. To test these hypotheses, two additional logistic regressions were performed, regressing verdict onto guilt rating, attitudes relating to justice issues (such as beliefs about the danger of convicting the innocent and the importance of liberty), and attitudes relating to the “threshold” for guilt (such as what to do if a person is “obviously guilty” or the meaning of “reasonable doubt”). The first model provided good fit for the data,  $\chi^2 (df = 344) = 307.98, p = 0.92$ , correctly predicting 73.5 percent of verdicts; two predictors were significant (see Table 19). Guilt rating was a strong predictor of verdict. Additionally, participants who strongly believe that too many obviously guilty are acquitted were more likely to vote guilty.

Table 19. Predicting Guilt with Criminal Justice Attitudes.

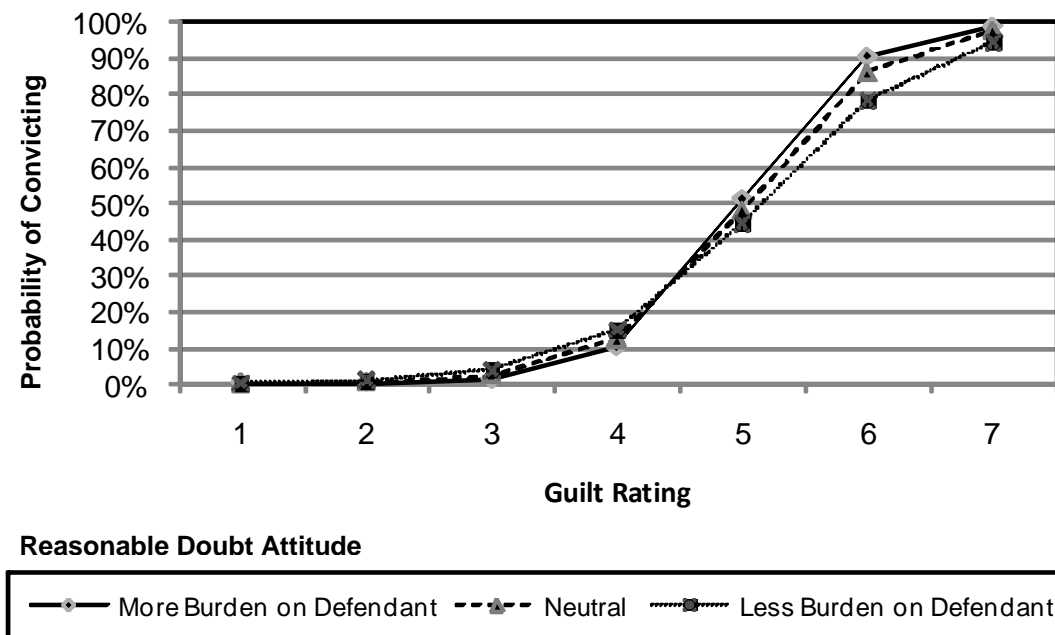
<i>Predictor</i>	<u>Model 1</u>			<u>Model 2</u>		
	<i>B</i>	<i>SE</i>	<i>Wald</i>	<i>B</i>	<i>SE</i>	<i>Wald</i>
Constant	-0.46	0.16	-2.93	-0.45	0.16	-2.78**
Obviously Guilty	0.37	0.16	2.29*	0.29	0.18	1.64 <sup>+</sup>
Reasonable Doubt	-0.17	0.14	-1.22	-0.06	0.15	-0.38
Free Guilty v. Convict Innocent	-0.17	0.14	-1.23	-0.17	0.16	-1.07
Meaning of True Liberty	-0.01	0.16	-0.06	0.03	0.17	0.16
Juries Use Evidence Only	0.07	0.14	0.48	-0.01	0.16	-0.1
Guilt Rating	1.81	0.20	8.96**	1.89	0.22	8.65**
ObGuilt x Guilt Rating				0.15	0.22	0.67
ReasDoubt x Guilt Rating				-0.32	0.18	-1.81 <sup>+</sup>
FreeGuilt x Guilt Rating				0.03	0.18	0.14
Liberty x Guilt Rating				-0.17	0.19	-0.88
JurEvidence x Guilt Rating				0.16	0.19	0.82

Notes. +  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ .

In the second model, interactions were created between each attitude and guilt rating. Overall, the model provided good fit for the data,  $\chi^2 (df = 339) = 301.01, p = 0.93$ , predicting 74.1 percent of verdicts. Guilt remained a significant predictor and attitude toward “obviously guilty” was marginally significant (see Table 19). Additionally, the interaction between “reasonable doubt” attitude and guilt rating was marginally significant. Post-hoc probing (using Preacher, Curran, & Bauer, 2004a) revealed that, though few groups convicted when their guilt rating was low, participants who placed more burden of proof on the defendant (to prove innocent beyond a reasonable doubt) convicted at a lower guilt rating than participants who placed less burden on the defendant (see Figure 5 below for predicted values). For example, an individual who assigned a guilt rating of 6, and who places more burden of proof on the defendant, has a 91.1 percent change of convicting, while an individual assigning a guilt rating of 6 who

does not expect the defendant to prove innocence beyond a reasonable doubt has only a 77.9 percent chance of convicting.

Figure 10. Effect of Guilt Rating and Reasonable Doubt Attitude.





## CHAPTER EIGHT

### DISCUSSION

The current study both advances our understanding of PTP effects and raises new questions for further research. It was hypothesized that PTP information would only bias participants who had strong beliefs that such information indicated guilt, and that jury instructions would affect participants differently depending on their attitude and PTP information received. This hypothesis was supported in part, but only among participants receiving prior record PTP. That is, when participants did not believe a prior record indicated guilt, receiving PTP information and standard jury instructions resulted in a lower guilt rating, while among participants with a strong belief that a prior record indicated guilt, receipt of PTP information and standard jury instructions increased guilt rating. More specific jury instructions made PTP participants provide guilt ratings that were more similar to neutral participants. The effect of these variables on dichotomous verdicts was slightly different, but it was found that participants with a strong attitude toward prior record were more likely to vote guilty in the presence of PTP and participants who did not believe prior record indicated guilt were less likely to vote guilty in the presence of PTP.

These patterns differ slightly from what was hypothesized. Instructions were expected to affect verdicts among participants with weaker attitudes, and not participants with strong attitudes. Instead, specific and strong instructions served to weaken the

effects of PTP among both participants with weak and strong attitudes, making their guilt ratings similar to those receiving neutral PTP. Because there did not appear to be a different pattern for low and high attitude participants, belief perseverance is not a good explanation for these results. Additionally, psychological reactivity is also probably not occurring, because if it were, stronger instructions would have increased guilt ratings.

Among the other two PTP groups, confession, and resisting arrest, no three-way interactions were observed, but some significant effects were uncovered. Knowledge of a confession resulted in higher guilt ratings, though this information alone was not enough to affect dichotomous verdicts. Additionally, jury instructions affected guilt rating among participants receiving confession or resisting arrest PTP, but not in the same way as it did among participants receiving prior record PTP. That is, instructions that specifically addressed PTP led to increases in guilt ratings, and also marginally increased guilty verdicts when confession PTP was used. Once again, because attitudes did not play a role, belief perseverance is probably not the culprit. Thought suppression or ironic processes may still explain these results, but psychological reactivity may also be a possibility, since specific instructions to ignore such information strengthened their effects. Finally, when resisting arrest PTP was used, participants with a stronger attitude that resisting arrest implied guilt assigned higher guilt ratings; this effect was not reduced by jury instructions to ignore outside information, and specific instructions strengthened the effect of PTP on guilt rating.

Many PTP researchers have hypothesized that certain PTP content will be more biasing than other content. Otto, Penrod, and Dexter (1994), for instance, compared

different types of PTP information, including information on the defendant's negative character, weak inadmissible evidence, strong inadmissible evidence, and information on the defendant's socioeconomic status, and found that negative character information had the strongest effect on verdict. Additionally, studies have shown that participants receiving confession information (DeLuca, 1979) and prior record information (Hvistendahl, 1979) show more bias toward guilt than participants receiving neutral information. It may be that some information is inherently more biasing than other information. Additionally, individuals may respond differently to instructions to ignore certain pieces of information, showing the ability to ignore some information when asked and, at other times, being more likely to use certain information even when it should be ignored. More research is needed to explore this possibility. One method that has not been used would be to have participants read more than one type of PTP content and rate how damaging certain information is comparatively.

Variation in attitudes of participants may also explain why certain information appears more biasing. Though participants were randomly assigned to PTP and jury instruction conditions, attitudes were measured after these assignments had already been made. Measuring attitudes beforehand, and stratifying assignment to groups based on attitude would guarantee that participants with low, mean, and high attitudes were distributed equally across conditions. This method may make interactions with attitude easier to detect. Future research should include this additional step, which would help to illuminate whether certain information is more biasing or whether participant attitudes explain these different results.

Despite the fact that key hypotheses were not supported, this study provides more evidence for the biasing effects of PTP. It is important to keep in mind, however, that PTP is not always biasing, and that some studies do not find PTP effects, or may even find the opposite effect – that PTP reduces bias toward guilt. Type of case and amount of PTP may explain differences in results compared to other studies of PTP. As previously discussed, most PTP studies use homicide or sexual assault cases, which use different types of evidence; these cases may also be considered more sensational by the public and may garner more attention, both in the amount of media coverage of the event as well as attention to the media information by consumers. Additionally, this study used one article with only one sentence that introduced the biasing information; many other studies of PTP either use more articles or more biasing information within a single article. Brusckie and Loges (1999) found, in an analysis of 134 federal first-degree murder cases that about 46 percent received no publicity, and 50 percent received between 1 and 31 articles. Though the number of articles can range much higher than that, with one case in their study being covered in 141 articles, studies on cases receiving small to moderate amounts of PTP may provide results that are more applicable to real cases.

According to Finkel (1995), however, different results may be due to more than crime of which the defendant was accused or amount of PTP. He argues that the story potential jurors form to explain the events, and their interpretations of individual case facts, will determine the outcome, meaning effects would be expected to vary for different individual cases. Additionally, the information used to generate a story can

come from a variety of places, including pre-existing attitudes, case facts heard in court, and extralegal information from sources like PTP. Even if jurors do not come into the courtroom with a pre-developed story, they may use information heard beforehand to shape the story they develop while in court to create a cohesive account of the events and to attribute blame.

It is difficult to determine how PTP information was used, however, because participants did not respond to any questions about trial evidence or how they arrived at their verdict. This information may provide insight into how, and if, participants used outside information, such as PTP or attitudes, to understand the case and arrive at an outcome. Because participants were already completing an attitude questionnaire and reading a trial transcript, a measure of responses to evidence was not included in the present study, to reduce the possibility of participant fatigue. One way to reduce this fatigue would have been to expose participants to the PTP and/or given the attitude measure at a different time. Researchers should consider including measures of reactions to the trial, either through an interview or standardized questionnaire, and may also wish to conduct studies over two or more sessions, to lessen the chance for fatigue and more accurately reflect real-life exposure to PTP.

When a meta-analysis exists on a topic, many researchers (e.g., Chrzanowski, 2006) will compare their results to those obtained by the meta-analysis. This can help to demonstrate whether current findings are in-line with past research on a topic. In the meta-analysis (Locatelli, Fulero, & Kluwe, 2011), I found that effects of PTP obtained after presentation of trial evidence were significantly smaller than effects of PTP obtained

prior to presentation of trial evidence. Participants in the current study did not provide verdict preferences prior to presentation of the trial, but it would be expected that effects of PTP would have been stronger prior to trial presentation because at that point in time, participants have less information upon which to base their decisions. Additionally, posttrial effects, though smaller than pretrial effects, were still significant. The meta-analysis, however, was unable to examine the influence of individual difference variables that may explain why some studies find significant effects of PTP while others do not. The current study offers some evidence that individual attitudes can explain variability in study findings.

The meta-analysis also showed an effect of crime, with studies using either sex offenses or non-violent crimes finding smaller effects, and studies using homicides or other violent crimes finding larger effects. As stated previously, however, few studies included in the meta-analysis used an assault trial, as was used in the current study. This type of crime is less sensational than a homicide trial or certain other violent crimes, and verdicts in less sensational cases may be less affected by PTP. Finally, the meta-analysis did not find any significant change in PTP effects when a judicial admonition was used, but variations in wording, strength, and method of presentation of the admonition used in different studies may also have an effect.

In addition to this study's findings on PTP, the current study also provides some evidence that attitudes may explain different thresholds for guilt. Specifically, participant attitudes on the meaning of "reasonable doubt" influenced how certain the participant needed to be of guilt before convicting. These findings are only exploratory, however, so

additional research is needed to verify these findings. Such studies could use similar methodologies as studies of commonsense justice. These studies begin with a particular case, and manipulate different case facts to determine their effect on the outcome. For example, Finkel (1995) discusses felony murder, a charge allowed in certain US states. In felony murder cases, the defendant can be charged with the murder of anyone who died while the defendant was committing a crime, whether the murder was caused by the defendant (such as the defendant shot and killed a bank teller while robbing the bank) or was caused by something else (the bank teller had a heart attack while the defendant was robbing the bank). Though in states that allow felony murder charges, jurors would be expected to charge the defendant with murder even in the “heart attack” scenario, Finkel found that manipulating how responsible the defendant was for the victim’s death affected juror verdicts. Researchers may be able to explore jurors’ threshold for guilt by using a similar method as Finkel and other commonsense justice researchers, while including a measure of relevant attitudes and a continuous guilt rating in addition to verdict. This would help to determine whether different thresholds are the result of case information, attitudes, or some combination of both.

### **Study Limitations**

Many of the results that were expected to be significant were not found in the current study. One possibility is lack of power. Power analysis was conducted on the smallest expected effects for a continuous dependent variable, but not for a dichotomous dependent variable, meaning the logistic regression analyses may have been underpowered. Another possibility is that the theories used in the current study failed to

correctly predict results. PTP research has been criticized before as atheoretical (Bruschke & Loges, 2004), and only recently have researchers begun to use theory to inform hypotheses. Because attitude theory has been used in other studies of juror decision-making (Devine et al., 2001), it seemed only natural to include attitude theory in PTP research. Obviously, additional research is needed to generate or modify theory so that it can properly predict PTP effects.

Attitude theory could also be more fully tested in the context of PTP research by including additional questions on responses to the case and its evidence; both Chrzanowski (2006) and Honess, Charman, and Levi (2003) included such questions in their studies. For instance, the expectancy-value model was utilized in the current study to predict how pre-existing attitudes would interact with PTP information. This model states, however, that as more information is obtained about an attitude target, in this case, the defendant, that information will be evaluated (in terms of good-bad, for instance) and a strength will be assigned to that belief. Information about the attitude target includes not only PTP, but the case evidence. Asking jurors to respond to attitude questions about different pieces of evidence, or having them respond to open-ended questions about how they arrived at their verdict, would allow us to explore how attitudes about the defendant were formed, either based on previous attitudes or entirely new attitudes formed on-line, and how they combined to determine verdict preference. Some research suggests that attitudes formed on-line are stronger than memory-based attitudes (Bizer, Tormala, Rucker, & Petty, 2006), meaning the attitudes participants formed while examining case evidence may have a stronger influence on verdict preference than pre-existing attitudes.



Similarly, another limitation to the current study is that it did not include any questions that would relate to the victim or his behavior. In this particular case, the victim's eyewitness identification was questioned because he had not been wearing his glasses at the time of the attack. Participant attitudes toward eyewitness testimony and its accuracy may explain why some participants chose to convict and others did not. Similarly, little information was included about the victim in the PTP, but in some cases, PTP may routinely contain information about the victim. For example, in sexual assault cases, the media is generally free to publish information about the victim (Benedict, 1993), such as past sexual history, that would typically be inadmissible in court (National Center for Prosecution of Child Abuse, 2010). Few studies have examined how victim information presented in the media influences verdicts.

One exception is Chrzanowski (2006), who studied a sexual assault case in California, and compared a non-experimental group that had been exposed to PTP on their own to an experimental group assigned to view PTP. Non-experimental participants completed questionnaires to determine how much information they recalled viewing and whether that information was negative toward the defendants, neutral, or negative toward the victim. The experimental group of participants, from a different location, was randomly assigned to view PTP information that was negative toward the defendants, neutral, or negative toward the victim. Participants who were exposed to PTP that was negative toward the victim provided higher ratings of innocence than participants exposed to PTP that was negative toward the defendants. The effect was stronger in the non-experimental group.

No manipulation checks were performed to confirm whether participants attended to and understood the pretrial publicity or jury instructions. Though many studies on PTP do not include such manipulation checks, additional questions to probe participant memory or comprehension of this information may help to explain results obtained, including nonsignificant results. Another limitation is that, in order to fully test hypotheses in the current design, many statistical tests were performed. Most of these tests were planned and specific hypotheses were generated beforehand, but because alpha-rates were not adjusted, some significant results, particularly the unexpected significant results, could have been obtained merely by chance alone. Additionally, the unplanned exploratory analyses may have also capitalized on chance and inflated experiment-wise error. Additional research is needed to confirm these results.

The current study was also conducted using undergraduate participants completing the study on their own in a lab setting. Because few studies have examined the effect of specific attitudes and their interaction with PTP information and jury instructions, this study was considered a necessary first step to determine if larger, more realistic studies are necessary. The obvious next step would be to use participants drawn from the community or jury records, and to use more realistic trial materials, such as a recorded trial or mock trial. Additionally, future research should include deliberation and group verdicts. Including group decision making, however, would likely require different methods and/or statistical analysis. That is, attitudes and degree of exposure to PTP would be expected to vary across actual juries, meaning that assigning participants similar in attitude and exposure would artificially reduce differences among individual

jury members, and create a more homogenous group that may arrive at different conclusions than a more heterogeneous group. Of course, allowing attitudes and degree of PTP exposure to freely vary would make analysis of group verdicts using conventional techniques difficult. PTP researchers may wish to use a methodology and analysis technique that takes into account group member differences and interactions, such as social network analysis.

Social network analysis provides not only theory and methods to understand the interaction between individuals and groups, but also specific analysis techniques and software to model these complex interactions (Knoke & Yang, 2008). Specifically, a network is made up of two or more actors who are connected in some way; the type and strength of connections between actors can vary (Galaskiewicz & Wasserman, 1994). Though social network analysis was developed to examine linkages between parties, and relationships between number of linkages and social influence, Pattison (1994) argues that social network analysis could also be used in social cognitive research, such as research on attitude change. That is, social network analysis could be used to understand the process by which one or more individuals in a network exert influence over other individuals, resulting in attitude change.

Additionally, social network analysis could be used to understand information sharing in juries. Shared information has a strong influence on jury outcomes (Kerr & Tindale, 2004). Information shared by the members of the group will be more likely to be used than information held by only a few members of the group. In the case of PTP, unless a case received a great deal of publicity, it is reasonable to assume such

information would not be shared by all group members. Even with a great deal of publicity, different group members may be aware of different pieces of information, and there may be little overlap in knowledge. Furthermore, PTP information discussed during deliberation may or may not have an effect; it could be that participants exposed to PTP communicate that information with un-exposed jurors, causing a diffusion of treatment, or it could be that non-exposed jurors caution exposed jurors that the information being discussed is not from the trial (Bruschke & Loges, 1999), thereby nullifying any source monitoring errors.

There is likely, however, to be variation in individual jurors' level of influence. An influential jury member who discusses biasing PTP information may have a stronger effect on the other jury members than a less influential jury member with the same information, and so the propensity to use unshared information may vary by jury influence. Combining such research with social network analysis would allow researchers to take into account different levels of influence within the jury. In addition, such research could examine varying degrees of jury exposure, comparing juries where few members saw PTP to juries where the majority of members saw PTP; this would allow researchers to determine a dosage effect and could aid in jury selection, as well as in appeals on the basis of PTP.

Another potential future direction is to examine the interaction of PTP with cases involving additional instructions or defenses. For example, cases in which the defendant is eligible for the death penalty usually include the process of death qualification, in which potential jurors are questioned about their beliefs on the death penalty, and

individuals with moral objections to the death penalty are excluded. Research has already found that death qualification can bias a jury toward conviction. Butler (2007) examined the interaction between PTP and death qualification, and found that death-qualified participants were more likely to attend to and remember PTP, which could lead to additional bias. Researchers may wish to analyze the interactive effects of PTP in other situations, such as in insanity trials, where jurors are also often hesitant to acquit.

## CHAPTER NINE

### CONCLUSIONS AND GOING FORWARD

For years, there has been debate in the fields of psychology, communication, and law, as well as other related fields, on what should be done to deal with pretrial publicity effects. Unfortunately, there is not necessarily agreement in the field that PTP biases verdicts, mainly because of the variation in study results (Bruschke & Loges, 2004). Reviews of the literature, and studies like this one, suggest that PTP does not always bias verdicts, but in certain cases, and with certain jurors, such information can be biasing. Some potential moderating variables, such as case strength or content of PTP, have been explored (e.g., Locatelli, Fulero, & Kluwe, 2011), but more guidance is needed for such results to be useful for legal professionals.

This study is one attempt to explain why PTP may be biasing for some jurors. It may not be the information alone, but the pre-existing attitude of the juror, that interacts with the case and PTP information. Though additional work is needed to clarify the wording of instructions to correct for these effects, these results give lawyers and judges some information as to when PTP may bias jurors, and in how to uncover such bias. Unfortunately, knowledge is not enough.

#### **Why Knowledge is Not Enough**

Even if legal professionals agreed that these and similar findings are valid and that PTP may be a cause for concern, using such results to change practice is not

necessarily straightforward. Though one could argue that legal professionals simply do not understand psychological research well enough to apply these findings, the issue goes deeper than lack of understanding. Even the most capable and knowledgeable of *psychological researchers* may struggle with applying psychological research findings to their lives, let alone to an organization as complex as our legal system. It is not always clear how to put research findings into practice, and there are many barriers that may hinder such application. As Rogers (2003) points out, “Many technologists believe that advantageous innovations will sell themselves, that the obvious benefits of a new idea will be widely realized by potential adopters, and that the innovation will diffuse rapidly. Seldom is this the case” (p. 7).

Some researchers and organizations have attempted to introduce innovations in courtrooms. These examples show that more direction is needed than simply informing courtrooms of beneficial innovations. Though none of these innovations targeted PTP specifically, some target jury procedures that could be useful in cases involving PTP. For example, Sand and Reiss (1985) discuss seven experiments conducted in the District Courts of the Second Circuit. These experiments were planned by a committee established as part of the Second Circuit Judicial Conference and appointed by the Chief Judge for the Second Circuit. Second Circuit courtrooms were contacted and invited to adopt one or more procedures: (1) permitting attorneys to question potential jurors during *voir dire*, (2) allowing individual questioning of potential jurors in the judge’s chambers, (3) instructing jurors before trial, (4) allowing jurors to ask questions, (5) allowing jurors to take notes, (6) providing a written copy of the charges to jurors in the deliberation

room, and (7) providing jurors with a tape recording of the charges in the deliberation room. Courts were provided with detailed instructions on implementing the changes as well as measures to collect data on the effects of these innovations. Reactions for the seven innovations were generally positive, and the authors encouraged future experimentation with courtroom innovations.

Introducing innovations to a legal setting involves examining the process of implementation throughout, to assess any barriers or challenges to implementation. Whether an innovation is implemented as planned can influence the outcome, and differences in implementation may explain why a program “works” in one setting but does not in another. Gondolf (2010) discusses two evaluations of experimental programs targeting batterers, one an experimental design that failed to find expected differences, and the other, originally designed as an experiment, that was changed to a phased implementation. Gondolf uses these examples to caution against the over-dependence on experimental designs, and, more importantly, to encourage researchers using experimental designs to study court innovations to examine factors related to implementation, adverse events, and unintended consequences. In fact, lessons learned from these and other implementation projects could be useful in planning implementation research on innovations for PTP and juror bias.

One organization that regularly works with courts to develop and implement innovations is the Center for Court Innovation. Founded in the mid-1990s, the Center is made up of experts in law and social sciences, such as social work and psychology, who work together to introduce innovations to New York courtrooms (Center for Court



Innovation, 2011). Many of the projects within the Center involve “problem-solving justice”, in which the goal of the court is to solve the underlying problems of the defendant, victim, and community in order to prevent future crime (Berman & Feinblatt, 2003). The Center takes a hands-on approach with the courts targeted for innovations, and collects data throughout to inform the process and demonstrate program success.

Recently, the Center for Court Innovation published a report detailing lessons learned from various implementation projects (Cissner & Farole, 2009). These lessons are: (1) have a shared vision and identify program goals, (2) identify quantifiable objectives, (3) have a plan on how to collect data, (4) formalize the program model, (5) be strategic about when and how to engage stakeholders in the planning process, (6) think about how to facilitate buy-in from line staff, (7) be realistic, (8) beware the temptation to overestimate caseload volume, (9) adapt the program in response to early implementation experience, (10) designate a project director, and (11) find political champions.

Similarly, Visher, Newmark, and Harrell (2007) identified barriers to the implementation of a program for domestic violence offenders and victims. The program, known as the Judicial Oversight Demonstration, was designed to increase oversight on the offenders, ensure uniform responses to offenders, and coordinate services for victims and other affected parties, such as children of the victim. Though the program led to positive changes at the demonstration sites, five “challenges” to implementation were identified: (1) gaps in knowledge about the operations of other service partners, (2) unanticipated impact on workload of partner agencies, (3) delays caused by county and

state rules on new positions, (4) lack of systems to coordinate data-sharing, and (5) difficulty integrating community-based victim services into justice agencies.

Following these lessons, by setting clear goals, establishing leadership to direct operations, and formalizing communication and data-sharing, may prevent some issues with implementation from interfering with the innovation. Even so, researchers may still encounter a variety of barriers, and guidance on planning innovations to accommodate such barriers would maximize program success, which may help to persuade the courts to be involved in more partnerships with researchers in the future.

There is no framework or theory on the barriers to applying research evidence in a legal setting, but researchers in health care settings provide some guidance that may be applicable. That is, researchers in health care settings are experiencing similar difficulties with translating their findings into practice. Many of these researchers argue that the issue is lack of understanding by providers and other medical professionals, but the bigger issue is that changing practice is a slow process (Rogers, 2003), involving many variables, such as the organization's resources, involvement by leadership, and mechanisms for feedback (Greenhalgh, Robert, MacFarlane, Bate, & Kyriakidou, 2004; Rycroft-Malone et al., 2002). These issues explain why a well-designed, empirically validated program may not have the same results when introduced in a particular setting. They have tackled these issues by developing a different kind of research, which occurs after clinical research but before changed practice: implementation research.

## **Background on Implementation Science**

Implementation science and implementation research address the issues associated with applying research findings in practice. Specifically, implementation researchers place their main focus on examining the barriers and facilitators to implementation and in many cases, making changes to address those issues (Werner, 2004). These changes may be within the system to remove barriers or introduce facilitators; alternatively, they may make changes to the program to allow it to work with pre-existing structures and processes, by working to identify what pieces of the intervention that are vital to program success and what components that can be customized to fit into a certain environment. Though implementation research uses its own terminology and study designs, psychological researchers will be able to understand some of the underlying theory and components of these models and frameworks, in part because many are based on psychological theories, such as theories of behavior change (Bero et al., 1998).

In addition, many theories are based on Rogers's diffusion of innovations model (Glasgow, 2007), which grew out of Rogers's research on diffusion of agricultural innovations and today can be used to explain a variety of innovations, from medical treatments to computers. According to Rogers (2003), making an innovation part of regular practice is a phased process, in which individuals or organizations learn about the innovation (knowledge), evaluate the innovation (persuasion), decide whether to adopt or reject the innovation (decision), adopt the innovation (implementation), and seek evidence to reinforce their decision (confirmation). Different people will adopt

innovations at different stages; some will be “early adopters”, who will evaluate and adopt an innovation when it is very new, and others will wait for more evidence and advice from others before implementation. Furthermore, characteristics of the innovation, such as its relative advantage, complexity, and compatibility with current practice, will influence whether an innovation “catches on.”

One framework that is frequently used by implementation researchers is PARIHS (Promoting Action on Research Implementation in Health Services), which states that implementation is a function of three general categories: (1) evidence, or the support for the effectiveness of an intervention or practice, including research evidence, experience of parties within the system, and local data; (2) context, which includes the organization’s readiness for change, culture, leadership, resources, and evaluation and feedback systems; and (3) facilitation, such as individuals who have been appointed to help or enable others to change their practice (Rycroft-Malone et al., 2002; Rycroft-Malone et al., 2004). Any attempt to implement research evidence into practice should take these three categories into account, recognizing that any of these three categories, even evidence to support the intervention, is likely to vary by site. Because PARIHS was designed to be used in health care settings, many terms are specific to the delivery of patient care. The PARIHS model, however, could be useful in understanding implementation in a variety of systems, and can be used when planning an implementation intervention (Kitson et al., 2008) or to evaluate an intervention that has already been implemented (Helfrich et al., 2010).

RE-AIM is another potential framework that could be adopted by legal researchers. RE-AIM stands for: (1) Reach, the proportion of people or organizations that would be able to apply the intervention; (2) Effectiveness, how well the intervention improves a given outcome; (3) Adoption, the proportion of eligible people or organizations that choose to implement the practice or intervention; (4) Implementation, the degree to which an intervention is delivered as it was intended; and (5) Maintenance, the sustainability of a practice or intervention (Glasgow, 2003). Like PARIHS, RE-AIM is often applied in health care settings; PARIHS, however, identifies organizational factors influencing implementation, while RE-AIM is applicable to both organizations and individuals in understanding the factors that affect successful implementation (Glasgow, 2007). For example, maintenance could refer to an organization's ability to maintain a changed practice, but it could also refer to an individual's ability to continue participating in a program. The developers of RE-AIM suggest its use in designing an implementation, because it encourages individuals to consider more than the empirical evidence for a given intervention (effectiveness), such as factors that impact who may benefit from the intervention (Glasgow, Vogt, & Boles, 1999).

Another model, developed through a systematic review of 495 sources about interventions in health care settings, is a complex account of how innovations are disseminated, diffused, and implemented, and the linkages between the service organization and other systems (Greenhalgh et al., 2004). Though the authors note that their model is for use as a "memory aide for considering the different aspects of a complex situation and their many interactions" (p. 594), rather than a tool for planning

innovations, the model identifies many factors that should be taken into account when prescribing changes in complex systems. Specifically, aside from noting the importance of strong communication, and discussion of meaning and mission between the change agency and the target agency, Greenhalgh et al.'s model includes several characteristics of the innovation itself that would influence how easily it is implemented, which reflect attributes identified by Rogers (2003). Specifically, the innovation should be clearly effective and cost-effective; compatible with organizational norms and practices; simple to use; able to be experimented with, adopted in stages, or tailored; and result in observable changes.

Finally, Lukas and colleagues (2007) developed an organizational model of transformation based on interviews conducted as part of a program evaluation on *Pursuing Perfection*, a program designed to introduce dramatic changes to health care. Though the models previously discussed all remark that outside forces (from beyond the organization of interest) are at play, Lukas et al.'s model notes that the impetus to changed practice often comes from outside the organization through policy or public pressure. Leadership within the organization is placed at the top of the model, and promotes changes throughout the organization. Pressure from outside and activity by leadership leads to the development of improvement initiatives, which transform the organization by (1) improving operations, (2) connecting multidisciplinary staff so they can work together to solve a tangible, important problem, and (3) building momentum for future innovations. This model also notes the importance of context, including organizational mission, culture, resources, and current practice.

Though many of the frameworks, theories, and models just discussed were designed to be applied to health care settings, the basic components can be applied to a variety of situations, such as the legal system. Additionally, health care settings and legal settings are similar in complexity and resource limitations. Unlike health care settings, however, professionals in legal systems may not have as much opportunity to interact with professionals in other departments or locations. Therefore, some implementation tactics that are useful in health care settings, such as local opinion leaders who educate other practitioners and encourage buy-in, may not translate to legal settings. Even so, there may be ways for PTP researchers and psychologists to bridge the gap, and conduct research and interventions that could assist legal professionals in applying empirical evidence.

Based on these theories, models, and frameworks, there are several important issues to consider when planning implementation of an intervention for the legal system. Organizational readiness for change is an important consideration for making changes within the legal system; because of the strength of precedent in directing practice, anyone attempting to make changes within the legal system should carefully assess readiness for change, which may vary depending on the type of change being suggested (Kitson et al., 1998). Greenhalgh and colleagues (2004) note that a receptive context should have: (1) leadership and vision, (2) good managerial relations, (3) a risk-taking climate, (4) clear goals and priorities, and (5) high-quality data capture. Ensuring that these elements are sufficient for the proposed change will maximize the adoption and effectiveness of the intervention.

Limited resources, including financial, time, and human resources, should also be taken into account. Any suggestions for change should not require large changes in resource allocation, but should be feasible using current resources. In this case, suggesting minor changes to instruction wording or additional discussion of jury instructions may be manageable by most courts; suggesting greater use of change of venue or change of *venire* would be difficult, if not impossible, for many courts. If additional physical or human resources are necessary, researchers should consider applying for outside funding.

Similarly, any suggestions for changed practice should compliment current practice and not require a substantial change in time commitment. Psychological researchers could work on developing accessible tools, along with clear advice on when to use these tools, that could be adopted by court officials or lawyers. For example, because attitudes were found to have an influence on use of PTP information, a bank of attitude questions that could be used for a written measure or verbal questioning, could supplement current *voir dire* procedures and, provided clear guidance is given on how to use the results, may help these procedures to uncover more useful information.

Influence from outside of the organization and attention from leadership are also essential in encouraging and planning innovations in the legal system. Gaining the support from and working through oversight agencies may provide the additional push needed to get meaningful changes into practice. For example, in the seven experiments discussed by Sand and Reiss (1985), the invitation and instructions came from leadership in their circuit; for this reason, judges may have been more willing to participate and may



have seen these suggested changes as more significant and justified. Strong leadership is also important to organize communication and data-sharing among individuals or organizations, which will help to prevent delays or misunderstandings.

An additional issue to consider is to recognize the expertise of legal professionals when making suggestions for improvement, particularly if a psychological researcher is organizing the implementation trial. Psychologists have a strong understanding of human cognition and behavior, which is necessary to uncover bias. At the same time, lawyers understand the law better than psychologists, and as such, are better equipped to question potential jurors in ways that are legally permissible. Though in the famous O.J. Simpson homicide trial, trial consultants were appalled that prosecutor Marcia Clark chose to ignore her jury expert's advice to keep African-American women off the jury, deciding instead to purposefully select African-American women to serve on the jury (Linder, 2011), Clark may have also been concerned about violating statutes that prevent lawyers from targeting African-Americans for exclusion (*Batson v. Kentucky*, 1986). That is, evidence that she was purposefully excluding African-American women from serving could have been challenged by the defense; if the judge agreed that the prosecution was targeting certain individuals, and decided to seat these individuals on the jury despite the prosecution's request that they be excluded, these jurors could have begun hearing the trial believing the prosecutor was "against them." Therefore, Clark's decision may have been strategic, to get these individuals to believe she was "on their side", though, focus groups with African-American women suggest that jurors' opinion of her may not have been positive (Linder, 2011).

### **Bridging the Gap**

It is important to keep in mind that any proposed changes to a system as large and complex as our legal system should work with the pre-existing structures and correct the problem with the smallest change in practice possible. More research is needed to determine whether a change is feasible, and these studies should take place in actual legal settings. After lab studies showing whether a change affects the outcome, the next step is to conduct studies with the cooperation of a small number of courtrooms.

As part of these small studies, data should be collected on not only the outcome, but any barriers, facilitators, and unintended consequences (both positive and negative). These results would provide information on how feasible it is to implement this change into practice, and direction on how to alter the program or practice in a particular setting so that it works with, not against, current practice. As suggested by Cissner and Farole (2011), innovations should be designed in such a way to allow changes to be made when needed, by providing direction on how the practice could be changed if needed and providing a mechanism for feedback. Allowing for tailoring should not only make legal professionals more open to implementing the changes; it will also help to allay any concerns about adverse outcomes that could lead to mistrials or appeals.

### **Working Together to Move Forward**

For many years, psychological researchers have been examining the legal setting and legal professionals, and providing evidence on issues with current practice. From Sigmund Freud telling Austrian judges that their unconscious processes may bias their decisions (Bersoff, 1986) to criticism of jury selection in death penalty cases (Lynch,

2009), there are countless examples of psychologists and experts from related fields insisting that something needs to change with little guidance on what to do instead or, when changes are suggested, how to merge them with current practice. There are a few cases where these experts have stepped in, explained the program in language legal professionals can understand, and provided specific instructions and assistance on making the change work in a particular setting. If researchers want to effect real change in the legal system, we must be willing to meet the legal system halfway, and take this hands-on approach more frequently. By showing that we understand the burdens placed on legal professionals, or at the very least, making efforts to learn about the processes that may hinder improvements to practice, we will be able to work together to change practice for the better.

APPENDIX A:  
REVIEW OF WITNESS CHARACTERISTIC EFFECTS

Some PTP researchers have examined characteristics of the crime. The heinousness of the description has been found to affect verdicts. Though they did not compare two different crimes, Hoiberg and Stires (1973) manipulated whether the PTP contained a detailed description of a murder-rape or gave a short description of the murder without mentioning the rape. All participants learned about the sexual nature of the crime at the trial, but female participants who received the detailed description in PTP rated the defendant as more guilty after the trial.

Deitz and Sissman (1984), in addition to questioning respondents about a specific case, asked various questions about crime in general. Participants rated the seriousness of various crimes and estimated the number of defendants accused of each crime that were actually guilty. Though there were no differences in estimates across the different crimes, there were differences in the perceived seriousness of crimes on a scale of 1 to 10. Speeding and shoplifting received the lowest ratings. Credit card theft, possession of illegal drugs, and burglary received ratings just above the scale midpoint. The most serious crimes were generally crimes against people, such as kidnapping, rape, and murder. The one exception was distribution of cocaine.

It is likely that different crimes have different effects on reactions to defendants because of the emotions produced by such crimes. According to Ajzen and Sexton (1999) affective responses are, for the most part, independent from the cognitive elements involved in forming an attitude. These responses, however, can influence cognition in certain situations. For instance, the affective-primacy hypothesis states that affect

influences evaluations of novel stimuli if the person is unaware of the change in or source of the affect.

Baumeister, DeWall, and Zhang (2007) differentiate between affect and emotions. Affect is an automatic, predominantly unconscious reaction that guides behavior. Emotion is the more deliberate, conscious experience, which occurs more slowly and is less helpful in guiding behavior. One could think of the latter as what non-psychologists mean when they discuss emotion, whereas the former is more similar to mood. Certain crimes may affect decision making by eliciting sudden reactions. The person may not know the cause of his or her affect or mood, and may attribute it to something else (Gasper & Isbell, 2007), such as the defendant. This may then cause him or her to evaluate the defendant more negatively and as more likely to be guilty.

In addition, affect can influence the type of information seen as relevant (Gasper & Isbell, 2007), so that when a person is in a negative mood, he or she is more able to remember negative information. This would mean that participants receiving a crime that elicits a more negative mood would be more likely to recall negative PTP information, and therefore, more likely to use such information. Though participants with strong attitudes toward a given piece of information would likely remember, and use, such information regardless of mood, participants with weaker attitudes may also be likely to use such information when in a negative mood.

APPENDIX B:  
PILOT TESTING CONSENT SCRIPT

This study is about decision making in a court case. It is being conducted by Sara M. House for a dissertation under the supervision of Linda Heath in the Department of Psychology at Loyola University of Chicago.

You are being asked to participate because you are eligible for jury service. You must be at least 18 years of age to participate.

If you agree to be in the study, you will be asked to:

- Read an abbreviated trial transcript.
- Render a verdict and answer questions about your reactions to the case.

There are no foreseeable risks involved in participating in this research beyond those experienced in everyday life. There are no direct benefits to you from participation, but the results will be helpful in understanding reactions to trials.

Your results will be completely confidential. Your name will not be attached to any materials, and only the researcher will have access to the data.

Participation in this study is voluntary. If you do not want to be in this study, you do not have to participate. Even if you decide to participate, you are free not to answer any question or to withdraw from participation at any time without penalty.

If you have questions about this research study, please feel free to contact Sara M. House at [shouse1@luc.edu](mailto:shouse1@luc.edu) or the faculty sponsor, Linda Heath, at [lheath@luc.edu](mailto:lheath@luc.edu).

If you have questions about your rights as a research participant, you may contact the Loyola University Office of Research Services at (773) 508-2689.

By completing the survey you are agreeing to participate in the research.



APPENDIX C:  
PILOT TESTING DEBRIEFING

You have been participating in a study on jury decision making. The purpose of this study is to find a case of moderate strength for use in future research.

You received one of six versions of the case, each containing different combinations of evidence. In addition, some of you received a case where the victim was a middle-aged man, and some of you received a case where the victim was an elderly woman. It is expected that participants reading a case with an elderly female victim will experience more negative emotion. Finally, some of you completed this study online, and some of you completed the study in a classroom; the purpose is to determine if the way in which you completed the study affects your reactions to the case.

This research is ongoing, so please do not discuss the study with anyone. If you have any questions or would like to receive a copy of the results of this study, please contact Sara House at [shouse1@luc.edu](mailto:shouse1@luc.edu).

APPENDIX D:  
COMPLETE LOGISTIC REGRESSION TABLE

<i>Predictor</i>	<i>B</i>	<i>SE</i>	<i>Wald</i>	<i>df</i>	<i>p</i>
Constant	-1.386	0.791	3.075	1	0.080
Location	0.000	1.118	0.000	1	1.000
Overall Version			3.178	5	0.673
Version dummy 1	0.539	1.049	0.264	1	0.608
Version dummy 2	0.000	1.118	0.000	1	1.000
Version dummy 3	0.405	1.041	0.152	1	0.697
Version dummy 4	1.386	1.012	1.875	1	0.171
Version dummy 5	0.000	1.118	0.000	1	1.000
Victim	0.981	1.021	0.924	1	0.337
Overall Location X Version			7.520	5	0.185
Loc X Version dummy 1	-1.551	1.678	0.853	1	0.356
Loc X Version dummy 2	1.792	1.514	1.401	1	0.237
Loc X Version dummy 3	0.644	1.432	0.202	1	0.653
Loc X Version dummy 4	-1.099	1.522	0.521	1	0.470
Loc X Version dummy 5	1.386	1.508	0.845	1	0.358
Overall Victim X Version			9.169	5	0.103
Vic X Version dummy 1	-1.520	1.464	1.078	1	0.299
Vic X Version dummy 2	-38.466	67110000.0	0.000	1	1.000
Vic X Version dummy 3	-1.386	1.458	0.904	1	0.342
Vic X Version dummy 4	-2.367	1.438	2.711	1	0.100
Vic X Version dummy 5	1.792	1.514	1.401	1	0.237
Victim X Location	-1.386	1.683	0.678	1	0.410
Overall Victim X Location X Version			11.896	5	0.036
Vic X Location X Version dummy 1	2.937	2.377	1.526	1	0.217
Vic X Location X Version dummy 2	36.962	67110000.0	0.000	1	1.000
Vic X Location X Version dummy 3	1.281	2.176	0.346	1	0.556
Vic X Location X Version dummy 4	3.312	2.218	2.230	1	0.135
Vic X Location X Version dummy 5	-3.584	2.365	2.295	1	0.130

*Notes.* Loc = Location; Vic = Victim.

APPENDIX E:  
NEWS ARTICLE

This article appeared in the media about this case.

[www.chicagotribune.com/news/local/chi-washington-case-tuesdayaug18,0,6895496.story](http://www.chicagotribune.com/news/local/chi-washington-case-tuesdayaug18,0,6895496.story)

## **chicagotribune.com**

### **Arrest made in Washington case**

By Deborah Flores

Tribute reporter

An arrest has been made in the recent battery of Joseph Washington, a 45-year-old Chicago resident.

Mr. Washington awoke from a nap to find a man in his home on the morning of August 15<sup>th</sup>. The man attacked Mr. Washington, then left. Mr. Washington was later taken to the hospital and treated for a cut on his cheek and a broken rib.

On Monday, police arrested Brian Campbell, 35, a former employee of Jesse Washington, the victim's brother. Campbell is facing an aggravated battery charge.

*[Participants in the PTP conditions received one of the following additional statements:*

According to a police source, the suspect has confessed to the crime.

According to a police source, the suspect has been convicted of a similar crime in the past.

According to a police source, the suspect did not cooperate during arrest.]

Campbell posted bond and is due in court later this week. Campbell's attorney, Richard Bennett, has stated that his client will plead not guilty.

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APPENDIX F:  
VOIR DIRE QUESTIONNAIRE

### Voir Dire Questionnaire

You are now going to serve as a juror on the case you just read about. Before reading the trial transcript, please complete the following questionnaire, which is part of a process called 'voir dire'. Attorneys often use these questionnaires to learn more about potential jurors.

Gender: (circle one)    Male    Female

How old are you? \_\_\_\_\_

Year in School (circle one):    Freshman    Sophomore    Junior    Senior

Race: (circle one)

African-American/Black

Asian-American

Caucasian

Hispanic/Latino

Other

Prefer not to answer

Have you ever served on a jury before? Yes    No

Attorneys also want to know about potential jurors' views of the legal system. Read each statement and indicate how much you agree with it.

*Note: Questions are not presented here in the same order as they were received by participants; treatment attitude questions were intermingled with other questions to decrease participant suspicion.*

#### Questions Received by All Participants:

1. Unfair treatment of underprivileged groups and classes is the chief cause of crime.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

2. Too many obviously guilty persons escape punishment because of legal technicalities.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree



3. Evidence illegally obtained should be admissible in court if such evidence is the only way to obtain a conviction.
- Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree
4. Search warrants should clearly specify the person or things to be seized.
- Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree
5. No one should be convicted of a crime on the basis of circumstantial evidence, no matter how strong such evidence is.
- Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree
6. There is no need in a criminal case for the accused to prove his innocence beyond a reasonable doubt.
- Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree
7. Wiretapping by anyone and for any reason should be completely illegal.
- Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree
8. Defendants in a criminal case should be required to take the witness stand.
- Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree
9. All too often, minority group members do not get fair trials
- Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree
10. Because of the oppression and persecution minority group members suffer, they deserve leniency and special treatment in the courts.
- Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree
11. Citizens need to be protected against excess police power as well as against criminals.
- Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree
12. It is better for society that several guilty men be freed than one innocent one wrongfully imprisoned.
- Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

13. Accused persons should be required to take lie detector tests.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

14. When there is a “hung” jury in a criminal case, the defendant should always be free and the indictment dismissed.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

15. A society with true freedom and equality for all would have very little crime.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

16. It is moral and ethical for a lawyer to represent a defendant in a criminal case even when he believes his client is guilty.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

17. Police should be allowed to arrest and question suspicious-looking persons to determine whether they have been up to something illegal.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

18. The law coddles criminals to the detriment of society.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

19. The freedom of society is endangered as much by overzealous law enforcement as by the acts of individual criminals.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

20. In the long run, liberty is more important than order.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

21. Upstanding citizens have nothing to fear from the police.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

22. The punishments given to criminals accurately reflect the crimes they have committed.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

23. The skills of lawyers determine the verdict more than the truth.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

24. Too many criminals are out on parole.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

25. Police brutality is more common than people think.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

26. If a defendant has enough money he or she will be able to buy an acquittal.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

27. As a group, judges are more impartial and fair than people in general.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

28. Juries base their decisions only on the evidence given in court.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

29. Too many criminals are let free because of prison overcrowding.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

30. Court-appointed attorneys and public defenders do as good a job as personally hired attorneys.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

31. Police will often keep a suspect in custody, even when they don't have any firm evidence against him.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

32. Most prosecuting attorneys have a strong sadistic streak.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

### **Confession Questions:**

33. When a suspect confesses to the police, he or she does so voluntarily.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

34. Anyone who confesses to a crime should be automatically found guilty and sentenced.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

**Prior Record Questions:**

35. When determining a person's guilt or innocence, the existence of a prior arrest record should not be considered.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

36. If a suspect has committed a crime once, chances are he'll do it again.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

**Resisting Arrest Questions:**

37. Any person who resists arrest has committed a crime.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

38. People who run from the police must have something to hide.

Strongly Disagree    Disagree    Neutral    Agree    Strongly Agree

APPENDIX G:  
TRIAL TRANSCRIPT

## Trial Transcript

Note: This is an abbreviated transcript, not a complete transcript. You will read statements by the attorneys and questioning of key witnesses. Some witnesses have been omitted. After reading this transcript, you will be asked to determine a verdict, and answer some questions about your reactions to the case.

THE COURT: Good morning, ladies and gentleman. We are in open court with counsel and the defendant, and we have the jury present. The defendant has entered a plea of not guilty to the charge of aggravated battery.

THE COURT: The next part of the trial involves allowing the parties, if they wish to do so, to make an opening statement. The purpose of an opening statement is to provide you with a brief outline as to what the parties anticipate the evidence will show during the course of the trial. We start with the State.

THE COURT: I think we are ready to begin, Mr. Russell.

MR. RUSSELL: Thank you.

On the morning of August 15<sup>th</sup>, Joseph Washington saw his family off to work. Around 10:00 AM, he laid down to take a nap, and awoke shortly after 11:00AM to find a man in his home. This man proceeded to attack Mr. Washington, by pushing him down, and kicking him twice in the ribs. This caused Mr. Washington to sustain many injuries, including bruises, a cut on the cheek, and a broken rib. You will hear from Mr. Washington that he feared the attacker might intend to kill him. Then you will hear from Mr. Washington that he identified Brian Campbell as the man who attacked him. You will hear from a neighbor who recalled seeing the defendant's car in the area shortly before the attack and another neighbor who saw the same car in front of the Washington home. You will hear about a wooden case recovered from the defendant's home that resembles a case taken from the Washington home on the day of the attack. Finally, you will hear that the defendant was a former employee of Mr. Washington's brother, Jesse Washington, and that the defendant had been recently fired for poor performance. And you will hear that the defendant told Jesse Washington, "You'll be sorry for this" after he was fired.

This is the reason for the attack. It is entirely about revenge. Brian Campbell came into Joseph Washington's home and attacked him to get even with his former boss. Perhaps this was not his original plan when he entered the home. Maybe he wanted to steal something or ransack the place. But when he found Joseph asleep, his plans changed. He thought, "This is even better. This will definitely make Jesse Washington sorry." The defense will try to convince you that someone else is responsible for brutally attacking Joseph. Other than the case belonging to Jesse Washington, nothing was taken. The Washingtons have no enemies. The only person with a grudge, and who threatened Jesse Washington, is in this courtroom today. We will show you that Brian Campbell is guilty of this brutal crime.

THE COURT: Ladies and gentlemen, that completes the first opening statement. The next one is by Mr. Bennett, for the defense.

MR. BENNETT: Thank you, your honor.

Mr. Russell thinks that because Mr. Washington identified my client in a police lineup, my client must be guilty. He leaves out the fact that Mr. Washington wears glasses, which he was not wearing during the attack. Mr. Washington is does not see well without those

glasses. It would be difficult, if not impossible, for him to actually be able to see his attacker. And yet, Mr. Washington identifies my client. Why? Not because he is guilty, but because Mr. Washington had met my client and knew what he looked like. Because Jesse Washington had mentioned that my client was recently fired and may retaliate. Mr. Washington's identification of my client means nothing.

We will show that my client has an alibi for the time of the attack. The eyewitness who saw my client's car drive by her home is correct. My client was in the area, because his friend lives in the area. Once again, this identification means nothing. In fact, every piece of evidence the prosecution will show you can easily be explained. My client is not guilty of this crime. And that is what we are going to show you.



THE COURT: We have concluded opening statements. We will now move on to the taking of evidence. The State is ready to call their first witness.

MR. RUSSELL: The State calls Joseph Washington to the stand.

BY MR. RUSSELL: Good morning. Mr. Washington, please state your name and age for the record.

A. Joseph Washington. 45.

Q. Can you please describe what happened on the morning of August 15<sup>th</sup>?

A. Yes. I woke up earlier than usual, around 6:00AM. I had some work to do in my garage, so I started on that. About 7:30 AM, my brother and daughter left for work. I saw them off, and went back to the garage. About 10, I was feeling pretty sleepy, and I decided to take a nap. I woke up maybe an hour later, and thought I heard footsteps in my room. I figured it was my brother, come home early. I looked up and there was a man in my room, looking at me. He was too tall to be my brother. I stood up, but the man pushed me down on the floor. It knocked the wind out of me. I tried to get up, but he kicked me in the side. That hurt a lot more than being pushed down. He kicked me again. I remember lying there, wondering what would happen next. I was afraid he was going to kill me. He was bigger than me, and I didn't know if he had a gun or something. I looked around the room for something to fight back with, but I looked up and he was gone. I laid there for a while. I'm not sure how long. I was trying to build up strength to get to the phone. I couldn't stand, so I crawled to the phone and called my brother. I told him a man had attacked me. He was tall, maybe 5'11. I said, "I think it was Brian". My brother was quiet for a second and then said, "Are you sure?" And I said, "Yes."

Q. Why did you call your brother instead of the police?

A. When I reached up for the phone, I felt this sharp pain where he kicked me. I couldn't stand up, and reaching was so painful. I wanted to dial quickly, and my brother's number is on speed dial, so I only had to hit one button. I guess, looking back, I should have called the police first, but at the time, I was in such pain, I don't think I was thinking clearly. My brother asked if I had called the police, and I said no, so he called.

Q. What happened next?

A. The police arrived. And my brother. I didn't realize until then that my cheek was cut and I was bleeding. They took me to the hospital for stitches and x-rays. I found out my rib was broken, which explains the pain. I told the police what happened, and that Brian Campbell was my attacker. A couple of days later, they called me in for a lineup.

Q. And that's when you identified Brian Campbell?

A. Yes.

Q. How do you think he got into your home?

A: I'm not sure. The police said there were no broken windows or anything, but I know I didn't leave any doors open or unlocked. The front door was unlocked when the police arrived, but I remember locking it.

MR. RUSSELL: Thank you. No further questions.

THE COURT: Mr. Bennett, you may cross-examine.

MR. BENNETT: Thank you, your honor.

BY MR. BENNETT: Mr. Washington, would you mind removing your glasses?

A. No, I don't mind.

Q. How many fingers am I holding up?

A. I don't know. Three?

Q. May I approach the witness, your honor?

THE COURT: You may.

Q. Okay, this is about five feet away. How about now?

A. I think four.

Q. You're right. How close was the attacker to you?

A. Maybe a foot or so away.

Q. How difficult was it for you to see my hand?

A. A little difficult, I guess.

Q. You had to squint or strain?

A. A little.

Q. But you're confident that Mr. Campbell was your attacker?

A. Yes.

Q. Why?

A. I've met Brian before. I remember how tall he was and the color of his hair. I recognized that.

Q. Thank you. You can put your glasses back on. You said you told your brother you *thought* it was Brian. You weren't sure it was him?

A. I was pretty sure it was Brian who attacked me before that.

Q. Just pretty sure?

A. I was certain it was him. Who else could it be?

Q. How come you didn't say, "Brian attacked me" first? You described the attacker to your brother instead before identifying him, right?

A. Yes. I don't know. I guess I wasn't thinking too clearly.

MR. BENNETT: Thank you, Ms. Washington. No further questions.

MR. RUSSELL: The State calls Jesse Washington.

BY MR. RUSSELL: Can you state your name and age for the record?

A. Jesse Washington, 58.

Q. What do you do for a living?

A. I own a body shop.

Q. Did Mr. Campbell ever work for you?

A. Yes, he started in March of last year.

Q. And when did Mr. Campbell stop working for you?

A. When I fired him in late July.

Q. Why did you fire him?

A. He was always goofing off with another employee. Not getting anything done. Customers were complaining that their cars took too long. It was bad for my business.

Q. Who was that other employee?

A. Ralph Turner. Brian was a good worker until I hired Ralph. I think I hired him in early July. They spent all of their time goofing around. I talked to them about it. But that didn't seem to work, so I fired Ralph because I remembered Brian was fine until Ralph showed up. But after Ralph was gone, Brian was still not getting work done. He spent a lot of time on his phone, and he called in maybe three times in a couple of weeks. I talked to him about, but it didn't stop. So I fired him too.

Q. Did Brian get angry?

A. Yes, he punched a wall. And said, "You can't do this." I told him sorry, but I'd given him too many chances. And he said, "You'll be sorry for this."

Q. He threatened you?

A. Yes. He was really angry. I asked him what he meant by that, but he just walked away.

Q. Did you worry that he might try to hurt you?

A. A little. He was a big guy. I mean, not just tall, but strong. I mentioned something to my brother that night. Not to let him in if he comes by. He seemed worried.

Q. You live with your brother?

A. Not today.

Q. Did you in August of last year?

A. Yeah, I moved in early last year, because I doing some major work on my house.

Q. Did Mr. Campbell know where you were living?

A. Yes. We had him over once in May. We had everyone who works for me over for a barbecue.

Q. And your brother met Brian Campbell then?

A. I think it was then. He's been by the shop before, so maybe he met him then. He's talked to him a couple of times.

Q. The day your brother called, did you suggest Brian Campbell was the attacker?

A. No. Joseph called and said he'd been attacked, and he thought it was Brian. I only asked if he was sure. And he said, "Yes." I never said it was Brian. Or made him think it was Brian.

Q. And when you got home, was anything missing?

A. I have a wooden box on a bookshelf in the hallway. By my brother's room. It had some personal things in it. Pictures, letters, a set of keys to my house. I had to change all the locks.

Q. Why would Brian take that?

MR. BENNETT: Objection, your honor.

MR. RUSSELL: I'll rephrase. Why do you think Joseph's attacker took that?

A. Maybe he wanted the keys to my place. The letters had my address. Maybe he wanted to break into my house.

Q. Brian Campbell didn't know where you lived?

A. No, I wasn't living there when Brian was working for me. I never had a reason to tell him where my house was.

MR. RUSSELL: Thank you. Those are all my questions.

BY MR. BENNETT: You've never found that missing case?

A. I told the police it wasn't on the shelf when we were upstairs helping Joseph. Later on, I looked around and didn't find it, so I told the police it had been stolen. When they arrested Brian, and called us down to the station for the lineup, they showed me the case they found in Brian's house. It looked like mine, but there was nothing inside.

Q. Was there anything identifiable about the case? Any marks or writing?

A. No writing. I remembered the size and color. And the closure. The case they found was the same.

Q. And they didn't find any of the pictures of keys at Mr. Campbell's place?

A. No, I guess he threw them away.

Q. But it's possible he never had your case?

A. I guess. It looked exactly the same to me.

Q. Was this case bought from a special store?

A. No, some department store I think. My brother got it for me.

Q. So it was something Mr. Campbell could have bought on his own?

A. Yeah.

Q. You said that Mr. Campbell could have thrown away your pictures and keys. Why would he keep the case?

A. I don't know. Maybe he didn't throw them away. Maybe he hid them.

Q. Somewhere the police didn't look? Do you know where they searched?

A. His home. His car, I think.

Q. And found nothing?

MR. RUSSELL: Objection, your honor. The witness has already said the police didn't find the objects from the case.

THE COURT: Sustained.

Q. Was your home ever broken into?

A. No.

Q. When he punched the wall, about how hard would you say?

A. I don't know. Pretty hard I think.

Q. Did he leave a dent or a hole?

A. No.

Q. Have you ever punched something when you felt angry or frustrated?

A. Probably.

MR. BENNETT: No more questions, your honor.

MR. RUSSELL: The State calls Laura Phillips.

BY MR. RUSSELL: How do you know Joseph Washington?

A. He's my neighbor.

Q. Do you know the defendant?

A. No, I don't.

Q. So, what made you call police that day?

A. I heard Joseph had been attacked at home, and they thought the person drove an old white Ford truck.

Q. And you saw this truck?

A. Yes. I was outside, in my front yard, gardening. And I saw that car drive by.

Q. Was there anything special that made you remember that car?

A. There's not a lot of traffic on my street, and it was a quiet morning, and the car was really loud. Like there was no muffler. It made me look up and watch it drive by.

Q. What time did you see the defendant's car?

A. It was around 11:00 in the morning.

MR. RUSSELL: Let me remind the court that shortly after 11:00 AM, the victim was attacked.

Q. Did it appear that the defendant was driving toward the victim's home?

MR. BENNETT: Objection, your honor.

THE COURT: Sustained.

Q. Was the defendant driving in the direction of the victim's home?

A. Yes.

MR. RUSSELL: Nothing further, your honor.

THE COURT: Mr. Bennett, you may cross-examine.

BY MR. BENNETT: Is Mr. Washington's home the only one in the direction my client was driving?

A. Well, no. Of course not.

Q. So my client could have been driving any number of places after driving by your house?

MR. RUSSELL: Objection, your honor.

MR. BENNETT: Your honor, I'm not asking the witness if Mr. Campbell was driving somewhere else. Just that he could have been going anywhere, not necessarily the Washington home.

THE COURT: I'll allow it. The witness may answer.

A. There's a main street that way, so he could have been heading there.

MR. BENNETT: Thank you. Nothing further.

MR. RUSSELL: The State calls Sean Jones.

BY MR. RUSSELL: How do you know Joseph Washington?

A. I live across the street.

Q. What do you remember about the morning of August 15<sup>th</sup>?

A. I remember seeing Joseph working in his garage earlier in the day. I was getting into my car to go to my office. About 11:00 AM, I came back home because I forgot an important file. As I was turning into my driveway, I noticed a white truck parked in front of Joseph's house.

Q. Did this seem strange to you?

A. I wasn't sure who the car belonged to, but I figured Joseph or someone had company. I went inside to find the file.

Q. Was the truck there when you came back out?

A. I don't remember seeing it, no.

Q. When did you come back out?

A. Maybe 10 minutes later. I didn't really think anything of it until I heard about the attack. I mentioned to the police that I'd never seen that white truck before that day.

MR. RUSSELL: Thank you. Nothing further.

BY MR. BENNETT: You never heard the car leave?

A. I thought I heard a loud noise, like a loud car or a motorcycle while I was inside my house. I remember thinking it was strange to hear such a loud car. We live in a pretty quiet neighborhood.

Q. When did you hear the noise?

A. I think it was a few minutes after I got home. I don't remember for sure.

Q. The truck you saw. Was it a Ford?

A. It might have been. I didn't really pay attention.

Q. You mentioned that Joseph *or someone* maybe had company. What did you mean by that?

A. Well, the truck wasn't directly in front of Joseph's house. It was kind of in between his house and his next-door neighbor, Mr. Turner's house.

Q. So you don't know if the driver of the truck was in the Washington home?

A. No, I don't.

MR. BENNETT: Thank you. No further questions.

MR. RUSSELL: Your honor, The State rests.



THE COURT: Thank you, Mr. Russell. Mr. Bennett, you may call your first witness.

MR. BENNETT: The defense calls Ralph Taylor to the stand.

BY MR. BENNETT: Was Mr. Campbell with you on the day Joseph Washington was attacked?

A. Yeah. We were watching TV at my apartment.

Q. You live near Mr. Washington?

A. Just a few blocks.

Q. So, when Mrs. Phillips saw my client driving, he was probably heading to your apartment?

MR. RUSSELL: Objection, your honor.

MR. BENNETT: I'll withdraw that question, your honor. About what time did my client arrive at your apartment?

A. It was a little after 11:00 AM.

Q. And how long did he stay?

A. A couple of hours.

Q. At any time during his stay, did he mention anything to you about Jesse Washington?

A. No. I think he was over being fired. We were looking for new jobs. We talked about where we'd like to work next. He seemed excited about starting a new job.

MR. BENNETT: Thank you. Nothing further.

BY MR. RUSSELL: You are also a former employee of Jesse Washington?

A. Yes, that's correct.

Q. And you were laid off shortly before the defendant?

A. Yes.

Q. Did it make you angry at Jesse Washington?

A. Yeah, I guess so. Really, I was more surprised and upset, maybe a little angry.

Q. And if something happened to one of Jesse Washington's loved ones, you would do what you could to help, even lie?

MR. BENNETT: Objection, your honor.

MR. RUSSELL: Withdrawn, your honor.

Q. About how long would it take to get from Mr. Washington's home to your apartment?

A. I don't know. 5 minutes maybe.

Q. So it's possible that Brian Campbell stopped there before coming to your place?

A. I don't think he would have gone there. Is it possible? I don't know. Maybe.

Q. Did anyone else see you and Mr. Campbell together?

A. I told my sister he was coming over, but no one else was there while we were watching TV.

MR. RUSSELL: No further questions.

MR. BENNETT: Redirect, your honor?

THE COURT: Proceed.

BY MR. BENNETT: Mr. Russell is suggesting that you're lying about my client being with you at the time of the attack. Are you lying?

A. Absolutely not. Brian is my friend, but I would never lie about something like this.

MR. BENNETT: Thank you. No further questions, your honor. The defense rests.

THE COURT: We will now hear closing arguments. Once again, we will begin with the State.

MR. RUSSELL: The defense would have you believe that this case is entirely about eyewitness testimony. Specifically Mr. Washington's testimony that Brian Campbell was his attacker. But this case is about more than that. There are not one, but three witnesses, and the other two provide information that shows Mr. Campbell was in the area, and was in front of Mr. Washington's house. But this case is still about more than that. Mr. Bennett would have you believe that Jesse and Joseph Washington were quick to believe Mr. Campbell was the attacker because of his recent firing. So quick to believe this that they ignored the possibility that someone else was the attacker. But they believed this because Mr. Campbell threatened Jesse Washington. He worried Jesse Washington enough for him to tell his brother. For him to warn that Mr. Campbell might retaliate, and "Don't let him in if he comes by." It seems that they were perfectly warranted in believing that Mr. Campbell was the attacker. That combined with the eyewitnesses is enough to show Mr. Campbell is guilty.

The defense even admits that Mrs. Phillips saw Brian Campbell's car drive by shortly before Joseph Washington was attacked. Ralph Taylor claims that Brian Campbell was with him watching TV at that time, but Ralph Taylor is also a former employee of Jesse Washington, who was fired for the same reason as Brian Campbell. Even if Mr. Taylor isn't lying for Brian Campbell, the attack took a few minutes. Brian Campbell could have easily driven to Mr. Taylor's afterward and still have arrived shortly after 11:00 AM.

Ralph Taylor suggests that Brian Campbell was at peace with his firing. That he had "gotten over it." Of course he was at peace. He had just gotten his revenge. Perhaps he had more planned when he took those keys, but maybe he decided, "This is enough. Time to move on." His revenge was at the expense of Joseph Washington's health, his feeling of safety in his own home. Brian Campbell's cruelty cannot go unpunished. Find him guilty of this crime.

THE COURT: We will now hear the defense's closing argument.

MR. BENNETT: Joseph Washington was brutally attacked. There is no question about that. And my client and I hope that the police catch the man who attacked him. But my client was not that man. We have shown that my client was with another person at the time of the attack. A witness saw my client's car in the area, but that's because my client was driving to Ralph Taylor's apartment. We have also shown that Joseph Washington was not wearing his glasses at the time of the attack, and did not clearly see his attacker's face. He never heard the attacker's voice. And when he told his brother about the attack, he started by describing the attacker. He said, "A man who was 5'11" attacked me", and it was only after that Mr. Washington identified the attacker as my client.

The wooden case found in my client's home was similar to one owned by Jesse Washington. But he admitted that the case was plain and unornamented, and could have been purchased from many stores. The case taken from Joseph's home contained personal objects, none of which were found in the case taken from my client's home. The State has not, and cannot,

prove that the case in the courtroom today is the one from Joseph's home. Brian Campbell was upset about being fired from his job. Anyone would be upset about that. Perhaps he said something he now regrets. That does not mean he attacked someone.

Mr. Russell thinks that my client's friend, Ralph Taylor is lying. Unfortunately, there's no way I can prove that his friend isn't lying, except that he says he isn't. You'll just have to take his word for it. But if you disbelieve Mr. Taylor, it's because you're taking Mr. Washington's word for it. The only evidence the state has that my client attacked Mr. Washington is that Mr. Washington says so. And Mr. Washington is mistaken.

APPENDIX H:  
JURY INSTRUCTIONS

THE COURT: Ladies and gentlemen of the jury, you have heard all of the evidence. It is my duty to instruct you on the law.

First, you must determine what facts have been proved from the evidence received in the trial and not from any other source

[*Added in specific instructions*: “, such as the media.”]

[*Added in strong instructions*: “, such as the media. Such outside information can be unreliable.”]

Second, you must apply the law that I state to you to the facts as you determine them, and in this way arrive at your verdict, even if you don’t agree with the law.

You must not be influenced by pity for or prejudice against a defendant. You must not be influenced by sentiment, conjecture, sympathy, passion, prejudice, public opinion or public feeling.

Statements made by the attorneys are not evidence. Evidence is either direct or circumstantial. Direct evidence is evidence that directly proves a fact. Circumstantial evidence is the proof of facts or circumstances which give rise to a reasonable inference of other facts which tend to show the guilt or innocence of the defendant. Circumstantial evidence should be considered by you together with all the other evidence in the case in arriving at your verdict.

The defendant’s guilt must be proved beyond a reasonable doubt. In other words, before an inference essential to establish guilt may be found to have been proved beyond a reasonable doubt, each fact of circumstances on which the inference necessarily rests must be proved beyond a reasonable doubt.

Motive is not an element of the crime charged and need not be shown.

A defendant in a criminal action is presumed to be innocent until the contrary is proved. The State has the burden of proving the defendant’s guilt beyond a reasonable doubt. Reasonable doubt is defined as the following: it is not a mere possible doubt, because everything relating to human affairs is open to some possible or imaginary doubt. It is that state of the case which, after the entire comparison and consideration of evidence, leaves the minds of the jurors in that condition that they cannot say they feel an abiding conviction of the truth of the charge.

In the crime charged, aggravated battery causing great bodily harm, there must exist severe or aggravated bodily injury to the victim, involving apparent broken bones, loss of teeth, possible internal injuries, severe laceration, or loss of consciousness.

To sustain the charge of aggravated battery causing great bodily harm, the State must prove the following propositions:

*First:* That the defendant intentionally engaged in conduct which caused Joseph Washington severe or aggravated bodily injury.

*Second:* That the bodily injury involved apparent broken bones, loss of teeth, possible internal injuries, severe laceration, or loss of consciousness.

If you find from your consideration of all the evidence that these propositions have been proved beyond a reasonable doubt, you should find the defendant guilty.

If you find from your consideration of all the evidence that these propositions have not been proved beyond a reasonable doubt, you should find the defendant not guilty.

APPENDIX I:  
POSTTEST



Please provide a verdict and rating of the defendant's guilt. **Answer both questions.**

1. As a juror in this case, what would your verdict be (circle one)?

Guilty

Not

Guilty

2. Using the following scale, how likely is it that the defendant is actually guilty (circle one)?

Definitely  
not guilty

1

2 3 4 5 6 7

Neutral

Definitely  
guilty

APPENDIX J:  
CONSENT SCRIPT

This study is about decision making in a court case. It is being conducted by Sara M. Locatelli for a dissertation under the supervision of Linda Heath in the Department of Psychology at Loyola University of Chicago.

You are being asked to participate because you are eligible for jury service. You must be at least 18 years of age to participate.

If you agree to be in the study, you will be asked to:

- Read a short news article.
- Complete a questionnaire.
- Read an abbreviated trial transcript.
- Render a verdict.

There are no foreseeable risks involved in participating in this research beyond those experienced in everyday life. There are no direct benefits to you from participation, but the results will be helpful in understanding reactions to trials.

Your results will be completely confidential. Your name will not be attached to any materials, and only the researcher will have access to the data.

Participation in this study is voluntary. If you do not want to be in this study, you do not have to participate. Even if you decide to participate, you are free not to answer any question or to withdraw from participation at any time without penalty.

If you have questions about this research study, please feel free to contact Sara Locatelli at [shouse1@luc.edu](mailto:shouse1@luc.edu) or the faculty sponsor, Linda Heath, at [lheath@luc.edu](mailto:lheath@luc.edu).

If you have questions about your rights as a research participant, you may contact the Loyola University Office of Research Services at (773) 508-2689.

When you are finished reading this form, return it to the experimenter to receive your study packet. By completing the packet, you are providing consent to participate in the research.

APPENDIX K:  
DEBRIEFING

You have been participating in a study on jury decision making and the effect of pretrial publicity. Pretrial publicity is defined as any media coverage of a case not yet in trial. Past research has shown that pretrial publicity, or PTP, can bias final verdicts in a trial.

Before reading the case, you received a news article. Some of you received only neutral information about the case, and the rest of you received one piece of biasing information (that the defendant had confessed, was convicted of a similar crime, or failed to cooperate with police during arrest). You then completed a measure of legal attitudes, and read the trial transcript. At the end of the trial, some of you received standard jury instructions, while the rest of you received more specific instructions, in which you were asked to ignore pretrial information. It is expected that the negative information from the news article will only bias verdicts of people with strong legal attitudes. It is also expected that the instructions to ignore PTP will work on people with moderate legal attitudes, but not people with strong attitudes.

This research is ongoing, so please do not discuss the study with anyone. If you have any questions or would like to receive a copy of the results of this study, please contact Sara Locatelli at [shouse1@luc.edu](mailto:shouse1@luc.edu).

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## VITA

Sara Marie Locatelli graduated *Magna cum Laude* with a BS in Psychology from Baker University in 2004. She earned her Masters of Arts in Social Psychology from Loyola University Chicago in 2007, conducting her thesis research on media and stereotype threat.

During her time at Loyola, she has served as a Research and Teaching Assistant (2004-2006), a Graduate Scholar in Feminist Studies (2005-2006), and a PreDoctoral Teaching Scholar (2009-2010). She has taught courses in Learning and Behavior, Psychology and Law, Research Methods, and Statistics; she received her department's Johnson Award for Excellence in Teaching by a Graduate Student in 2010. She has also conducted program evaluation research in cooperation with Chicago Public Schools, and independent research on relationships among Facebook use, rumination, savoring, and health outcomes. She was awarded an Association for Psychological Science Student Research Award in 2010 for her meta-analysis on pretrial publicity effects.

Sara is employed at Edward Hines VA Hospital in Hines, Illinois, where she is a Research Assistant in the Spinal Cord Injury Quality Enhancement Research Initiative (SCI-QUERI) and Center for Management of Complex Chronic Care (CMC<sup>3</sup>). After completing her PhD, she will begin a 2-year Post-Doctoral Research Fellowship at Hines VA.