

Chapter 35

Social Psychology and Law

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One significant value of much social science research is that it makes clearer what we only dimly perceive, if we perceive it at all. It is not surprising to hear people say about many psychological findings that, "of course, we knew this all along." Yet, very often, what we thought we knew all along is not quite correct or, more importantly, not quite correct in substantial detail.

(Faigman, Kaye, Saks, & Sanders, 2005, p. 568).

Common sense and intuitive beliefs about human behavior are fundamental to why people claim, "Of course, we knew this all along." Social science now impressively documents the human tendency to rely on common sense and intuition when engaged in social cognition (Fiske & Borgida, 2008). At the same time, many insights from psychological science, including theory and research in social psychology, challenge the intuitive understandings that people hold about a wide range of behavioral domains. Many of these behavioral domains intersect with legal processes (e.g., eyewitness identification, jury selection and pretrial publicity, false confessions, polygraphs and lie detection, stereotyping, prejudice, and discrimination) and reveal a substantive "disconnect" between intuitive conceptions of human behavior (i.e., what people assume to be true about eyewitness behavior or the effects of pretrial publicity or why people confess to crimes they did not commit) and the pertinent scientific data base (Borgida & Fiske, 2008).

This chapter highlights various applications of social psychology (both theory and methods) to the legal system, with a focus on examining the ways in which the U.S. criminal justice system implicates common sense and intuitive assumptions about human behavior that are empirically testable and have legal or theoretical significance, or both. There are many topical candidates for such an exercise, but the selected examples—the reliability of eyewitness identification, interrogations and confessions, jury selection, pretrial publicity effects on juror decision making, and legal decision makers' evaluations of expert evidence—exemplify this tradition and also

reflect an accumulation of quality science that provides the foundation for generating insights about the disconnect between legal standards and their assumptions and science-based understandings. In so doing, several other central topics at the intersection of psychology and law (e.g., risk assessment, competency issues, offender treatment) that are studied primarily by clinical and developmental psychologists are purposely sidestepped. A few other topics (e.g., history of the field, attribution of responsibility, media violence, juror competence and jury deliberation) were expertly reviewed in the fourth edition of the *Handbook of Social Psychology* (Ellsworth & Mauro, 1998). Similarly, a discussion of theory and research on social justice (Tyler, 2001; Tyler & Smith, 1997) and moral actions and judgments (e.g., Carlsmith, Darley, & Robinson, 2002), topics that occupy territory at the intersection of social psychology and law, also are not included because these topics are covered extensively in other chapters in this volume.

ROLE OF SOCIAL AND BEHAVIORAL SCIENCE IN THE LAW

Quality science provides the foundation for applications of social and psychological science to the law. To be admissible in court, for example, expert testimony must be legally relevant to the case at hand and scientifically valid (Faigman, 2008). The scientific validity of many social scientists' conclusions is impressive. Contemporary social scientists generally base their understanding of phenomena not on single studies but on large groups of studies that have been submitted to rigorous statistical analysis to examine the magnitude and consistency of their findings across samples and methods.

After a relatively large number of such studies accumulate, scientists may not be able to summarize them reliably merely by reading the studies and giving their impression of the most common findings. Such qualitative, informal summaries are likely to be flawed. Therefore, scientists

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increasingly use quantitative methods to summarize findings across studies. These techniques, known as meta-analysis, involve statistically combining a group of studies to produce a general answer to a question (Lipsey & Wilson, 2001; Schmidt & Hunter, 2004). Because this method clarifies similarities and differences in the findings of related studies, it facilitates the orderly accumulation of scientific facts.

Meta-analyses evaluating the robustness of findings across related studies occasionally establish that findings have not proven to be stable across studies and therefore should be dismissed as having insufficient scientific validity. Alternatively, a finding could prove to be reliable only under certain conditions. Another possible outcome of meta-analyses is that findings prove to be equally strong across a wide range of conditions. A comparison of the generalizability of well-known meta-analytic findings in physics and psychology demonstrated the stability of many findings in psychology (Hedges, 1987). This comparison involved 13 reviews from the Particle Data Group in physics and 13 reviews from psychology (e.g., the effects of teacher expectancy on IQ; the effects of desegregation on educational achievement, sex differences in spatial ability). The psychological reviews proved to be slightly more consistent across studies than the physics reviews, despite evidence of some inconsistencies in both fields. All in all, multiple replications have confirmed many findings in social psychology in particular and the social sciences in general, and quantitative, meta-analytic reviewing has established their robustness. Depending on the specifics of this evidence, such findings can be applied in a wide range of contexts or within a context where they are maximally applicable.

Take the specific case of social psychological research on gender prejudice and stereotyping. Research on explicit and implicit gender prejudice represents an example of scientific research that has been presented to legal fact finders (Eagly & Koenig, 2008; Faigman, Dasgupta, & Ridgeway, 2008; Hunt, Borgida, Burgess, & Kelly, 2002). Research on gender stereotyping shows that the content of stereotypes of men and women differs reliably: women are seen as communal, and men are seen as agentic (Diekmann & Eagly, 2000). Although people do not attribute more positive qualities to men than they do to women (Eagly & Mladinic, 1989), the qualities they ascribe have implications for evaluations of women in the workplace. Specifically, women are evaluated positively when they are thought of in traditional ways (i.e., homemakers, mothers) but not when they work in masculine-typed occupations, such as management (Heilman, 1995). In a situation in which women's presumed qualities do not "fit" the tasks judged necessary for the job, evaluations of their performance are likely to

suffer, and their chances for success are likely to be compromised (Heilman, 1983, 2001). Extensive research has established a reliable scientific relationship between gender stereotypes and a range of work-related outcomes (Eagly & Carli, 2007).

Individuals also may hold implicit gender stereotypes that influence attitudes and behavior. Implicit bias occurs outside an individual's awareness and exists even among individuals who report explicit positive attitudes toward a group (Hofmann, Gawronski, Gschwendner, Le, & Schmitt, 2005). For many years, social psychological researchers have documented implicit bias using experimental designs that conceal the fact that gender is a factor in an individual's decision (e.g., Correll, Benard, & Paik, 2007; Dovidio & Gaertner, 1983). More recently, implicit bias has been measured with reaction-time methods borrowed from cognitive psychology, such as priming tasks and the Implicit Association Test (Fazio & Olson, 2003). Although some claim that reaction-time measures capture stereotypes generally present in the culture rather than individual prejudice, measures such as the Implicit Association Test reliably predict meaningful behavioral outcomes (Greenwald, Poehlman, Uhlmann, & Banaji, 2009).

In addition, scientists have worked toward a more nuanced understanding of gender prejudice and discrimination by identifying moderator variables. Research has identified various circumstances that foster discrimination. Studies have shown, for example, that prejudice and discrimination against women are typically stronger when fewer women occupy a particular type of position (e.g., Davison & Burke, 2000; Eagly, Makhijani, & Klonsky, 1992) or when the cultural stereotype of occupants of the position is more masculine (Eagly & Karau, 2002). Similarly, personnel evaluations consisting of subjective appraisals of workplace performance produce greater discrimination than more objective appraisals (Heilman & Haynes, 2008). In addition, male evaluators generally judge women more harshly than female evaluators do (Eagly, Karau, & Makhijani, 1995; Eagly et al., 1992).

To the extent that such conditions characterize the case in question, discrimination is rendered more understandable from the perspective of the scientific literature. A substantial body of quality science illuminating the moderating conditions that affect the likelihood that discrimination is more or less likely to occur provides the scientific foundation for experts testifying in court to more confidently "rule in" or "rule out" explanatory accounts of the discriminatory behavior in issue. In this way, then, gender prejudice research provides insights into the potential cause(s) of a workplace outcome that would be informative and useful in the legal context. The next few

sections of the chapter present additional topics for which the legal system has developed commonsense assumptions about how people behave: eyewitness behavior, interrogations and confessions, jury selection, pretrial publicity, and expert scientific evidence. Sometimes these assumptions have proven correct; more often, the assumptions have proven flawed.

(UN)RELIABILITY OF EYEWITNESS IDENTIFICATIONS

People are convicted for crimes everyday based solely on the testimony of a single eyewitness who identifies them as the perpetrator of a crime. Yet the evidence continues to mount that eyewitness memory, like other forms of memory, is fallible. For many years, studies of the underlying causes of wrongful convictions have identified mistaken eyewitness identifications as a primary source of error in these cases, with eyewitness errors appearing in 50% to 90% of the cases studied (Borchard & Lutz, 1932; Garrett, 2008; Huff, Rattner, & Sagarin, 1986; Rattner, 1988; Scheck, Neufeld, & Dwyer, 2000; Wells et al., 1998). In most cases, the exonerated men and women have spent many years in prison, with 80% spending more than 5 years in prison and the majority spending more than 10 years (Gross, Jacoby, Matheson, Montgomery, & Patel, 2005). Thus, eyewitness misidentification has significant societal costs.

It would be an error to think that multiple identifications of a suspect would protect against mistaken identifications. In several of the cases in which people have been exonerated by DNA evidence that excludes them as the perpetrator, multiple witnesses—sometimes as many as five witnesses—had identified the exoneree as the perpetrator (e.g., Wells et al., 1998). There are variables, however, that provide information about the relative likelihood that a witness has made an accurate identification. Two types of variables have been proposed: estimator and system variables (Wells, 1978). Estimator variables are characteristics of the crime, the witness, and the perpetrator that are present in the witnessing context and can be used to postdict witness accuracy. System variables, in contrast, are characteristics of the identification procedure that are under the control of the actors in the criminal justice system and are related to eyewitness accuracy.

Much of the research on the effects of estimator and system variables on eyewitness identification accuracy uses a mock witness simulation paradigm in which mock witnesses—generally college undergraduates—watch a simulated crime video in which a perpetrator appears.

Subsequently, witnesses participate in one of several types of identification procedures. A showup consists of the presentation of a single suspect, either in person or via a photograph. This is the most common procedure used in actual crime investigations (Flowe, Ebbesen, Burke, & Chivabunditt, 2001; Gonzalez, Ellsworth, & Pembroke, 1993), but there are limits to its use because the presentation of a single suspect does not allow for an assessment of witness guessing, because any witness guess would result in the identification of the suspect. Showups do not allow for estimating witness guessing and may be more inherently suggestive because they are often conducted with the suspect handcuffed or in the back of a police car. Because of concerns about the suggestiveness of showups, police officers may only conduct showups soon after the crime was committed when their use will shorten the time between the witnessed event and the identification procedure and consequently prevent time from decaying the witness's memory of the perpetrator.

Both live lineup and photo array identification tasks involve the presentation of a suspect and some number of known innocents (referred to as "fillers") to the witness. This procedure is thought to be fairer than a showup because the fillers provide some safeguard against witness guessing, because unbiased arrays should evenly distribute witness guesses across the suspect and multiple fillers. However, actual police lineups and photo arrays are biased against the suspect in that the suspect draws more guesses than do other known innocent members in the lineup (Brigham, Meissner, & Wasserman, 1999; Py, DeMarchi, Ginot, & Wasiak, 2003; Valentine & Heaton, 1999; Wells & Bradfield, 1999).

Although some scholars have advocated for increased study of identifications conducted in the field (Mecklenburg, Bailey, & Larson, 2008), others have noted that laboratory experiments provide information that cannot be gained through the archival study of actual identifications or through field experimentations (Wells, 2008). In laboratory experiments, researchers can manipulate whether the perpetrator is present or absent in the lineup, photo array, or showup. This manipulation allows researchers to assess correct identifications and incorrect rejections of the lineup when the perpetrator is present and mistaken identifications and correct rejections of the lineup when the perpetrator is absent; it also allows researchers to estimate the extent of witness guessing by examining the rates at which witnesses identify fillers. Use of this method can help researchers identify factors that increase the rate of correct identifications and decrease the rate of mistaken identifications.

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Estimator Variables

Researchers have identified a number of variables that affect rates of correct identifications, mistaken identifications, or both. These factors include whether the perpetrator and the witness are of the same race; whether the perpetrator wore something that obscured his face, hair, or hairline; whether the witness had seen the suspect in a context other than as the perpetrator of the crime; whether the witness experienced stress during the witnessed event; whether the perpetrator carried a weapon; and whether the duration of the witness's exposure to the perpetrator and the interval between the witnessed event and the identification task were short or long.

Own-Race Bias

Although witness race does not influence witness accuracy independently nor are perpetrators of particular races more easily identified, witness and perpetrator race interact to affect witness reliability. People are more accurate in their identifications of perpetrators with whom they share racial group membership than of perpetrators who are members of a different racial group. This phenomenon has been termed the own-race identification bias or the cross-race identification effect (Meissner & Brigham, 2001) and it appears across racial and ethnic groups (Ng & Lindsay, 1994; Platz & Hosch, 1988). A meta-analysis of more than 30 studies of the own-race bias suggests that it is robust across a variety of settings and materials (Meissner & Brigham, 2001). The analysis confirms that the effect of the own-race bias on the rate of correct identifications is similar for both Black and White participants but suggests that White participants exhibited a larger own-race bias on the rate of mistaken identifications than did Black participants, although the effect size was significant for both groups. The size of the cross-race effect was moderated by exposure duration and retention interval such that the identifications of witnesses who viewed the perpetrator's face for shorter periods of time and who waited longer to complete the identification task were more prone to exhibit the bias.

Several hypotheses have been offered to explain the own-race bias. Although some have suggested that the effect represents a form of prejudice against outgroup members, the meta-analysis failed to find a relationship between prejudicial attitudes and the size of the own-race bias (Meissner & Brigham, 2001). Another explanation rests on the assumption that people who have greater contact with those of other races will be better able to differentiate among member of those other races. The findings from studies examining group differences in the own-race bias of people who live in integrated neighborhoods versus those who live in racially segregated neighborhoods are

mixed, with some studies supporting the contact hypothesis and others failing to find these group differences (for a review, see Meissner & Brigham, 2001). It may be that one must be practiced in differentiating among members of other-race groups. A study of own-race bias among White basketball fans and those who are not fans found that those who follow professional basketball—and therefore must regularly differentiate among the players who are primarily Black—are less prone to the own-race bias than are those who are not professional basketball fans (Li, Dunning, & Malpass, 1998). In summary, substantial evidence of an own-race bias in the accuracy of eyewitness identifications exists but the psychological mechanisms underlying this bias remain uncertain.

Disguises

It may be commonsense that when a perpetrator wears a disguise such as a ski mask, witnesses have more difficulty making accurate identifications. However, even more subtle obscuring of facial cues, such as covering the hairline with a baseball cap or bandana, may lower witness identification accuracy. In one eyewitness simulation, half of the witnesses viewed a perpetrator who wore a hat that covered his hair and hairline; the remaining witnesses viewed a perpetrator without a hat. Almost twice as many witnesses could correctly identify the perpetrator without a hat from a subsequent lineup than could identify the perpetrator whose hair and hairline were covered (Cutler, Penrod, & Martens, 1987a). A review of six studies ($N > 1,300$ witnesses) examining the effects of a head covering on witness accuracy further supports the finding that perpetrators who cover their hair and hairlines are more difficult to identify than the same perpetrators with bare heads (Cutler, 2006). Alterations in appearance due to glasses, hairstyle, facial hair, and age also make it more difficult for witnesses to make accurate identifications (Read, 1995; Read, Tollestrup, Hammersley, McFadzen, & Christensen, 1990).

Unconscious Transference

Sometimes witnesses are shown a lineup or a photo array that contains a suspect whom they have seen previously but who is not a perpetrator. Perhaps the innocent suspect and the witness both live in the neighborhood in which the crime took place. Perhaps they went to the same school, attended the same party, or frequented the same bar. Because of this previous contact, the innocent suspect appears familiar to the witness, who mistakenly believes that familiarity is due to the fact that the suspect was the person who committed the witnessed crime. Because the witness is not aware that the suspect is familiar because of exposure in a context other than that of the crime perpetrator, this phenomenon is known as *unconscious transference*.

One paradigm that has been used to study unconscious transference has participants witness an event, with some participants viewing only a target/perpetrator and other participants viewing both the target and a bystander to the event (e.g., Read et al., 1990, Exp. 5). All witnesses attempt an identification from a photo array that contains a picture of the bystander and four previously unseen foils (i.e., all arrays are perpetrator-absent). Witnesses who previously saw the bystander are more likely mistakenly to identify the bystander as the perpetrator. In another paradigm, witnesses view a series of mug shots between witnessing an event and making an identification attempt from a photo array or lineup (e.g., Gorenstein & Ellsworth, 1980). Mere exposure to mug shots between witnessing an event and attempting to identify a target face increases the likelihood that a person depicted in a mug shot will be mistakenly identified as the perpetrator of the witnessed event (Brown, Deffenbacher, & Sturgill, 1977).

A recent meta-analytic review provides support for transference effects on eyewitness identification accuracy (Deffenbacher, Bornstein, & Penrod, 2006). A meta-analysis of 32 tests of the mug shot exposure effect confirmed that when witnesses were exposed to mug shots, they were less likely to identify perpetrators correctly when they were present and more likely to identify innocent lineup members than those not exposed to mug shots. These effects were increased if the witness had chosen the person from a mug-shot array and then saw that person in a subsequent identification procedure than if the witness had not committed to choosing the person in the mug shot previously. Moreover, merely viewing mug shots that did not contain depictions of any of the people who appeared in the subsequent identification procedure did not affect witness accuracy. Thus, for mug-shot exposure to produce a negative effect on witness accuracy, it had to allow for transference errors by including an innocent person among the lineup members who also appeared in the mug shots. An additional meta-analysis of 19 tests of studies testing for transference effects using either mug-shot exposure or bystander paradigms also found significant transference effects on the accuracy of eyewitness identifications, but those effects were smaller when using the bystander paradigm.

Stress

Witnesses frequently view crimes under extremely stressful conditions, including circumstances in which their lives are threatened by a weapon-wielding perpetrator. Witnessing crimes is not always stressful; perhaps a witness sees a perpetrator fleeing from a crime scene but does not yet realize that a crime has been committed and therefore has no reason to be scared. Scholars

have tried to examine the effects of stress on accuracy by categorizing witnesses on the basis of the likely stress that they experienced and then examining the rates at which these categories of witnesses identified the suspect in actual lineups (Behrman & Davey, 2001; Tollestrup, Turtle, & Yuille, 1994; Valentine, Pickering, & Darling, 2003). These field studies of the stress-accuracy relationship have provided no clear picture of the relationship between these two variables, perhaps because the categorizations did not accurately reflect differences in experienced stress.

Laboratory studies that manipulate experienced stress are less likely to suffer from this criticism, but they are often criticized because the levels of stress that one can induce in the lab are ethically constrained and likely do not reach the high levels of stress experienced by witnesses whose lives are threatened by actual weapon-brandishing perpetrators. One exception is a study conducted with more than 500 military personnel enrolled in a survival-training program in which participants learn to withstand high-stress interrogations involving physical confrontation (Morgan et al., 2004). Personnel experienced a high-stress interrogation, a low-stress interrogation that lacked physical confrontation, or both types of interrogation and then attempted to identify their interrogators from photo arrays. Irrespective of how the identification procedure was administered (e.g., live vs. photographic, simultaneous vs. sequential), participants who experienced low-stress interrogations were more likely to make a correct identification of their interrogator when the interrogator was present than were participants who experienced high-stress interrogations. However, the stressfulness of the interrogation did not influence the rate of mistaken identifications that the participants made when the interrogator was not present in the lineup.

How does a witness's level of stress influence identification accuracy? For years, the relationship between stress and memory accuracy was hypothesized to produce an inverted U-shaped curve. Low levels of stress would not cause the witness to orient toward the relevant features of the crime, including the perpetrator. Moderate levels of stress would improve accuracy by increasing orienting responses and witnesses' attention to important event details. High levels of stress were hypothesized to interfere with witnesses' cognitive processing, lowering witness identification accuracy (Deffenbacher, 1983). More recent theorizing about the relationship between stress and memory has moved away from a discussion of an inverted-U relationship and instead posited that levels of cognitive and somatic anxiety are important for predicting the stress-accuracy relationship. When levels of cognitive anxiety are high—as would be the case if

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witnesses are aware that they are witnessing a crime, increasing levels of somatic anxiety would at first result in steadily improving performance accuracy. At some critical point, however, the increasing somatic anxiety would cause a precipitous drop in performance (Deffenbacher, 1994).

A meta-analysis of 27 tests of the effects of stress on identification accuracy was recently conducted (Deffenbacher, Bornstein, Penrod, & McGorty, 2004) that included only those studies that contained an experimental manipulation of stress that produced a change in experienced stress as measured either by changes in the physiological state of witnesses or in witnesses' self-reported arousal. This analysis confirmed that stress negatively influences the accuracy of witness identifications. Consistent with the findings from Morgan and colleagues' (2004) study of military personnel in survival training camp, the meta-analysis found that high stress reduced the rate of correct identifications but did not increase mistaken identifications. Stress had a greater negative influence in studies using an ecologically valid eyewitness paradigm compared with studies using more traditional facial recognition paradigms in which participants are shown a number of faces during the study phase and then asked to differentiate between faces they had seen previously and new faces at the recognition phase.

In summary, despite the lack of an effect of stress on target-absent lineups, the decrease in correct identifications means that when considering results from both target-present and target-absent lineups together, lower stress witnessing conditions will produce a higher percentage of accurate identifications than will higher stress conditions. This effect of stress on the mix of accurate and inaccurate identifications will be even greater when the choice of foils biases the witness toward choosing the suspect, as appears to be the case for a significant proportion of lineups and photo arrays used in real cases (Brigham et al., 1999; Py et al., 2003; Valentine & Heaton, 1999; Wells & Bradfield, 1999).

Weapon Focus

Many factors may cause a witness to experience high stress while witnessing an event, including the presence of a weapon. In contrast to other factors that contribute to the stress a witness may experience, weapons have a unique influence on witness accuracy because they tend to draw the attention of the witness, leaving fewer attentional resources to be allocated to the perpetrator's facial and physical characteristics. Thus, when witnesses see a weapon during the commission of a crime, they are less likely to encode the characteristics of the perpetrator's face, negatively influencing their ability to make accurate identification decisions, than when there is no weapon present.

This effect of weapon presence on eyewitness accuracy is known as *weapon focus* (Stebly, 1992).

Laboratory studies of weapon focus have taken one of two forms: witnesses watch a videotaped crime reenactment that manipulates the presence of a weapon—often a gun (O'Rourke, Penrod, Cutler, & Stuve, 1989)—or witnesses experience a live situation that manipulates the presence of an object which could be construed as a weapon (e.g., a syringe; Maass & Kohnken, 1989). The two paradigms complement each other as the videotaped reenactment paradigm allows a presentation of weapons that cannot be ethically presented to participants in live crime simulations. Questions arise, however, about whether the effects obtained in these situations in which the witnesses are not physically confronted with a weapon may differ from the results obtained when witnesses view the weapon *in vivo*. Indeed a meta-analysis of 19 tests of weapon focus found that the effect of weapon presence is significant but is larger in studies that were more ecologically valid (e.g., brief exposure to the perpetrator and long delays between viewing the perpetrator and the memory test) and for studies in which the weapon was a gun (Stebly, 1992), stressing the importance of the use of both paradigms when studying the phenomenon. Studies conducted since this meta-analysis suggest that children are also susceptible to the weapon focus effect (Davies, Smith, & Blincoe, 2008; Pickel, Narter, Jameson, & Lenhardt, 2008).

There are two leading hypotheses regarding the underlying psychological mechanism for the weapon focus effect. From some studies, it appears that the effects of weapon presence may be due to increased attentional focus to the weapon. In one study in which witnesses watched a customer at a fast-food restaurant either point a gun at or hand a check to a cashier, eye-tracking data revealed that witnesses made more frequent fixations on the gun than on the check (Loftus, Loftus, & Messo, 1987). Yet other studies suggest that the unusualness of an object rather than its status as a weapon causes attention to be diverted from the perpetrator to the weapon. In one such study (Pickel, 1998), a simulated crime was filmed in a hair salon; the perpetrator carried an item that varied in how threatening and how unusual it was for the setting. High-threat items were a gun (unusual) and scissors (usual), whereas the low-threat items were a wallet (usual) and a raw chicken (unusual). Threat did not affect participants' memory for the perpetrator, but participants remembered fewer details about the perpetrator when he held an unusual object than a common one. Moreover, carrying a weapon may not always negatively influence participants' reports of information about a target person if the weapon is consistent with the context in which the weapon is viewed (e.g., carried by a police officer rather than a priest or at a shooting range rather than

a baseball game; Pickel, 1999). A recent study suggests that both unusualness and threat contribute to decreased attention to target appearance (Hope & Wright, 2007), reinforcing the conclusion that weapon presence reduces eyewitness accuracy.

Exposure Duration and Retention Interval

Not surprisingly, the amount of time a witness has to view the perpetrator's face (exposure duration) and the length of time between viewing the perpetrator and the subsequent identification task (retention interval) both influence the accuracy of witness identifications. With lengthened exposure to perpetrators, witnesses are better able to encode their physical characteristics and consequently make more accurate identification decisions. Some studies of exposure duration have been conducted using traditional facial recognition paradigms in which participants are shown a large number of faces for varying amounts of time and then view a new set of faces, some of which they have seen before and some of which they have not. In these studies, participants were more accurate making judgments when they had seen the faces for the longer duration (e.g., Laughery, Alexander, & Lane, 1971). Studies using more ecologically valid eyewitness paradigms also find that witnesses are more likely to make a correct identifications and less likely to make false identifications when the exposure duration is longer rather than shorter (e.g., 45 vs. 12 seconds; Memon, Hope, & Bull, 2003). Archival studies of actual crimes generally produce similar results with witnesses identifying suspects more frequently when exposure times were longer as opposed to shorter (Klobuchar, Steblay, & Caligiuri, 2006; Valentine et al., 2003).

Another temporal variable that affects eyewitness accuracy is the retention interval, the amount of time that passes between viewing a crime and the eyewitness identification. Sometimes identification procedures can take place a relatively short time after the crime occurred, as happens when a suspect is apprehended in the neighborhood relatively soon after the crime takes place and is brought to the witness for a showup identification. Sometimes it takes the police days, weeks, months or even years to produce a suspect to place in an identification array. In laboratory and field experiments, shorter retention intervals are related to more accurate identifications (Cutler, Penrod, O'Rourke, & Martens, 1986; Krafska & Penrod, 1985). Archival studies of actual crimes confirm that witnesses identified fewer suspects as the length of time between the crime and the identification procedure increased (Behrman & Davey, 2001; Tollestrup et al., 1994).

A quantitative meta-analysis of facial recognition research—including eyewitness identification research—supports the robustness of the effects of both exposure time

and retention interval on eyewitness accuracy (Shapiro & Penrod, 1986). Across eight tests of the effects of exposure time on rates of correct identifications, exposure time had a moderate to large effect ($d = .61$). As expected, longer exposure times produced more correct identifications. The effect of exposure time was smaller for mistaken identifications ($d = .22$), but again shorter exposure times produced more false identifications. Similarly, the meta-analysis supported the proposition that retention intervals are related to increased identification accuracy. Across 18 tests of the effects of retention interval on the rate of correct identifications, retention interval was negatively related to correct identifications ($d = .43$). Their analysis of 14 tests of the effects of retention interval on rates of mistaken identifications demonstrated a small to moderate effect of retention interval ($d = .33$) with longer intervals resulting in more mistakes.

Thus, overall, a variety of characteristics associated with the conditions present when someone witnesses a crime—disguises, stress, weapon presence, exposure to other faces in temporal contiguity to the crime, and whether the witness and perpetrator belong to the same racial and ethnic group—influence the accuracy of the witness identifications of perpetrators. When estimating the likely accuracy of a witness, it is helpful to know whether any of the features that decrease witness accuracy were present. However, studying these estimator variables provides no information about how to increase the accuracy of eyewitnesses because actors in the criminal justice system cannot control whether they are present when someone witnesses a crime. To help improve the quality of eyewitness identifications that may subsequently be entered into evidence against a defendant, it is important to identify variables associated with the collection of eyewitness identification evidence that investigators can control.

System Variables

System variables are those features of a lineup administration that are under the control of actors in the criminal justice system. An identification task can be thought of as an experiment in which the hypothesis being tested is that the suspect and the perpetrator are the same person (Wells & Luus, 1990). The same types of experimental features that can lead to problems in drawing inferences from experiments also make it problematic to infer that a suspect is the perpetrator even when a witness positively identifies the suspect. For example, the instructions given by the lineup administrator might bias the witness toward supporting the hypothesis that the suspect is the perpetrator. Administrators may unintentionally leak their hypothesis to witnesses, or they may interpret the witnesses' responses

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in a manner that is consistent with their hypothesis. The materials (e.g., the lineup members) may contain demand characteristics that communicate the hypothesis (Wells & Luus, 1990). These observations have led researchers to focus their efforts on studying the effects of lineup composition, lineup instructions, the method of presenting lineup members to witnesses, and administrator behavior on witness accuracy.

Lineup Composition

The method of selecting fillers for photo arrays and lineups can influence the accuracy of witnesses. There are two primary methods for selecting fillers to pair with a suspect. Using the first approach, known as "match-to-suspect," an administrator would select fillers that resemble the suspect. In the second approach, known as "match-to-description," the administrator selects fillers that match the description that the witness gave of the perpetrator. The fillers must possess any characteristic that is mentioned in the witness's description but may vary on characteristics that were omitted from the description. Thus, if the witness reports that the perpetrator had brown hair, all lineup members must have brown hair, but their hair may be straight or curly because this is not a characteristic mentioned by the witness.

Researchers have promoted the match-to-description method because it reduces the likelihood that witness guessing or deduction would result in the false identification of an innocent suspect. Imagine a lineup in which the suspect is the only lineup member who matches the description of the perpetrator given by the witness. In that situation, a witness may deduce who the suspect is and choose that person on the basis of their deduction paired with an assumption that the police have placed the suspect in the lineup for a reason (e.g., other evidence implicates the suspect in the crime). Match-to-description lineups are thought to guard against this type of deduction because all of the lineup members will match the witness's description. In match-to-suspect lineups, lineup members may match the suspect on features that were omitted from the description but fail to match the description on included features, leaving open the possibility that the witness could deduce the suspect's identity on the basis of his or her match to the description.

Match-to-description photo arrays appear to increase correct identifications and decrease mistaken identifications in comparison to match-to-suspect arrays. In one study (S. Clark & Tunnicliff, 2001), 30 minutes after watching a staged crime, undergraduates viewed one of three types of photo arrays: a culprit-present array in which the fillers were matched to the culprit, a culprit-absent array in which the fillers were matched to the innocent suspect, or a culprit-absent array in which fillers were matched to the

culprit. When the photo array did not contain the perpetrator, witnesses were more likely to identify the innocent suspect from the photo array in which the fillers were matched to the suspect than from the array in which the fillers were matched to the culprit. Thus, matching the fillers to the suspect seems to increase the likelihood of mistaken identifications of innocent suspects.

In a more direct test of the benefits of the match-to-description method of selecting foils, researchers constructed individual photo arrays for each witness to a staged theft (Wells, Rydell, & Seelau, 1993). Depending on the condition, they either matched fillers to the suspect or to the witness's description of the perpetrator or they selected fillers that did not match the witness's description of the culprit. The researchers also varied whether the culprit was present in the lineup. When the perpetrator was present, witnesses were more likely to make correct identifications when they viewed an array in which the fillers were chosen on the basis of whether they matched the witness's description of the perpetrator than when the fillers were matched to the suspect. Although there was no difference between the proportion of witnesses who made correct identifications from arrays that were chosen to match or mismatch the witnesses' description of the culprit, deduction may have played a role in identifications from the lineups chosen to mismatch the description. In contrast, when the culprit was absent from the photo spread, witnesses were more likely to make false identifications when they viewed the arrays in which the fillers did not match the description than when they viewed either the suspect-matched or the match-to-description arrays. Overall, it was the arrays in which filler selection was based on a match-to-description strategy that produced the best ratio of correct identifications to false identifications.

Lineup Instructions

The instructions that lineup administrators give to witnesses who are about to view a lineup or photo spread have the potential to influence the likelihood that a witness will make a choice from the lineup (i.e., make a positive identification). When lineup administrators deliver instructions suggesting that the perpetrator is one of the lineup members or that the witness is expected to make a positive identification, witnesses are more likely to make a choice from the lineup. In contrast, when a lineup administrator instructs the witness that the perpetrator may not appear in the lineup, witnesses are less likely to make a positive identification (Stebly, 1997). In an early laboratory study of the effects of lineup instruction, witnesses who heard biased instructions ("We believe that the [perpetrator] is present in the lineup. Look carefully at each of the five individuals in the lineup. Which

of these is the person you saw?") were more likely to choose a lineup member than were witnesses who heard unbiased instructions ("The [perpetrator] may be one of the five individuals in the lineup. It is also possible that he is not in the lineup."; Malpass & Devine, 1981). Similar results have been obtained in other laboratory studies (e.g., Cutler, Penrod, & Martens, 1987b).

In the first quantitative meta-analysis of the literature, 19 tests of the effect were examined (Stebay, 1997). The meta-analysis found that biased instructions produce more false identifications than unbiased instructions when the perpetrator is absent from the lineup but that instruction suggestiveness does not affect correct identification rates. A qualitative reanalysis of the studies in this meta-analysis reached a somewhat different conclusion: unbiased instructions produce a small decrease in correct identifications from culprit-present photo spread (S. E. Clark, 2005). Despite this different conclusion about whether instruction suggestiveness significantly influence correct identification rates, both authors agreed that the size of any decrease in correct identifications from culprit-present arrays due to the use of unbiased instructions is far smaller than the decrease in mistaken identifications from culprit-absent arrays.

Lineup Administration

Typically, the lead investigator will be the person who administers the lineup or photo spread to the witness. Thus, administrators of lineups generally know which member of the lineup is the suspect, and this knowledge has the potential to influence their behavior in a way that might steer the witnesses toward the suspect and away from fillers. This change in behavior need not be intentional to exert influence on the witness (Greathouse & Kovera, 2009). When lineup administrators know the identity of the suspect in a lineup, this is called a single-blind administration, because the witness is blind to the suspect's identity but the lineup administrator is not. In contrast, double-blind administration refers to the situation in which an investigator who is not involved in the case and does not know the identity of the suspect administers the lineup or photo spread because both the witness and the administrator are blind to the suspect's identity. Of course, the idea that investigators can communicate—intentionally or unintentionally—their expectations to participants and that these expectations influence participants' behavior is a well-established principle of research methods in psychology (Rosenthal, 2002). Several laboratory studies have confirmed that when lineup administrators know the suspect's identity and therefore have a hypothesis that the witness will choose the suspect, the witness will indeed choose the suspect more frequently when there is contact between the administrator and the witness (Greathouse & Kovera, 2009; Haw & Fisher,

2004; Phillips, McAuliff, Kovera, & Cutler, 1999; Russano, Dickinson, Greathouse, & Kovera, 2006).

Most recently, researchers have begun to examine whether other features of the lineup administration moderate the effects of investigator knowledge of the suspect's identity on witness accuracy (Greathouse & Kovera, 2009). Specifically, do other variables—such as biased instructions and simultaneous presentation—that increase the likelihood a witness will make a positive identification (i.e., choose) from a lineup also increase the effect of investigator knowledge? If an administrator fails to warn a witness that the culprit may not be in the lineup, might a witness who is prone to guessing because of lineup features that promote choosing search the administrator's behavior for cues to the suspect's identity?

Research suggests that the answer is yes. When witnesses received biased instructions and viewed simultaneous lineups, they were more likely to identify suspects in single-blind than in double-blind lineups (Greathouse & Kovera, 2009). This effect obtained irrespective of whether the target was in the lineup or not. The pattern of witnesses' identifications of fillers and suspects suggests that the increase in mistaken identifications due to administrator knowledge of the suspect's identity was the result of a shift in filler identifications to suspect identifications. That is, witnesses rejected the lineups (i.e., made no identifications) equally often in single- and double-blind lineups; however, filler identifications that were made under double-blind conditions were redistributed to suspect identifications under single-blind conditions. Essentially, administrator knowledge did not sway those who believed the perpetrator was not present but shifted those who had made filler identifications to suspect identifications, especially under conditions that would promote guessing (and a higher rate of filler identifications under double-blind conditions). Moreover, the diagnosticity of identifications made when the administrator was blind to the suspect's identity was twice that of identifications made when the administrator knew whom the suspect was. These findings provide support for the recommendations that all lineups be conducted with blind administrators (e.g., Wells et al., 1998).

Lineup Presentation

Most lineup administrators present lineup members or photos from a photo spread to witnesses simultaneously and ask the witness whether the perpetrator is among the people presented (Wogalter, Malpass, & McQuiston, 2004). In contrast, some scholars have suggested that a preferable method of presentation is to show the photos or lineup members to witnesses one at a time, asking witnesses to make a judgment as to whether the person is the perpetrator after each presentation (Wells et al., 1998). This second method of presentation is known as a sequential lineup.

In the first study of the relative merits of simultaneous and sequential lineups, witnesses to staged thefts attempted eyewitness identifications from culprit-absent or culprit-present photo spreads (Lindsay & Wells, 1985). Half of these arrays were presented simultaneously, and the other half were presented sequentially. When the culprit was present in the lineup, the rate of correct identifications was similar for simultaneous and sequential lineups. However, when the culprit was absent from the lineup, witnesses were more likely to make false identifications from simultaneously than from sequentially presented lineups. In part, the increase in false identifications is due to the fact that witnesses are more likely to make positive identifications (i.e., choices) from simultaneous lineups (Meissner, Tredoux, Parker, & MacLin, 2005).

This basic sequential superiority effect has been replicated by others (e.g., Cutler & Penrod, 1988) and confirmed in a meta-analysis of the studies testing the effects of presentation style on witness accuracy (Stebly, Dysart, Fulero, & Lindsay, 2001). This meta-analysis revealed that witnesses were more likely to make correct identifications from simultaneous lineups than from sequential lineups when the culprit was present in the lineup. When the culprit was not present in the identification procedure, witnesses who viewed sequential lineups were more likely to decide that the perpetrator was not there and less likely to make mistaken identifications than witnesses who viewed simultaneous lineups and photo spreads. Although the sequential procedure does reduce correct identifications, the reduction is small and much smaller than the reduction in false identifications produced by sequential presentation.

Despite these demonstrated benefits of sequential presentation, some have begun to question whether the push to make sequential presentation standard operating procedure is wise. Some scholars have argued that the push to change public policy is premature given that the psychological mechanisms underlying the sequential superiority effect are not well understood (McQuiston-Surrett, Malpass, & Tredoux, 2006). These authors are especially concerned about understanding whether certain features such as backloading of pictures (i.e., not letting the witness know how many people are in the photo spread), the stopping rule (i.e., whether photo presentation stops when a witness makes an identification or does the presentation continue until the witness sees all the photos), and the treatment of multiple identification decisions are necessary for the benefits of sequential presentation to obtain.

Practitioners have also questioned the benefits of sequential presentation based on the results of the Illinois Pilot Program on Sequential Double-Blind Identification Procedures (Mecklenburg, 2006), a field experiment

in which researchers randomly assigned the identification procedures that police would use when conducting a lineup. The study compared double-blind sequential lineup administration with single-blind simultaneous administration and found that witnesses were more likely to identify suspects and less likely to identify fillers from single-blind simultaneous lineups. Although conducting a field test of these methods is admirable, this pilot study is seriously flawed for several reasons. First, equating identifications of the suspect with correct identifications is unwise because in the field, the accuracy of an identification is unknown unless DNA evidence confirms the identification. Second, the researchers confounded the manipulation of lineup presentation with the manipulation of blind presentation, rendering it difficult to make valid inferences about the effects of either variable (Schacter et al., 2008). Finally, although filler identifications are mistakes, it is possible that officers record the same witness behaviors toward fillers differently depending on whether they are blind to who are the fillers (Wells, 2008). For example, a witness who says, "I think it may have been Number Three, but I am not sure" may be recorded as a filler identification by a police officer who is blind to the suspect's identity and therefore does not know whether the uncertain identification is of the suspect or a filler but as a nonidentification by a nonblind police officer because of the witness's uncertainty. Because of these concerns about the methods of the pilot study, many commentators have cautioned against the use of these data to support public policy (Diamond, 2008; S. J. Ross & Malpass, 2008; Schacter et al., 2008; Steblay, 2008; Wells, 2008), especially in light of the controlled laboratory studies that routinely find that sequential presentation reduces mistaken identifications.

Showups Versus Lineups

When planning to collect identification evidence from witnesses, investigators must make a choice between conducting a showup, in which a witness views a single suspect and makes an identification decision, or a procedure in which the witness views the suspect along with a group of known innocent fillers (i.e., a lineup or photo array). Generally, an investigator will choose to conduct a showup when a suspect is found quickly near the area where the crime was committed. Under those circumstances, the suspect is brought to the witness, or the witness is brought to the location where the suspect is being held. On occasion, investigators will present a single photo to a witness. The potential suggestiveness of showup is a concern because, unlike a lineup, any positive identification results in the identification of the suspect, so there is no method of assessing the extent to which a witness is guessing as is the

case when the lineup contains fillers. Courts have upheld the use of showup when they are conducted within 30 minutes of the crime, arguing that the short retention interval contributes to greater witness accuracy and that waiting for the construction of a lineup would eliminate this advantage (*People v. Brnja*, 1980; *Singletary v. United States*, 1978). Moreover, because a showup can be thought of as the first photo in a sequential photo spread, with no other photos to follow, some might argue that the data suggesting that witnesses are less likely to make false identifications from sequential lineups would support a hypothesis that showups are less likely to produce false identifications than simultaneous lineup procedures.

Do accuracy rates differ between showups and lineups? One study found that witnesses made more correct identifications from showups than from lineups when the culprit was present in the identification task but they also made more than twice the number of false identifications (Yarmey, Yarmey, & Yarmey, 1996). Longer retention intervals and clothing bias (when the innocent suspect wears clothing similar to the perpetrator's) exacerbated problems associated with showups (Yarmey et al., 1996). A meta-analysis of field and laboratory research testing the effects of showups versus lineups on witness accuracy confirmed these findings (Stebly, Dysart, Fulero, & Lindsay, 2003). Witnesses made fewer choices (i.e., positive identifications) from showups than from lineups and photo spreads. Because witnesses made fewer choices from showups, they were also more likely to reject correctly a culprit-absent showup than a culprit-absent lineup or photospread. Although overall, showups produced the same rate of correct and false identifications as lineups and photospreads, showups were most likely to produce false identifications when the innocent suspect more closely resembled the perpetrator. Thus, it appears that showups are more dangerous under some conditions (e.g., longer delays, the suspect looks like the perpetrator or is wearing similar clothes) than others. Because in real cases it is unknown whether some of these conditions are present, it would be safer to conduct lineups to avoid any chance of increasing false identifications.

Postdictors of Witness Accuracy

Those in a position to postdict witness accuracy (e.g., police officers, prosecutors, judges, jurors) may use another class of variables in addition to estimator and system variables. These variables include witness confidence, the accuracy of a witness's description of a perpetrator, witness consistency, and identification speed. Of these variables, identification speed is the most promising.

Witness Confidence

U.S. case law specifies that judges should consider the confidence with which witnesses make their identification of a lineup member when evaluating whether the identification resulting from suggestive lineup procedures is reliable despite the circumstances under which it was obtained (*Manson v. Braithwaite*, 1977). Judges may instruct jurors to consider witness confidence when evaluating the weight to give eyewitness identification evidence when arriving at a verdict (*United States v. Telfaire*, 1978). But does witness confidence deserve the status given to it by these legal decisions? Is witness confidence an accurate predictor of witness accuracy?

There are numerous studies that measure both witness confidence and witness accuracy. The most comprehensive of the meta-analyses that examine the relationship between witness confidence and accuracy (Sporer, Penrod, Read, & Cutler, 1995) found a relatively weak relationship between these variables; however, the relationship was much stronger for choosers (e.g., witnesses who made a positive identification from the lineup) than for nonchoosers (e.g., witnesses who rejected the lineup). Viewing conditions also moderated the confidence-accuracy correlation. Better, more optimal viewing conditions produced higher confidence accuracy correlations than did less optimal viewing conditions, according to the *optimality hypothesis* (Deffenbacher, 1980).

One reason that confidence may be only weakly related to witness accuracy is that witness confidence appears mutable. The confidence-accuracy relationship is stronger when confidence data are collected immediately after the identification than when witnesses have the opportunity to receive information that confirms or disconfirms their identification. In the first investigation of confidence malleability, pairs of witnesses viewed a staged theft (Luus and Wells, 1994). If witnesses heard that their cowitness identified the same person whom they identified, their confidence in the accuracy of their identification increased. In contrast, if witnesses heard that their cowitness had identified a different lineup member or had failed to choose anyone from the lineup, their confidence in the accuracy of their identification decreased.

Additional studies have demonstrated that feedback not only alters witnesses' confidence in the accuracy of their identification but also alters their reports of the conditions under which they viewed the crime. In one study (Bradfield, Wells, & Olson, 2002), confirming feedback not only reduced the accuracy-confidence correlation, it also caused witnesses to report that they had a better view of the perpetrator, paid more attention to the video, had a better basis for their identification, made their

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identification more easily, were more willing to testify, and had a better image of the perpetrator's face in their mind than did witnesses who did not receive feedback. An archival study of actual crimes shows a similar effect of feedback on reported viewing conditions (Wright & Skagerberg, 2007).

A meta-analysis of 20 studies of the effects of postidentification feedback on confidence malleability reveals that this effect is robust and quite large ($d = .79$), with confirming feedback increasing confidence and disconfirming feedback decreasing confidence. The effects of postidentification feedback on witnesses' reports of how good a view they had and how much attention they paid to the perpetrator were somewhat smaller but still moderate effects according to conventions of evaluating effect sizes. These effects are problematic given that case law in *Manson v. Braithwaite* (1977) requires that judges who are ruling on the admissibility of an identification obtained using suggestive procedures may rule in favor of admissibility if they judge the identification to be reliable despite the suggestiveness of the procedures. Three of the criteria that judges are to use when evaluating the reliability of the identification are the very variables that are altered by postidentification feedback: how good of a view the witness had, how much attention the witness paid to the perpetrator, and witness confidence (Wells & Quinlivan, 2009).

Witness Description Accuracy

When defense attorneys consult with experts on eyewitness identifications, they will often argue that their client must be innocent because the witness gave a description of the perpetrator that does not match their client. To what extent are witness descriptions related to identification accuracy? Are the identifications of witnesses who provide complete descriptions of the perpetrator, a large number of accurate details, a small number of inaccurate details, or descriptions that are congruent with the identified suspect any more accurate than the identifications provided by witnesses who provide poorer descriptions?

In one field study, an experimenter visited banks and approached tellers to deposit blatantly altered money orders (Pigott, Brigham, & Bothwell, 1990). When the tellers refused to cash the money orders, the experimenter became quite angry and departed. Later that same day, a different experimenter posed as an investigator and collected descriptions and identifications from the clerks. After coding the descriptions for accuracy, completeness, and congruence, the experimenters tested whether these features of the descriptions correlated with identification accuracy. All of the correlations were positive, but none were significant. Subsequent studies have

replicated the weak to nonexistent relationship between witness descriptions and witness accuracy (Susa & Meissner, 2008).

Witness Consistency

Litigation manuals suggest that trial attorneys attempt to extract inconsistencies from witnesses when they testify and to impeach the witness with these testimonial inconsistencies. This strategy is not particularly difficult to practice given that witnesses are normally interviewed multiple times over the course of an investigation, and these multiple accounts of the witnessed event are likely to contain different details about the event and the perpetrator. But to what extent do these inconsistencies in eyewitness reports relate to identification accuracy?

In four studies that examined the relationship between identification accuracy and the consistency of eyewitness reports, witnessed viewed staged thefts and then provided descriptions of the perpetrators—sometimes multiple perpetrators for each theft—and the events at two times (Fisher & Cutler, 1996). At the end of each interview, the witnesses attempted identifications of the culprits from photo spreads, videotaped lineups, or live lineups. The researchers coded these descriptions for consistency across the two interviews and then correlated witness consistency with witness identification accuracy. The correlations were small, and all but one was nonsignificant, suggesting that witness consistency is not a particularly good predictor of witness accuracy.

Identification Speed

Sometimes when witnesses view a photo spread or a lineup, they immediately identify one of the lineup members as the perpetrator. At other times, witnesses may deliberate on the lineup members and take a relatively long time to make a positive identification. Does response latency prove to be a significant predictor of identification accuracy? Four crime simulation studies suggest that those witnesses who make their identifications within 10 to 12 seconds of viewing a photo spread are more accurate than those witnesses who take more time before making a choice (Dunning & Perretta, 2002). In one of these studies, undergraduates viewed a videotape of a purse snatching. The witnesses wrote their descriptions of the events and attempted to make identifications from culprit-absent and culprit-present photo spreads. Witnesses who made correct identifications did so more quickly than did those witnesses who made false identifications. Subsequent research has not supported the 10- to 12-second decision rule but did support the inverse relationship of response latency and identification accuracy (Weber, Brewer, Wells, Semmler, & Keast, 2004). There was no relationship between the time

witnesses took to reject a lineup and the accuracy of that decision.

Identification speed and accuracy may also be related in real crimes (Valentine et al., 2003). In their study of 600 identification attempts by 600 witnesses to real crimes, Valentine and colleagues found that witnesses who police officers dubbed “fast” choosers were more than twice as likely to choose the suspect from the lineup than were witnesses labeled “average” or “slow” choosers. Identification speed was unrelated to witnesses’ known mistakes (i.e., choices of fillers from the lineups). Although investigators’ knowledge of the suspect’s identity could have influenced their judgments of identification speed—artificially creating a relationship between speed and accuracy—the consistency of these findings with those from laboratory simulations suggest that faster identifications may be more accurate.

In conclusion, the commonsense notion that eyewitnesses to crime will reliably identify perpetrators is untenable. Eyewitnesses are not as reliable as jurors believe them to be (Cutler, Penrod, & Dexter, 1990; Schmechel, O’Toole, Easterly, & Loftus, 2006). Characteristics of events that interfere with witness memory for faces—targets and perceivers from different racial or ethnic groups, weapon presence, stress, disguised target faces—are often present in witnessed crimes. Although research supports the adoption of several procedural changes (e.g., instructions warning the witness that the perpetrator may not be in the lineup, sequential presentation, double-blind administrators), many in the position to implement these changes have resisted doing so (e.g., Mecklenburg, Bailey, & Larson, 2008). Without the implementation of these evidence-based procedures, juries likely will continue to wrongfully convict at least some defendants because of mistaken eyewitness identification evidence.

INTERROGATIONS AND CONFESSIONS

Just as there are many crimes for which no DNA evidence links a perpetrator to a crime, there are also crimes for which the only witness—or at least the only surviving witness—is the perpetrator. In cases in which there are no eyewitness and no physical evidence to identify the perpetrator, a confession from the suspect may be the only evidence linking a suspect to the crime (Kassin, 2005). Confessions, like eyewitness identifications, represent a powerful form of evidence; jurors rarely question the validity of an obtained confession (Kassin & Sukel, 1997).

But are confessions always reliable? Are there circumstances under which an innocent suspect will confess to committing a crime that was perpetrated by someone else?

Reviews of DNA exonerations of the wrongfully convicted suggest that 15% to 20% of the exonerated had falsely confessed to the crimes (Garrett, 2008; Scheck et al., 2001). People have even falsely confessed to committing extremely horrific crimes, including five boys who falsely confessed to raping and brutally beating a jogger in Central Park (Kassin & Gudjonsson, 2004). These confessions were not elicited with brutal beatings or threats of harm; instead, more subtle psychological factors seem to be at work in most false confession cases. A number of variables increase the likelihood of false confessions, including the tendency for innocent suspects to waive the legal protections to which they are entitled (e.g., *Miranda* rights), interrogators’ presumption of the suspect’s guilt, and problematic interrogation tactics (Kassin & Gudjonsson, 2004). Moreover, after a defendant’s confession is entered into evidence, jurors are swayed by the defendant’s admission of guilt even when evidence exists that the confession may have been obtained under dubious circumstances (Kassin & Sukel, 1997).

Innocence and Waiver of Legal Rights

Suspects in criminal investigations have a variety of constitutional rights intended to protect them. Among these rights is the Fifth Amendment right to freedom from coercion intended to elicit self-incriminating statements. In *Miranda v. Arizona* (1966), the Supreme Court held that investigators must warn suspects of these rights (e.g., the right to remain silent, the right to an attorney). Suspects must voluntarily waive these rights before issuing a self-incriminating statement if the state wishes to use this statement in the prosecution of that suspect. *Miranda* has its critics, with some arguing that it ties the hands of investigators and prosecutors, reducing the number of confessions they obtain, with the consequence that sometimes potentially dangerous offenders are returned into society (Cassell, 1996a, 1996b; Cassell & Hayman, 1996). Others have argued that *Miranda* has important benefits to society because it encourages more humane police practices and has informed citizens of their constitutional rights (Leo, 1996a).

Relatively little research informs these debates about *Miranda*, but a small body of research speaks to whether these warnings protect innocent suspects from bad outcomes in criminal prosecutions. For *Miranda* warnings to be effective, people must understand these warnings and when they waive their rights, suspects must do so “voluntarily, knowingly, and intelligently” (*Miranda v. Arizona*, 1966). Three competencies are required for this test to be met (Grisso, 1981). First, suspects must understand the words in the *Miranda* warnings. Second, suspects must

understand the intended protections provided by *Miranda*, including that the assistance of counsel will protect them from the adversarial nature of an interrogation. Third, suspects must have the ability to think reasonably about the consequences that will arise from waiving or preserving their constitutional rights.

A variety of factors influence suspects' competencies to waive their *Miranda* rights, including age (Feld, 2006; Grisso, 1998; Oberlander & Goldstein, 2001; Viljoen & Roesch, 2005; Viljoen, Zapf, & Roesch, 2007), psychiatric status (Cooper & Zapf, 2008), and intelligence (Fulero & Everington, 1995; O'Connell, Garmoe, & Goldstein, 2005), with children under age 14, those experiencing psychiatric symptoms, and suspects who are mentally retarded being less likely to show competence in these areas than older children, adults, and those of normal intelligence and with no psychiatric symptoms. Indeed, children who are 15 or younger are more likely than older children to waive their rights and to confess, and their decisions to do so appear unrelated to the strength of the evidence against them (Viljoen, Klaver, & Roesch, 2005). Although some have criticized the assessment tools used to establish the deficiencies in these populations' abilities to competently waive their *Miranda* rights (Rogers, Jordan, & Harrison, 2004), the content of *Miranda* warnings may be responsible for some of the difficulties these groups experience, with warnings used ranging in reading level from Grade 2.2 to postgraduate (Rogers, Harrison, Shuman, Sewell, & Hazelwood, 2007; Rogers, Hazelwood, Sewell, Shuman, & Blackwood, 2008).

Although *Miranda* comprehension is important, an examination of live and videotaped interrogations suggests that approximately 80% of suspects waive their rights (Leo, 1996c). The police appear to achieve these high rates of waiver by establishing rapport, presenting themselves as sympathetic to the suspect's plight, and minimizing the importance of the rights to be waived (Leo, 1996c). Who waives their rights? Those who have no previous history with the criminal justice system (i.e., no police record) are more likely to waive their rights than are suspects who have criminal justice histories (Leo, 1996b). This disparity suggests the possibility that innocent suspects, who are less likely to have previous records than suspects who actually committed crimes, may be especially likely to waive their rights (Kassin, 2005). However, it is difficult to draw any strong causal conclusions from these data.

Kassin and Norwick (2004) developed a mock-crime paradigm to provide an experimental test of the hypothesis that innocent suspects may be more likely to waive their *Miranda* rights. The "guilty" participants were to enter a nearby room, open a drawer, and take \$100 from the drawer. The "innocent" participants entered the room, opened and

shut the drawer, but did not take any money. After leaving the room, all participants were taken to another room where they met a "detective" who was blind to whether the participant had taken the money. The detective informed the participant that he was there to question the participant about some stolen money, but first he needed the participant to sign a *Miranda* rights waiver form. It did not matter whether the detective acted kindly or aggressively; the demeanor of the detective did not influence participants' willingness to waive their *Miranda* rights. In contrast, innocence significantly predicted whether participants signed away their constitutional rights, with innocent suspects waiving their rights at a 2:1 ratio to guilty suspects.

What reasons did participants provide for relinquishing these protections? Guilty participants were afraid they would appear guilty if they did not cooperate. Although innocent participants mentioned this concern as well, they were more likely to report that they waived their rights precisely because they were innocent and had nothing to hide (Kassin & Norwick, 2004). Mock suspects in another study believed that others who watched their denials of guilt would accurately judge whether they are guilty or innocent (Kassin & Fong, 1999). Thus, it appears that the very people who need protection—the innocent—are more likely to surrender those protections voluntarily (Kassin, 2005).

After suspects have waived their *Miranda* rights, the interrogation may commence; yet interrogators seem to conclude that suspects are guilty before the interrogation even starts. Joseph Buckley (2004), one of the authors of the leading manuals of interrogation techniques (Inbau, Reid, Buckley, & Jayne, 2001), is reported to have said that he was unconcerned about whether his techniques might promote false confessions from innocent people because "we don't interrogate innocent people" (Kassin & Gudjonsson, 2004, p. 36). How can interrogators be so certain that everyone they interrogate is guilty? The popular Reid technique encourages detectives first to conduct a preinterrogation interview with the suspect to allow the investigator to determine whether the suspect is guilty and trains interrogators to examine verbal and nonverbal behaviors for cues to deception. The interrogator may make this determination of suspect guilt on the basis of whether the suspect fits a particular profile (e.g., an unfaithful husband to a murdered wife; Davis & Follette, 2002; Wells, 2003), whether the suspect displays behavioral cues that the Reid technique manual claims indicate deception (Inbau et al., 2001), or merely a suspicion developed by the investigator (Kassin & Gudjonsson, 2004). This technique raises two questions. First, how accurate are people, including interrogators, at judging whether someone is being deceptive? Second, after an investigator determines that a suspect is being deceptive in his or her denials of guilt, how does the

interrogators' presumption of guilt influence the remainder of the interrogation process?

Deception Detection

If the first task of an interrogator is to judge whether the denials proffered by a suspect in a preinterrogation interview are true or false, then understanding how well people—including investigators—detect deception is important. Both laypeople and scholars expect that liars will experience guilt while lying and that truth-tellers will not experience guilt, that guilt will engender nervousness and discomfort in the liar, and that this nervousness and discomfort will manifest itself in behavior (DePaulo & Morris, 2004). Similarly, people who are lying may not be able to create a story that is as compelling and free of contradictions as someone who is telling the truth (DePaulo & Morris, 2004). A meta-analysis of studies testing behavioral cues—both verbal and nonverbal—to deception suggests that there are behaviors that help distinguish liars from truth-tellers, including pupil dilation, tension, and voice pitch; however, other behaviors that are thought to be related to lying (e.g., fidgeting, blinking) were not reliably related to deception (DePaulo et al., 2003). Despite the existence of behavioral cues to deception, people perform at chance levels when asked to evaluate whether someone is lying or telling the truth (Ambady & Weisbuch, this volume; Bond & DePaulo, 2006; Leach et al., 2009; Memon, Vrij, & Bull, 2003; Vrij, 2000; Zuckerman, DePaulo, & Rosenthal, 1981).

Perhaps experience with lie detection improves one's ability to differentiate truth from lies. Yet people who have jobs that require deception detection ability do not appear to have special abilities to detect lies, with customs inspectors, judges, mental health professionals, police investigators, and polygraph examiners detecting deception at rates only slightly better than chance (Bull, 1989; DePaulo, 1994; DePaulo & Pfeifer, 1986; Ekman & O'Sullivan, 1991; Elaad, 2003; Garrido & Masip, 1999; Garrido, Masip, & Herrero, 2004; Koehnken, 1987; Leach, Talwar, Lee, Bala, & Lindsay, 2004; Porter, Woodworth, & Birt, 2000). Although some scholars have claimed that they have identified a small number of lie-detection "wizards" (O'Sullivan, 2005, 2007), some have questioned whether these people with special lie-detecting abilities exist (Bond & Uysal, 2007), whereas others have failed to find a reliable individual difference in deception-detection ability (Leach et al., 2009).

Experience may not be the best measure of ability. After all, someone can practice a skill, but without any feedback about how one is performing, practice may have no positive effect on one's abilities. Perhaps those

who are in professions that practice lie detection have been attempting lie detection without receiving any special training for how best to do it. Although one might suspect that people who are trained to detect deception may prove to be more adept at differentiating liars from truth-tellers, evaluations of training programs suggest that they improve lie detection performance very little (Bull, 1989; Kassin & Fong, 1999; Porter et al., 2000; Vrij, 1994; Zuckerman, Koestner, & Alton, 1984). For example, undergraduates trained in the Reid technique for deception detection before they watched videotapes of the interrogations of fellow students, half of whom had committed one of four mock crimes (breaking and entering, vandalism, shoplifting, computer break-in) and the other half who had committed similar but noncriminal acts (Kassin & Fong, 1999). Consistent with the general literature on deception detection, these observers were not able to differentiate reliably between guilty and innocent suspects, performing at levels that did not significantly differ from chance. What is surprising is that trained observers did no better than untrained observers at discriminating between truth-tellers and liars; they were actually worse at detecting deception but more confident in their deception judgments. Rather than providing observers with skills that would aid deception detection, the training caused observers to see deception more frequently.

In a follow-up study using the same materials, police officers' deception detection performance was no different from chance, but they were more confident in the accuracy of their judgments than were the untrained college students, suggesting that interrogators may have a bias toward presuming deceptiveness in suspects (Meissner & Kassin, 2002). These results were confirmed with a meta-analysis of the literature examining the effects of training—with training assumed by testing different groups who varied on whether lie detection was a skill needed in their profession or manipulated—on deception detection and bias (Meissner & Kassin, 2002). Again, training produced a bias toward viewing others as deceptive.

Presumption of Guilt

What effects does this presumption of guilt have on the interrogation process and outcome? Social psychologists have been studying the effects of interpersonal expectancies on social interactions (e.g., Rosenthal, 2002; Snyder & Swann, 1978; Snyder, Tanke, & Berscheid, 1977). Specifically, behavioral confirmation processes are thought to have three stages in which 1) perceivers develop an expectation about a target, 2) the perceivers' expectancies alter their behavior toward the target in expectancy-congruent ways, and 3) the perceivers' behavioral changes produce target responses

that confirm the perceivers' expectancies (Klein & Snyder, 2003; Snyder, 1992; Snyder & Klein, 2005; Snyder & Stukas, 1999).

Does the investigator's presumption of guilt initiate behavioral confirmation processes that cause suspects to behave in a manner that makes them look guilty? In another mock crime simulation, some participants stole \$100 from a specified location, whereas others did not (Kassin, Goldstein, & Savitsky, 2003). Student investigators interviewed all participants—innocent and guilty—via headphones from a separate location. The researchers instructed half of the investigators that 80% of the suspects in the study were guilty and the other half of the instructors that 20% of the suspects were innocent. Interrogators who were led to expect a guilty suspect asked more questions that presumed the guilt of the suspect than did those who were led to expect an innocent suspect. Innocence also worked against suspects in this experiment in that interrogators reported trying harder to get a confession from and exerting more pressure on suspects who were innocent, even though the interrogators did not know the guilt status of the suspects. The suspects' ratings of how hard the interrogator worked to get a confession and how much pressure the interrogator applied during the interview mirrored those from the interrogators, with the innocent suspects reporting that they were subjected to more high-pressured interviews. The interrogators' expectation about the suspect's guilt predicted their ultimate judgments about whether the suspect was guilty, with guilt presumptive interrogators judging more suspects guilty, but the actual guilt of the suspect was unrelated to interrogators' judgments of suspect guilt. Observers, blind to the experimental condition, also rated suspects interviewed by guilt-expecting interrogators to be more anxious and defensive than those interviewed by interrogators led to expect innocence and were more likely to judge suspects interviewed by guilt presumptive interrogators to be guilty (Kassin et al., 2003). These results have been replicated in a study conducted in the United Kingdom using a similar paradigm (Hill, Memon, & McGeorge, 2008). Thus, it appears as if the presumption of guilt prevalent among interrogators has the potential to put innocent suspects at risk because it results in the use of more pressure and guilt-presumptive questions during interrogations, which in turn causes suspects to confirm the guilt presumption by behaving anxiously and defensively.

Police Interrogation Tactics

What types of interrogation tactics do the police use, and which of those tactics, if any, are related to confessions and—more important—to false confessions? The Reid technique of interrogations contains nine steps that investigators

can take when questioning a suspect to a crime (Inbau et al., 2001), which others have reduced to three primary processes (Kassin & Gudjonsson, 2004; see also Hogg, this volume, on social influence tactics). The first process is custody and isolation, which involves removing suspects from their typical surroundings, holding them at the police station, and preventing contact from familiar others who may provide comfort. The second process involves confrontation, including accusations of guilt, expressed certainty of the suspect's guilt, the presentation of fabricated evidence to support the suspect's guilt, and the prohibition of denials of guilt from the suspect. The third process involves minimization, in which the interrogator provides suspects with justifications for why they may have committed the crime, implies that suspects will be treated more leniently if they confess, and that confession is the only behavior that will result in the suspect being released from custody.

How often do interrogators use these high-pressure techniques? A survey of police investigators about their interrogation practices suggested that the use of high-pressure techniques may be relatively uncommon (Kassin et al., 2007). Given the problematic nature of some of these techniques, investigators may have been motivated to underreport their use of these tactics. Findings from an observational study of close to 200 live and videotaped confessions demonstrating that the use of Reid techniques was relatively frequent (Leo, 1996b) lend support to this self-presentational explanation for the low frequency of reported high-pressure tactic use in the Kassin survey. Even if high-pressure techniques are relatively infrequent, at least some police officers use two of these tactics—fabricating independent evidence of the suspect's guilt and minimization strategies—in some interrogations. Laboratory studies suggest that both of these techniques increase the rate of false confessions (Horselenberg, Merckelbach, & Josephs, 2003; Kassin & Kiechel, 1996; Redlich & Goodman, 2003; Russano, Meissner, Narchet, & Kassin, 2005), although admittedly the confessions are not to crimes but to other types of transgressions.

In the first experimental study to examine the effects of presenting false evidence on rates of false confessions, researchers instructed participants to type letters into a computer while avoiding a particular key that if struck purportedly would cause the computer to crash (Kassin & Kiechel, 1996). To manipulate whether participants would be vulnerable to the influence of false evidence, participants were instructed to strike the keys quickly or slowly. While the participants were entering their keystrokes, the computer did crash even though they did not strike the key they were to avoid. The researcher then accused the participants of causing the computer crash and causing all data to be lost by pressing the prohibited key. For half of the

participants, a confederate presented false evidence, claiming that she had witnessed the participant striking the key.

Three forms of influence have been measured: compliance, internalization, and confabulation (Kassin & Kiechel, 1996). If participants signed a confession written by the experimenter, they were deemed compliant. Participants were judged to have internalized the confession if, when describing their experience to a confederate whom they encountered after the experiment, they accepted responsibility for crashing the computer (e.g., they admitted that they hit the prohibited key, causing the computer to crash). To measure confabulation, the experimenter questioned the participants about how the computer crash could have happened; participants confabulated if they provided details describing how they hit the offending key. Across all conditions, participants were more likely to sign the confession (e.g., comply; 69%) than they were to internalize the confession (29%) or confabulate (9%), but vulnerability and false evidence moderated these effects. When participant vulnerability was low (e.g., the typing pace was slow) and there was no false evidence, not a single participant internalized a confession or confabulated details—although a third of the participants did sign the confession written by the experimenter. When vulnerability was high (e.g., the typing pace was fast) and there was false evidence presented of the participant's guilt, all of the participants signed the confession, two-thirds internalized the confession, and one-third confabulated details. Thus, susceptibility to making a false confession increased with the presentation of false evidence. Using the same paradigm, these effects have been replicated in other labs (Forrest, Wadkins, & Larson, 2006), other countries (e.g., the Netherlands; Horselenberg et al., 2003, 2006), with children (Candel, Merckelbach, Loyen, & Reyskens, 2005; Redlich & Goodman, 2003), and when a cost is associated with confessing (Redlich & Goodman, 2003). Participants are less likely to confess falsely using this paradigm when the prohibited key would be harder to hit accidentally (e.g., the "esc" key as opposed to the "alt" key; Klaver, Lee, & Rose, 2008).

Experimental simulations also suggest that minimization tactics increase the likelihood that an innocent suspect will falsely confess. With minimization tactics, investigators provide suspects with a variety of justifications or excuses for their involvement in the criminal act of which they are accused (Kassin & Gudjonsson, 2004). Even without explicit promises of leniency, the use of minimization tactics in interrogations leads people to believe that leniency in sentencing is forthcoming if a confession is proffered. Participants who read a transcript of an interrogation containing minimization tactics, explicit promises of leniency, or neither tactic provided an estimate of the

expected sentence that the suspect would receive (Kassin & McNall, 1991). Participants who read an interrogation containing minimization or promises of leniency predicted that the suspect would receive a shorter sentence than participants who read the interrogation using neither tactics; the estimates of those reading interrogations using minimization tactics and promises of leniency did not differ.

Do these expectations of leniency translate into an increased likelihood of false confessions when interrogators use minimization tactics? Using a new problem-solving paradigm for producing confessions, researchers tested whether minimization tactics affected the diagnosticity of confessions obtained (e.g., the ratio of true to false confessions; Russano et al., 2005). In the first phase of the study, participants worked on solving a set of problems with a confederate who induces the participants to cheat or not, depending on the condition. At the conclusion of the first phase, the experimenter accused all participants of cheating. During the interrogation of the participants, the experimenter orthogonally varied whether the interrogation contained promises of leniency and minimization of the offense.

Although participants were more likely to confess to cheating when they were guilty than when they were innocent, they were also more likely to confess when the interrogator promised leniency or used minimization. The diagnosticity of a confession—which is calculated by dividing the rate of true confessions by the rate of false confessions—was greatest when neither tactic was used (Russano et al., 2005). Thus, it appears that the police can skirt the law prohibiting them from directly promising leniency by using minimization techniques, which are legal, cause suspects to infer promises of leniency, and exert similar pressures to confess as the illegal promises (Kassin & Gudjonsson, 2004).

Confessions and the Jury

False confessions are most problematic if they lead to the wrongful conviction of innocent people. Like other forms of evidence that may be unreliable—such as eyewitness evidence—jurors have the role of evaluating the evidence and determining what weight they should give it when weighing which verdict is appropriate. When confession evidence has been presented at trial, jurors are to evaluate the circumstances under which a confession was obtained and use that information to determine whether the confession was voluntary or whether the suspect was coerced into providing it. Thus, even if police practices produce some false confessions, injustice may be averted if jurors can appropriately judge the coerciveness of an interrogation

and make accurate predictions about whether a confession is true or false.

For justice to be served, jurors viewing confessions obtained under coercive circumstances must infer that the suspects' confessions were the product of coercive interrogations, rather than the product of something internal to the suspects (e.g., their guilt). Research on correspondent inference (also known as the fundamental attribution error) suggests that people may find it quite difficult to estimate the extent to which situational constraints might overcome dispositional tendencies to influence behavior (E. E. Jones, 1990; L. Ross, 1977). Even if people do assign some weight to the situational influences in producing a behavior, they may insufficiently adjust their initial dispositional inferences to account for the situation (Gilbert & Malone, 1995).

Research examining the effects of confession evidence on jurors' decisions is consistent with the research on correspondence bias: jurors underestimate the influence of some types of situational pressures on suspects to confess (Kassin & Sukel, 1997; Kassin & Wrightsman, 1980). In general, confession evidence is persuasive, even more persuasive than eyewitness or character testimony (Kassin & Neumann, 1997). Mock jurors do discount confession obtained through threats of physical harm (Kassin & Wrightsman, 1980). In contrast, jurors are just as likely to vote guilty when a confession was elicited through promises of leniency than when no promises were made, even though they do acknowledge that the voluntariness of the confession is questionable (Kassin & Wrightsman, 1980) and this effect persists in the face of jury deliberation (Kassin & Wrightsman, 1985) and judicial instructions to disregard involuntary confessions (Kassin & Wrightsman, 1981). Thus, even when jurors acknowledge that a confession is involuntary (e.g., a police officer waved a gun in a threatening way during an aggressive interrogation) and claim that the confession did not influence their decisions, they are more likely to render guilty verdicts than are jurors who are not exposed to the involuntary confession (Kassin & Sukel, 1997).

Jurors' failure to consider the role of the interrogator in eliciting a confession is exacerbated if they view a videotape of the confession that focuses solely on the suspect (Lassiter & Irvine, 1986; Lassiter, Slaw, Briggs, & Scanlan, 1992). Jurors are more likely to consider situational factors in their inferences of voluntariness and their verdicts when they watch a videotaped confession that contains both the interrogator and the suspect in the scene (Lassiter & Geers, 2004; Lassiter, Geers, Handley, Weiland, & Munhall, 2002; Lassiter, Geers, Munhall, Handley, & Beers, 2001). Even experienced legal professionals and law enforcement officers fall prey to the camera-perspective bias (Lassiter,

Diamond, Schmidt, & Elek, 2007). The bias appears to be perceptually based (Ratcliff, Lassiter, Schmidt, & Snyder, 2006), with new eye-tracking data confirming that this effect of camera view is in part due to increased visual attention to the interrogator (Ware, Lassiter, Patterson, & Ransom, 2008).

These studies suggest that jurors do not appropriately adjust the weight they give to a confession based on the conditions under which it was obtained but none of these studies allowed jurors to view an actual confession to determine whether it was true or false. People viewing confessions elicited from innocent and guilty mock suspects who had participated in a replication of the alt-key computer-crash paradigm (Kassin & Kiechel, 1996) were unable to detect which suspects were guilty and which were innocent at better than chance rates (Lassiter, Clark, Daniels, & Soinski, 2004). Neither college students nor police officers were able to predict accurately whether confessions offered by prison inmates—who confessed to the crime of which they had been convicted or to another inmates' crime—were true or false, although students were less likely to judge false confessions to be true than were police officers who were biased toward inferring guilt (Kassin, Meissner, & Norwick, 2005). In archival studies of proven false confessions cases in which defendants proceeded to trial, 73% (Leo & Ofshe, 1998) to 81% (Drizin & Leo, 2004) of the juries returned guilty verdicts.

JURY SELECTION

Once sufficient evidence exists against a suspect, either from a positive identification by an eyewitness, a confession from the suspect, or some other form of evidence, the suspect will be charged with the crime, brought before a grand jury, and potentially indicted and brought to trial. When suspects become defendants and are tried for the crimes with which they have been charged, they have constitutional rights to be tried by an impartial jury of their peers. Similarly, in civil cases in which jurors must make decisions about whether defendants engaged in behaviors that violated their duties to prevent harm of others, juries are assembled to determine defendant liability for a plaintiff's harm and, if so, what compensation that plaintiff should receive for the harm suffered. To assemble that jury, members of the community are assembled at the courthouse to form a pool from which jurors will be drawn. This pool of community members is called a *venire*. After the venire is assembled, a judge and attorneys representing the two sides question the venirepersons to determine whether they hold any biases that would prevent them from hearing the evidence fairly or following the relevant

laws when making their decisions. This pretrial proceeding in which jurors are questioned to uncover bias and the attorneys challenge jurors whom they perceive to be biased is known as *voir dire*.

During *voir dire*, attorneys attempt to identify jurors who will be biased against their case so that they might be removed from service. So in reality, juries are not selected; rather, jurors are chosen for exclusion from the jury in one of two ways. With a challenge for cause, if an attorney can demonstrate to a judge that a venireperson is unfit for jury service, perhaps because of relationships with the parties in the case, preconceptions about the defendant's guilt, or attitudinal bias that would prevent them from following the law, then the judge will excuse the venireperson from serving on the jury. A judge may grant an unlimited number of challenges for cause; all that is required is for the attorney to convince the judge that the venireperson is unfit for service. Sometimes rather than granting the challenge for cause, judges may attempt to rehabilitate venirepersons, extracting promises from them that they will put aside any biases they possess and will follow the law. With these promises, venirepersons are deemed unbiased and fit for service. Research on the effectiveness of this process is in nascent stages, but early findings suggest that this rehabilitation process may induce biased jurors to adopt less biased attitudes (Crocker & Kovera, 2009). The second method of removing a venireperson is a peremptory challenge; attorneys are given a limited number of these challenges, which they may use to eliminate a potential juror without stating their reasons for doing so unless the opposing side accuses them of removing jurors because of their race (*Batson v. Kentucky*, 1986) or gender (*J. E. B. v. Alabama*, 1994), which is impermissible.

Jury selection, as attorneys traditionally practice it, generally involves attorneys relying on stereotypes, implicit theories of attitudes and personality, and folklore based on other attorneys' trial experience (Fulero & Penrod, 1990a, 1990b). Some of the different beliefs about defendant wealth include the following: wealthy jurors are bad defense jurors unless they are trying a white-collar criminal; poor jurors are good defense jurors in civil cases because they are uncomfortable with large sums of money; and poor jurors are bad jurors for civil defendants because their dissatisfaction with their own financial status would lead them to play "Robin Hood" by delivering large awards to plaintiffs who are harmed by wealthy corporations (Fulero & Penrod, 1990b; Page, 2005). Some attorneys hypothesize that jurors who are similar to their client will have more empathy for them and will therefore be desirable (Blue, 1991; Kerr, Hymes, Anderson, & Weathers, 1995). In contrast to this defendant-similarity hypothesis, some attorneys fear that similar jurors may

want to distance themselves from in-group members who have committed very bad acts, viewing them as black sheep and not worthy of their support (Marques, Abrams, Paz, & Martinez-Taboada, 1998). These examples clearly illustrate that attorneys' commonsense notions of what makes a desirable juror are often contradictory and raise questions about whether traditional jury selection is likely to be effective.

Effectiveness of Traditional Jury Selection

There have been a few attempts to empirically evaluate the efficacy of attorneys' jury selection efforts. In one of the earliest of these evaluations (Zeisel & Diamond, 1978), venirepersons who were excluded from serving on 12 federal juries through peremptory challenges observed the cases from which they were excluded and then rendered a verdict at the conclusion of the trial. Researchers compared the verdicts obtained by the seated juries with the verdicts that would have been rendered by the juries that would have been seated if no peremptory challenges were allowed. Although peremptory challenges influenced the outcomes in a small number of cases, overall there was no influence on verdicts. In a similar study, researchers compared the verdicts decided by 10 actual juries, 10 juries composed of randomly selected venirepersons, and 10 juries consisting of challenged venirepersons (Diamond & Zeisel, 1974). Actual juries were less likely to convict than the two constructed juries. Although there are obvious limitations to these studies that make strong causal inferences difficult (e.g., only the actual juries deliberated or made decisions with real consequences), they remain classic investigations into the effectiveness of traditional jury selection.

Using a different methodology, researchers evaluated attorneys' jury selection performance in a series of studies assessing their strategies for judging juries (Olczak, Kaplan, & Penrod, 1991). Attorneys read a series of venireperson profiles and indicated what types of information they would seek from the prospective jurors during *voir dire*. The participants then read one of two felony trial transcripts and rated the venirepersons on their bias toward the defendant and a variety of personality traits, including leniency, intelligence, and attractiveness. Attorneys relied on a very small number of characteristics when making inferences about prospective jurors, and their strategies for selecting jurors did not differ from those used by college students. In another study, law students and attorneys read a summary of a manslaughter case, reviewed characteristics of a series of prospective jurors, and then rated their desirability as jurors. The prospective jurors had actually served as mock jurors in a different study and had rendered verdicts in the manslaughter case in question. Both the law

students and the attorneys accepted more jurors who had previously voted to convict the defendant as opposed to acquit, despite being tasked with finding desirable defense jurors.

A study of attorney-conducted *voir dire* in four felony trials also demonstrates its ineffectiveness (Johnson & Haney, 1994). Both prosecutors and defense attorneys effectively used their peremptory challenges to eliminate the most extremely biased jurors at both ends of the prosecution–defense continuum. However, the attitudes of the seated jurors were no different from the attitudes of the juries composed of the first 12 venirepersons or of 12 randomly chosen venirepersons. Thus, it appears that traditional attorney-conducted *voir dire* may identify extremely biased jurors but that more subtle biases are unlikely to be detected.

Scientific Jury Selection

In the early 1970s, a team of social scientists (Schulman, Shaver, Coltman, Emrich, & Christie, 1973) first attempted what is now known as scientific jury selection when they used empirical methods to assist a team of attorneys defend the “Harrisburg Seven,” a group of antiwar activists charged with conspiracy to kidnap then Secretary of State Henry Kissinger. Through community surveys, they identified demographic and attitudinal characteristics that correlated with potential jurors’ biases for and against the defendants. The scientists used the results of these surveys to develop profiles of favorable jurors, and the defense team relied on these profiles to guide their use of peremptory challenges. The use of scientists to assist in jury selection in this case represents the birth of what is now a multimillion dollar industry in litigation consulting (Seltzer, 2006). In addition to conducting case-specific surveys to determine correlates of verdicts in a specific case, litigation consultants may rely on the research that has identified demographic, personality, and attitudinal correlates of verdicts when developing juror profiles.

Demographic Predictors of Verdict

The identification of demographic predictors of verdict is desirable given that some states (e.g., California) and jurisdictions (e.g., federal court) severely limit the time and scope of *voir dire*. Under these circumstances, attorneys are often left with little information other than demographic characteristics on which to base their decisions about which jurors to challenge. Unfortunately for attorneys, demographic variables are only weakly and inconsistently related to verdict. In one study, juror age, gender, marital status, and occupation did not predict damage awards in a civil case (Goodman, Loftus, &

Greene, 1990). Other studies found relationships among income, occupation, education, and verdicts, with jurors having higher incomes, prestigious occupations, and more advanced education being more likely to convict than jurors with lower incomes, menial occupations, and less education (Adler, 1973; Simon, 1967).

Juror race does not reliably predict verdict either. Some early research suggested that Black mock jurors were more likely to find a defendant not guilty by reason of insanity than were White mock jurors (Simon, 1967). More recently, race was related to community members’ perceptions of O. J. Simpson’s culpability in his ex-wife’s death (Brigham & Wasserman, 1999). At every stage of the trial—before it began, after the evidence was presented, and after the verdict was rendered—Blacks were less likely to believe that Simpson murdered his ex-wife than were Whites. However, other evidence suggests that upper-middle-class Black jurors may be more punitive toward Black defendants, especially those who are charged with committing violent crimes (Nietzel & Dillehay, 1986), providing some evidence for the black sheep effect discussed earlier (Marques et al., 1998).

In contrast to race, gender appears to be a reliable predictor of verdict, at least in certain types of trials. Women are more punitive toward child sexual abuse defendants than are men (Bottoms, Davis, & Epstein, 2004; Bottoms & Goodman, 1994; Kovera, Gresham, Borgida, Gray, & Regan, 1997; Kovera, Levy, Borgida, & Penrod, 1994; McCoy & Gray, 2007; Quas, Bottoms, Haegerich, & Nysse-Carris, 2002). This gender bias generalizes to other types of cases in which women are more likely to be the complainants than are men, such as rape (Brekke & Borgida, 1988; Wenger & Bornstein, 2006), intimate partner violence (Feather, 1996; Kern, Libkuman, & Temple, 2007), and sexual harassment (Blumenthal, 1998; Gutek et al., 1999; Huntley & Costanzo, 2003; Kovera, McAuliff, & Hebert, 1999; O’Connor, Gutek, Stodale, Geer, & Melançon, 2004; Wayne, Riordan, & Thomas, 2001). The effect is not limited to cases in which women are disproportionately likely to be the victims; women are also more likely to acquit women with a history of domestic violence victimization who are charged with murdering their allegedly abusive partners (Schuller, 1992; Schuller & Hastings, 1996). Overall, these findings suggest that gender differences arise when one gender may be better able to take the perspective of the complainant or the defendant (O’Connor et al., 2004; Wiener, Watts, Goldkamp, & Gasper, 1995). Little evidence exists to suggest that gender reliably predicts verdicts in cases in which gender differences in perspective taking are irrelevant.

Although gender does not appear to predict verdicts reliably across different types of cases, it may predict whether a particular juror will be influential during jury deliberations.

Studies have found that men may exercise greater influence in deliberations than women; men spoke more frequently (James, 1959), were more likely to be selected to be the jury foreperson (Dillehay & Nietzel, 1985; Strodbeck, James, & Hawkins, 1957), and changed their votes less frequently (Golding, Bradshaw, Dunlap, & Hodell, 2007) than women. Thus, it may not be possible to predict verdict with gender, but it may be possible to ensure that a particular viewpoint is expressed during jury deliberations if a significant proportion of the men seated hold that view.

Personality Predictors of Verdict

Although demographic information is the most easily collected information about venirepersons, if given the opportunity to question jurors, it may be possible for attorneys to collect some information about venirepersons' personality traits during *voir dire*. Like demographic predictors, the relationship between personality traits and verdict is weak and inconsistent, which may account for why attorneys fail to pick jurors based on their Big Five personality traits (J. Clark, Boccaccini, Caillouet, & Chaplin, 2007). For example, a belief in a just world—the belief that bad outcomes happen to bad people—sometimes is associated with holding victims responsible for what happened to them and sometimes is associated with punitiveness toward defendants (Gerbasi, Zuckerman, & Reis, 1977; Moran & Comfort, 1982).

Individual differences in jurors' beliefs about personal responsibility predict verdicts at least some of the time. Some research shows that jurors who have an internal locus of control are more likely to convict a defendant than those with an external locus of control, especially when the evidence against the defendant is weak (Phares & Wilson, 1972). Similarly, jurors who hold strong beliefs in personal responsibility are more likely to hold plaintiffs responsible for experienced harm if they contributed even partially to that harm (Hans, 1992). Extraversion also appears to influence juror verdicts, with extraverts being more likely than introverts to acquit criminal defendants (J. Clark et al., 2007).

The personality trait that best predicts verdict across a variety of cases is authoritarianism. People with an authoritarian personality are more likely to respect authority, adhere to conventional views, and to punish those who fail to conform to authority or convention (Adorno, Frenkel-Brunswik, Levinson, & Sanford, 1950). An early measure of authoritarianism was developed in the context of research on prejudice (Adorno et al., 1950), but others have constructed measures of authoritarianism that are specific to the legal system, including the Legal Attitudes Questionnaire (LAQ; Boehm, 1968) and the

revised LAQ (Kravitz, Cutler, & Brock, 1993). A meta-analysis of the studies that tested the relationship between authoritarianism and verdict confirmed that jurors who are high in authoritarianism are more likely to convict defendants than jurors who are low in authoritarianism (Narby, Cutler, & Moran, 1993). This relationship is even stronger when authoritarianism was measured using a scale specifically created to measure legal authoritarianism (Narby et al., 1993). Authoritarianism also predicts sentencing, with authoritarian jurors recommending longer sentences (Bray & Noble, 1978) and recommending death sentences at higher rates (Butler & Moran, 2007) than nonauthoritarian jurors. If an authority figure (e.g., a police officer) is the person accused of wrongdoing, then authoritarians may be less punitive than nonauthoritarians (Nietzel & Dillehay, 1986). Thus, an authoritarian personality is related to verdicts that are consistent with upholding conventional norms and the legitimacy of authority figures.

Attitudinal Predictors of Verdict

Several attempts have been made to develop attitudinal measures of general juror bias for or against the prosecution. The Juror Bias Scale (JBS), which consists of two subscales, one measuring respondents' beliefs that people charged with crimes probably committed those crimes (probability of commission) and the other measuring respondents' beliefs about reasonable doubt, represents one of these attempts (Kassin & Wrightsman, 1983). More recent investigations have used confirmatory factor analysis to refine the scale and improve its predictive validity (Myers & Lecci, 1998). The scale has proved useful in predicting verdicts in studies conducted in other countries, such as Spain (De La Fuente, De La Fuente, & Garcia, 2003) and in reactions to real trials such as the O. J. Simpson case (Chapdelaine & Griffin, 1997). The items tapping beliefs about reasonable doubt are better predictors of verdict than are the items tapping beliefs about the probability of commission (Lecci & Myers, 2002).

Because the original item pool for the JBS is limited to items tapping beliefs about probability of commission and reasonable doubt, the items may underrepresent the construct of juror bias, leading to poor prediction of juror verdicts (Lecci & Myers, 2008). To correct this perceived problem, Lecci and Myers generated additional items designed to tap beliefs about conviction proneness, system confidence, cynicism toward the defense, racial bias, social justice, and innate criminality. Not only did this new scale, which they named the Pretrial Juror Attitude Questionnaire (PJAQ), significantly predict verdicts in five of the six trial summaries that mock jurors read, it also

showed incremental validity in that it predicted verdicts even after controlling for mock jurors' JBS and RLAQ scores.

Demographic characteristics and personality traits serve as proxies for jurors' beliefs and attitudes; at best, they can suggest a general tendency for a juror to evaluate evidence in a particular way. Similarly, most measures of juror bias assess general tendencies toward supporting crime control versus due process issues. Perhaps one of the reasons that there are few identified demographic variables, personality traits, or general attitudinal measures that predict verdict is that they provide insight into general evaluative tendencies rather than more case-specific attitudes or beliefs. Social-psychological research on the attitude-behavior relationship suggests that specific attitudes are stronger predictors of behavior than are more general attitudes (Kraus, 1995). Perhaps more specific case-relevant attitudes would serve as better predictors of verdicts.

Evidence that case-specific attitudes are good predictors of juror verdict inclinations before trial has grown over the years. Attitudes toward tort reform predict verdict inclinations, with jurors favoring reform more likely to favor the defense in civil trials (Moran, Cutler, & DeLisa, 1994). Similarly, attitudes toward psychiatrists predict verdict inclinations in insanity cases, with favorable attitudes related to a greater likelihood of voting not guilty by reason of insanity (Cutler, Moran, & Narby, 1992), and attitudes toward drugs predict ratings of defendant guilt in drug cases (Moran, Cutler, & Loftus, 1990). However, all of these studies used a survey methodology, and the researchers assessed the predictive validity of the attitudinal measures without the presentation of any evidence.

For those studies that have tested the predictive validity of specific attitudes after the presentation of evidence, results are more mixed. Despite a psychometrically strong measure of jurors' attitudes toward eyewitnesses, these attitudes were unrelated to mock juror verdicts in a robbery case containing eyewitness evidence (Narby & Cutler, 1994). Belief in a litigation crisis significantly predicted verdict in a tobacco and a pharmaceutical case, but not in an insurance case (Vinson, Costanzo, & Berger, 2008). Other researchers, however, have found attitudes that predict verdicts. Attitudes toward the insanity defense predict mock juror verdicts in insanity cases (Skeem, Louden, & Evans, 2004), and attitudes toward the death penalty predict verdicts in capital cases (O'Neil, Patry, & Penrod, 2004). Similarly, the attitudes of formerly impaneled jurors toward the death penalty predicted whether they had voted guilty in the trial in which they had served, irrespective of whether it had been a capital trial in which the death penalty was an option (Moran & Comfort, 1986).

Effectiveness of Scientific Jury Selection

How effective is scientific jury selection? This question is best answered in relation to the effectiveness of traditional attorney-conducted jury selection because it is the default method of jury selection. Even concluding that there are juror characteristics that allow the prediction of jurors' verdict choices, attorneys may be just as good at predicting jurors' verdict inclinations without the additional expense of scientific methods. Only one published study has compared the efficacy of traditional and scientific jury selections (Horowitz, 1980). In this study, researchers trained law students to use either traditional or scientific selection methods and collected their predictions of mock jurors' verdicts in four simulated trials. The results from this study were mixed, with two cases showing a superiority of scientific jury selection, one case showing the superiority of traditional jury selection, and one case showing no preference for jury selection type. Scientific jury selection performed better only when there was a strong relationship between the predictors—personality, demographic, and attitudinal characteristics—and verdict.

Bias in and From Jury Selection

Jury selection has several purposes. It provides judges and attorneys an opportunity to introduce jurors to the general issues that they expect will arise in the case and to educate jurors on the relevant law. When judges provide attorneys with latitude in *voir dire*, attorneys may use the opportunity to ingratiate themselves with jurors. The most important purpose of *voir dire*, however, is to identify jurors who are unfit for jury service because of bias and to eliminate them from the jury pool. The research reviewed so far suggests that traditional attorney-conducted *voir dire* allows attorneys to perform at about chance levels when identifying biased jurors (Olczak et al., 1991), that traditional jury selection produces juries that have the same attitudinal composition as juries produced through random selection from the venire (Johnson & Haney, 1994), and that scientific jury selection only sometimes produces better attorney decisions than does traditional jury selection (Horowitz, 1980). Given the spotty record of jury selection for the identification of juror bias, it is troubling that racial bias is evident in jury selection decisions and that *voir dire* may actually produce bias in jurors.

Race and Peremptory Challenges

Attorneys are prohibited from using peremptory challenges to eliminate jurors because of their race (*Batson v. Kentucky*, 1986) or gender (*J. E. B. v. Alabama*, 1994). Yet each year, a number of appellants question verdicts because they allege that race played a role in attorneys' decisions

to eliminate jurors from a venire. Several studies of jury selection in actual cases have found that Black venirepersons are more likely to be excused by the prosecution than by the defense (J. Clark et al., 2007; Rose, 1999). The Supreme Court recently ruled that racial bias in the use of peremptory challenges could be established by demonstrating either (a) that attorneys asked different *voir dire* questions of jurors belonging to different racial groups or (b) that the justifications used to provide a non-race-based explanation for peremptory challenges used to dismiss member of one race were equally applicable to members of a different racial group who were not dismissed (*Miller-El v. Dretke*, 2005; *Snyder v. Louisiana*, 2008). Despite these rulings, appeals of verdicts based on the improper exclusion of jurors usually fail (Gabbidon, Kowal, Jordan, Roberts, & Vincenzi, 2008), in part because attorneys are skilled at providing reasons other than race for excluding jurors, even when race has likely played a role in their decision making (Sommers & Norton, 2008).

To test whether venireperson race influences attorneys' use of peremptory challenges, prosecuting attorneys read profiles of two venirepersons for a case in which a Black defendant was charged with robbery and aggravated assault (Sommers & Norton, 2007). Irrespective of the other characteristics in the profile, attorneys were more likely to exclude a Black venireperson than a White venireperson. When asked why they chose to exclude the juror, the attorneys provided race-neutral explanations for their decision. Thus, racial bias in jury selection may go undetected because attorneys provide compelling race-neutral justifications for their racially biased decisions (Page, 2005). In fact, in his dissenting opinion in *State v. Snyder* (2006), Justice Johnson observed that "Verbalized facially neutral reasons can be a pretext for conscious or unconscious racism" (p. 506).

Conviction-Proneness and Voir Dire in Capital Cases

When prosecutors seek the death penalty for a defendant (i.e., in capital cases), special jury selection procedures are required to determine whether jurors' attitudes toward the death penalty would interfere with their ability to consider the evidence impartially and to follow the law (*Wainwright v. Witt*, 1985; *Witherspoon v. Illinois*, 1968). A capital case has two phases: a guilt phase in which evidence of the defendant's guilt—and sometimes innocence—is presented and the jury determines whether the defendant is guilty of the crimes as charged, and a penalty phase that occurs only if the defendant is convicted in the guilt phase. In this penalty phase, evidence is presented about aggravating and mitigating factors that would argue for or against sanctioning defendants by taking their lives. Jurors may

not serve in either phase of a capital trial if their attitudes toward the death penalty would render them incapable of following the law, either because they would be unwilling to impose the death penalty or because they could not fairly evaluate the evidence supporting the defendant's guilt knowing that a conviction could result in the death of the defendant. The special *voir dire* process by which jurors are evaluated for their fitness to serve in a capital case is called death qualification.

Death qualification produces a jury that has different demographic and attitudinal characteristics than a jury seated in noncapital cases (Fitzgerald & Ellsworth, 1984; Moran & Comfort, 1986). In a sample of impaneled felony jurors (Moran & Comfort, 1986) and two random samples of community members (Fitzgerald & Ellsworth, 1984; Haney, Hurtado, & Vega, 1994), African Americans, women, Democrats, and the poor were significantly more likely to oppose the death penalty than were European Americans, men, Republicans, and the wealthy. Meta-analysis confirms that women and minorities are more likely to hold attitudes that would systematically exclude them from capital juries (Filkins, Smith, & Tindale, 1998). Thus, the death qualification process reduces the likelihood that specific groups of people will serve on a jury. This reduction in the diversity of jurors minimizes the chances that alternative points of view will be expressed during deliberation (e.g., Sommers, 2006).

In addition to the possibility that jurors—both those from underrepresented groups and those of European descent—will be less likely to exchange pertinent information on the less diverse juries created through death qualification, the jurors who remain after death qualification are more likely to convict a defendant than are those who oppose the death penalty (e.g., Cowan, Thompson, & Ellsworth, 1984; Moran & Comfort, 1986; Thompson, Cowan, Ellsworth, & Harrington, 1984). Three meta-analyses of the studies testing the relationship between death penalty attitudes and verdicts confirm that those who favor the death penalty are more conviction prone (Allen, Mabry, & McKelton, 1998; Filkins et al., 1998; Nietzel, McCarthy, & Kern, 1999), with death-qualified jurors approximately 25% to 44% more likely to render a guilty verdict than are jurors who oppose the death penalty. Juries composed of death-qualified jurors are more critical of defense evidence and generally remember less evidence than do juries that contain jurors with a mix of death penalty attitudes (Cowan et al., 1984). Merely watching a death-qualifying *voir dire* makes jurors more conviction-prone; death-qualified mock jurors who watched a death-qualification *voir dire* provided pretrial ratings indicating that they thought it was more likely that the defendant was guilty than did mock jurors who watched a standard

voir dire (Haney, 1984). Thus, death-qualifying *voir dire* appears to bias jurors toward conviction.

Behavioral Confirmation and the Voir Dire Process

Researchers are just beginning to study the *voir dire* process and how it might lead to the creation of bias rather than its identification and elimination. There are several points during the *voir dire* process at which attorneys may be led astray when trying to predict the bias of individual venirepersons. In the information-seeking stage, attorneys' stereotypes or expectations about venirepersons may influence the types of questions that attorneys ask of jurors, including questions that are biased toward confirming the attorney's hypothesis or that lack diagnosticity. In the information-generation stage, attorneys' questions may influence the information gathered from venirepersons, especially if venirepersons are motivated to provide socially appropriate responses. In the inferential stage, the questions asked by attorneys, the hypotheses they hold, and the answers they receive from venirepersons may bias the conclusions that attorneys draw from venirepersons' responses to *voir dire* questions. Finally, the very act of endorsing a trial-relevant attitude, even an endorsement that is evoked through behavioral confirmation processes (Snyder & Klein, 2005), may increase the likelihood that jurors will vote to convict or acquit a defendant.

Early research on how attorneys gather information during *voir dire* and how venirepersons react to their questions suggests that *voir dire* may bias jurors because attorneys engage in biased hypothesis testing and their expectations influence jurors to change their expectation-relevant attitudes. In one study, attorneys were told to test a particular hypothesis about a venireperson. Specifically, attorneys generated two *voir dire* questions that they would use to test whether a juror held legal authoritarian attitudes or civil libertarian attitudes, or to determine which of these attitudes a venireperson held (Crocker, Kennard, Greathouse, & Kovera, 2009). Attorneys used a positive test strategy (e.g., asked hypothesis-confirming questions) in that they were most likely to ask a question that legal authoritarians would answer "yes" when they were testing the legal authoritarian hypothesis and least likely to ask this type of question when they were testing the civil libertarian hypothesis.

In another study, researchers manipulated the expectation that a mock attorney held about a venireperson by providing the attorney with data from a "juror questionnaire" (Greathouse, Crocker, Kennard, Austin, & Kovera, 2009). The data accurately reported demographic information, but researchers randomly manipulated whether attitudinal information portrayed the venireperson as pro-prosecution or pro-defense. After receiving this expectation, attorneys

conducted a capital *voir dire* with a community member, subsequently providing ratings of the mock venireperson's defense and prosecution bias. The mock attorneys' expectations about the mock venirepersons predicted their ratings of the venirepersons' bias even after controlling for the venirepersons' post-*voir dire* death penalty attitudes and blind coders' ratings of the pro-defense and pro-prosecution bias exhibited in the venirepersons' behavior. Moreover, venirepersons questioned by attorneys holding pro-prosecution expectancies held more positive attitudes toward the death penalty after *voir dire* than did venirepersons questioned by attorneys with pro-defense expectations, despite the attitudinal similarity of these groups prior to *voir dire*. These studies, taken together with the research on the ability of attorneys to identify favorable jurors, suggest that biased hypothesis testing and behavioral confirmation processes may be at work in *voir dire* and question whether the process serves the purpose for which it is intended: seating a fair and impartial jury.

PRETRIAL PUBLICITY

Juror bias can also be created through extralegal means. Certain types of information, if it is released before trial in the press, can prejudice the jury pool against a defendant, endangering the impartiality of the jury pool. Because of concerns about the prejudicial impact of this information on jury decisions, the American Bar Association (2000) recommended that attorneys avoid discussing or releasing entire categories of information before the start of a trial, including but not limited to (a) the defendant's prior criminal record; (b) information about the defendant's character or reputation; (c) any confession or admission against interest produced by the defendant (or the refusal to provide information); (d) whether the defendant has or has not submitted to any examination or test; and (e) any opinion about the defendant's guilt or about the sufficiency of the evidence against the defendant. Any information of this nature could lead a juror to infer that a defendant is guilty. This inference of guilt is problematic for two reasons: (a) this prejudicial information is often not admissible at trial because it tends to be unreliable and therefore should have no influence on jurors' decisions and (b) even if it is admissible, jurors are required to presume defendants' innocence unless the trial evidence proves their guilt.

Despite the prejudicial nature of some types of trial-relevant information, the First Amendment guarantees freedom of the press to publicize newsworthy information. But can pretrial exposure to these types of information about a defendant abrogate a defendant's Sixth Amendment right to a fair and impartial jury, negatively affecting the way

jurors evaluate defendants and the evidence against them? Pretrial publicity (PTP) exposure negatively affects not only pretrial judgments of defendant guilt but also post-trial judgments (Stuebaker & Penrod, 1997, 2005), meaning that the effect of PTP on jurors' decisions survives the presentation of trial evidence (Chrzanowski, 2005; Otto, Penrod, & Dexter, 1994; Ruva, McEvoy, & Bryant, 2007). Negative effects of media exposure are seen even when jurors are exposed to media that is not specific to the case they are trying. General PTP—media that is topically related to a case but does not include prejudicial information about the defendant in the specific case—can increase the likelihood that jurors will convict the defendant (Greene & Loftus, 1984; Greene & Wade, 1988; Imrich, Mullin, & Linz, 2005; Kovera, 2002).

The prejudicial effects of PTP on jurors' guilt judgments are generally robust. Although most of the studies of PTP have been conducted in the context of criminal cases, PTP also affects judgments in civil trials, with PTP exposure increasing the probability that jurors found the defendant liable in a personal injury case (Bornstein, Whisenhunt, & Nemeth, 2002). A meta-analysis of 23 studies and 44 tests of the effects of PTP exposure demonstrated that there was a small to moderate effect of PTP on jurors' judgments of defendant guilt across a variety of participants, settings, trial stimuli, PTP types, and research methods (Stebly, Besirevic, Fulero, & Jimenez-Lorente, 1999). As the number of types of prejudicial information to which jurors are exposed increases, so does the size of the PTP effect. When jurors read or hear about multiple categories of prejudicial information, they are more likely to find a defendant guilty than when they read or hear only one type of prejudicial information (Stebly et al., 1999).

Methods Used in Pretrial Publicity Studies

Researchers of PTP effects have generally used one of two types of methods (Stuebaker & Penrod, 2005). The more ecologically valid research method tests the effects of naturally occurring PTP exposure on community members' pretrial judgments about defendants in real cases. In these studies, researchers survey community members in the venue in which the real case is to be tried, asking the respondents questions designed to measure the extent of their exposure to PTP, the content of what they recall or recognize about the case, and their pretrial judgments about the defendant's guilt. Although it is possible to test the effects of PTP surveying only members of the potential venire by correlating self-reported extent of exposure with judgments of defendant guilt, researchers often survey a comparison group of community members from another venue that has not been saturated with publicity

about the case, allowing for the comparison of guilt judgments across the two venues that naturally differ in PTP exposure. The strength of the method lies in its use of real potential jurors and exposure to PTP in natural settings that allow people to pay as much or as little attention to the PTP as they would in real cases. However, this approach has several limitations, including an inability to draw strong causal conclusions about PTP effects because certain types of people may seek PTP exposure, which creates selection confounds. In addition, these studies assess PTP effects on juror judgments without allowing for the potential curative effects of trial evidence, judicial instruction to ignore the PTP, or deliberation.

The second method involves the experimental manipulation of PTP exposure followed by a trial simulation, allowing for random assignment of participants to level of PTP exposure, removing the potential for selection confounds present in the field studies of PTP. Within this general paradigm, researchers may also test the effects of other variables (e.g., type of prejudicial information, time between PTP exposure and trial, judicial instructions) by manipulating them. Although some criticize these methods because they usually lack the features of real trials (e.g., prolonged PTP exposure, sworn jurors, consequential decisions, deliberation), the trial simulation method has strong internal validity and allows for causal conclusions about the effects of PTP. Moreover, a comparison of the effects of naturally occurring and experimentally manipulated PTP found that exposure to prejudicial PTP led to increased perceptions of the defendant's guilt, irrespective of whether the exposure came from naturally occurring or experimental sources (Chrzanowski, 2005).

Although this study suggests that researchers should not be concerned about the external validity of findings obtained using experimentally manipulated PTP exposure, other study characteristics may moderate the effect of PTP on jurors' trial judgments. A meta-analysis of the PTP literature (Stebly et al., 1999) found that survey studies, which typically test the effects of PTP on pretrial judgments rather than on judgments made after considering trial evidence, produced larger PTP effects than did experimental studies, which almost always include the presentation of trial evidence (for an example of an experimental study that did not contain the presentation of trial evidence, see Ogloff & Vidmar, 1994). In the PTP literature, survey and experimental studies typically differ not only in terms of the research design but also in terms of the participant sample, with survey studies generally sampling from community members and experimental studies using a college student sample. However, exposure to prejudicial information about the defendant causes jurors to adopt a pro-prosecution bias when evaluating the trial evidence

that they subsequently hear (Hope, Memon, & McGeorge, 2004). This predecisional distortion serves to reinforce rather than mitigate the effects of PTP even in the face of trial evidence.

Remedies for Pretrial Publicity Effects

Certain procedural remedies are presumed to eliminate or at least mitigate the harms associated with prejudicial PTP, including *voir dire*—which is often extended in cases involving PTP to identify jurors who may have been prejudiced by exposure to PTP and to educate jurors on the need to ignore the PTP. Other remedies include judicial instructions to ignore PTP, delays of the trial (i.e., continuances) to allow the PTP to subside, jury sequestration, and changes of venue to a location where there was no PTP or at least where the nature and extent of the PTP was less prejudicial (American Bar Association, 2000). Holdings in several Supreme Court cases task judges with taking steps to ensure that extensive prejudicial pretrial media does not abrogate a defendant's Sixth Amendment rights to a fair trial (*Irvin v. Dowd*, 1961; *Rideau v. Louisiana*, 1963; *Sheppard v. Maxwell*, 1966). Although these cases primarily focused on changes of venue as a remedy for the prejudicial effects of PTP, the court did also suggest that other remedies for PTP could be effective, including extended *voir dire* and the rehabilitation of biased jurors, sequestration of juries, and judicial admonitions to disregard the PTP. Despite the court's faith in these remedies, the empirical evidence suggests that this faith may be misplaced.

Deliberation

Before considering these procedures specifically designed to combat PTP effects, it is important to consider whether deliberation—a process that occurs in every jury trial—provides an opportunity to correct the negative effects of PTP exposure. Some legal practitioners and scholars have criticized research showing PTP effects by arguing that all of the survey studies and most of the experimental studies fail to contain opportunities for jurors to deliberate, during which—they presume—fellow jurors will admonish other jurors who mention prejudicial information obtained from PTP, thereby minimizing any effects it could have. The few studies that have included deliberation report that jurors rarely referred to prejudicial news reports during deliberation (Kline & Jess, 1966; Kramer, Kerr, & Carroll, 1990). When jurors do mention PTP in deliberations, other jury members did remind the group that the pretrial information was prohibited from consideration (Kramer et al., 1990). Yet these admonitions to disregard PTP did not correct the PTP effects because juries exposed to prejudicial PTP were still more punitive than

were juries that were not exposed, even after participating in deliberation. Deliberations exacerbated the negative effects of emotional PTP. Deliberations did not eliminate PTP effects when the evidence against the defendant is strong; when the evidence against the defendant is more ambiguous, deliberation increases the effects of PTP (Kerr, Niedermeier, & Kaplan, 1999). Overall, this research suggests that the justice system cannot count on deliberation to correct for prejudicial effects of PTP exposure.

Voir Dire

Extended *voir dire*—in which attorneys receive increased latitude to question jurors—is the remedy on which judges rely most often in their attempts to correct the prejudicial effects of PTP. In cases in which extensive pretrial media attention has presented potentially prejudicial information, judges may use their discretion to allow attorneys to explore the extent to which the venire has been tainted by PTP exposure. In addition to allowing more extensive questioning, the judge may provide the attorneys with the opportunity to use *voir dire* as an educational tool, providing jurors with information about the dangers of relying on PTP when making decisions, encouraging potential jurors to disregard any information about the case that is not in evidence at trial, and seeking commitments from venirepersons that they will ignore information learned pretrial if they sit on the jury.

For *voir dire* to serve as an effective remedy for PTP, several conditions must be met. First, attorneys must be able to identify jurors who have been prejudiced by PTP and then have them removed from the jury pool either by challenging them for cause or by using a peremptory challenge if they fail to convince a judge that the venireperson has been tainted. There are no studies that directly test attorneys' abilities to identify tainted jurors, but the research reviewed in the earlier section on jury selection suggests that attorneys may have difficulty identifying biased jurors.

Second, the success of *voir dire* rests on the supposition that venirepersons' self-reports will accurately reflect the extent to which they have been biased by PTP exposure. Both social desirability concerns and lack of self-awareness can limit the validity of data obtained through self-report, but courts routinely rely on venirepersons' self-reports to determine bias (Posey & Dahl, 2002). Although a positive relationship between venirepersons' exposure to PTP and their perceptions of defendant guilt exists (Costantini & King, 1980/81; Moran & Cutler, 1991), jurors' self-reported ability to remain impartial is uncorrelated with their pretrial judgments of a defendant's culpability (Costantini & King, 1980/81; Kerr, 1989; Kerr, Kramer, Carroll, & Alfini, 1991; Moran & Cutler, 1991;

Simon & Eimermann, 1971; Sue, Smith, & Pedroza, 1975). For example, in one community survey, people residing in a venue with extensive media attention to a case were more likely to have prejudged the defendant to be guilty than those residing in the alternate venue where there was little media attention, but the members of each venue reported the same ability to remain impartial if they were to serve as a juror in the case (Moran & Cutler, 1991).

A lack of relationship between venirepersons' reports of impartiality and their judgments of defendant culpability should not be surprising given social psychological research suggesting that people lack the ability to access and report the cognitive processes driving their decisions (Nisbett & Wilson, 1977). Even if people were able to appreciate the causal factors in their decisions, they believe that they are able to disregard information accurately if they choose to do so, whereas others are less capable and more biased (Pronin, Gilovich, & Ross, 2004). Yet evidence suggests that in social and legal contexts, prohibited information still influences people's judgments even when they are instructed to disregard it (Stebly, Hosch, Culhane, & McWethy, 2006; Wyer & Budesheim, 1987; Wyer & Unverzagt, 1985). So any venirepersons who lack awareness that they have been biased by PTP are likely to be seated on a jury because they report being impartial and once seated are unlikely to follow instructions to disregard the PTP when forming their decisions of defendant guilt.

Social desirability concerns demonstrably influence jurors' responses during *voir dire*. Venirepersons may pick up on verbal or nonverbal cues from judges and attorneys that certain answers to questions about PTP are more satisfactory than others and could encourage socially desirable responding (LeVan, 1984). Ex-jurors have reported that when they feel anxious about being evaluated during *voir dire*, they were less likely to provide honest answers (Marshall & Smith, 1986). Prospective jurors are less likely to succumb to social desirability concerns when questioned by attorneys than by a judge (S. E. Jones, 1987). In a mock *voir dire*, community members completed a pretrial legal attitudes questionnaire and then participated in a *voir dire* conducted by a judge or attorneys. After *voir dire*, participants again completed the attitudinal questionnaire, with participants questioned by the judge changing their post-trial attitudes from their pretrial attitudes nearly twice as often as those questioned by attorneys.

One study of PTP effects on juror judgments specifically tested whether social desirability affected jurors' self-reports of bias by analyzing the responses of venirepersons to a questionnaire that they completed before *voir dire* (Chrzanowski, 2005). The questionnaire assessed their knowledge of the case and their self-reported ability to remain impartial and hear the case fairly. Chrzanowski

compared these data from the actual venirepersons with the responses to the same questions provided by community members residing in the trial venue during a telephone survey. Seventy percent of the telephone survey respondents reported an inability to hear the case fairly in contrast to 10% of the venirepersons in the case. These data suggest that the social pressures inherent in the courtroom may prevent venirepersons from accurately reporting their abilities to put aside information gleaned from PTP when deciding a case.

Even if the identification of jurors who are biased by PTP may prove difficult, perhaps *voir dire* serves as a mechanism to encourage jurors to set aside their biases. Only one study has provided a proper test of whether *voir dire* might correct juror bias from PTP exposure, and its findings are not particularly promising (Dexter, Cutler, & Moran, 1992). In this study, researchers manipulated participants' exposure to PTP and whether they participated in a standard or an extended *voir dire*. For *voir dire* to correct PTP bias, one must observe an interaction between *voir dire* type and PTP exposure, such that the extended *voir dire* eliminates the difference between the punitiveness of those who are exposed to PTP and those who are not. No such interaction was observed (Dexter et al., 1992). Despite this empirical evidence suggesting that the *voir dire* remedy is ineffective, it continues to be a popular remedy among judges, perhaps because it is relatively inexpensive to implement and judges believe that it works (Kovera & Greathouse, 2008).

Judicial Instruction

The courts have also suggested that if a judge instructs jurors to ignore information about a case that was obtained before trial, the effects of PTP will be eliminated. In these instructions, judges tell jurors of their duty to avoid the influence of PTP and to base their verdict only on the evidence that is presented at trial. It seems unlikely that judicial instruction to disregard PTP will be effective; a recent meta-analysis shows that jurors generally do not follow instructions to disregard inadmissible evidence that is mistakenly presented at trial (Stebly et al., 2006). However, the meta-analysis does suggest that people are better able to follow instructions when they understand the justification for disregarding the inadmissible evidence; so perhaps if judicial instructions help jurors understand why they must ignore PTP, the instructions will reduce or eliminate PTP effects.

Early research on PTP instructions suggested that judicial admonitions would reduce PTP effects (Kline & Jess, 1996; Simon, 1966); however, these early studies contained design features—missing control groups and demand characteristics—that make it difficult to draw strong inferences

from their findings. A less promising picture is painted by methodologically sound studies, which generally fail to find a curative effect of judicial instructions on PTP (Fein, McCloskey, & Tomlinson, 1977; Sue, Smith, & Gilbert, 1974). The failure of PTP instructions to remedy the negative effects of prejudicial PTP on jurors' judgments of defendant culpability applies to PTP that presents facts and PTP that arouses emotion (Kramer, Kerr, & Carroll, 1990) and to PTP effects in civil cases (Bornstein et al., 2002). Thus, one cannot rely on judicial instruction to eliminate the negative effects of PTP exposure.

Continuances

Another possible remedy for PTP is a continuance—a delay of the start of a trial. If pretrial media coverage of a case is extensive, the defense can move to delay the start of the trial with the hope that media coverage will dissipate during the delay. Although media coverage of a case may decrease if a trial date is moved to a future date, it will probably rebound as the new trial date approaches. However, if a continuance does decrease trial coverage immediately before the trial, what effects, if any, does PTP exposure have on juror judgments when there is a delay between exposure and judgment?

Few studies have specifically tested the effects of delaying a trial for a period of time after PTP exposure. In the best of these studies (Kramer et al., 1990), researchers manipulated exposure to factual PTP (e.g., publicity that presents inculpatory information suggesting the defendant's guilt) and emotional PTP (e.g., publicity that does not speak to the defendant's guilt but is designed to arouse emotions of those exposed). They also manipulated whether participants were exposed to the PTP several days or immediately before they began the experimental session in which they watched the videotaped trial simulation. Delay reduced the effects of factual PTP but did not reduce the effects of emotional PTP (Kramer et al., 1990).

The delay in the Kramer et al. study was relatively short—only several days. Continuances are often longer than this, so delays could counteract even emotional PTP if they were relatively longer. A meta-analysis examined whether the length of delay between PTP exposure and trial judgments moderated the strength of PTP effects (Stebly et al., 1999). The results suggest that PTP effects are greater when the delay between PTP exposure and trial judgments exceeds 7 days, although their results must be viewed with caution given that there were few tests that manipulated delay within a single study; therefore, other characteristics may systematically vary with delay to produce these results, although no such characteristics were easily identifiable. Although there are few studies that manipulate delay that allow us to confirm this meta-analytic

finding, delay might exacerbate PTP effects because it increases source-monitoring errors such that people find it harder to remember whether information that they know about case was learned pretrial or gleaned from trial evidence when the delay between PTP exposure and trial increases (Ruva & McEvoy, 2008). If one is going to ignore PTP, one needs to be able to discriminate remembered information that was obtained from PTP from information that was obtained during the trial. If continuances decrease people's ability to make this discrimination, then it is not surprising that delays might increase the influence of pretrial publicity.

Change of Venue

A final method to remedy a venire that is tainted by PTP exposure is to move the trial to a different community that has not been exposed to publicity surrounding the case or at least has been less biased by it. Assuming that the venire in the new venue has less exposure to prejudicial PTP than did the venire in the original venue, a change of venue should be a highly effective method to combat the negative effects of PTP. This condition should usually be met because research has repeatedly demonstrated that community members in counties where a highly publicized trial is to be tried are more likely to hold negative attitudes toward the defendant than are community members in other nearby counties (Costantini & King, 1980/81; Moran & Cutler, 1991; Nietzel & Dillehay, 1983; Simon & Eimmerman, 1971; Vidmar & Judson, 1981). Despite its efficacy, judges are often reluctant to grant changes of venue because it is costly to move a trial to a new location.

Judges may grant a change of venue motion if they are convinced that the community is so prejudiced against the defendant that he or she would not be able to have a fair trial anywhere within that jurisdiction. Results from a survey about the case from the current venue and at least one alternative venue often are submitted to support motions for changes of venue (Nietzel & Dillehay, 1983). Another method to establish whether venues have been saturated with prejudicial PTP is content analysis of local media coverage (Studebaker, Robbennolt, Pathak-Sharma, & Penrod, 2000). With content analysis, one can establish the amount of PTP in a venue, its prominence (e.g., lead stories, front page, above the fold of the newspaper) as well as the type (e.g., emotional vs. factual, pro-prosecution vs. pro-defense). If coverage is more extensive, prominent, and negative in the current venue than in an identified alternative venue, the content analysis could provide evidence of prejudice that would enable a judge to grant a change of venue motion. Even in extremely high-profile cases, such as Timothy McVeigh's prosecution in the Oklahoma City bombing case, it has been possible to demonstrate that

media coverage was more extensive and more prominent than it was in at least one alternate venue (Studebaker et al., 2000).

In sum, exposure to pretrial media coverage that casts doubt on the defendant's innocence or otherwise portrays the defendant in a negative light results in juries rendering guilty verdicts more often than they would without exposure to prejudicial PTP. Although the courts have developed safeguards intended to remedy the prejudicial effects of PTP, many of them (*voir dire*, judicial instruction, continuances, deliberation) do not fully protect defendants' rights to an impartial jury. Of the remedies available, a change of venue has the most empirical support.

EXPERT EVIDENCE

Although jurors' reactions to different types of evidence have been extensively studied over the past 50 years (Devine, Clayton, Dunford, Seying, & Pryce, 2001), one type of evidence that has received considerable research attention recently is scientific evidence presented by experts. In the later half of the 1990s, the focus of research on expert testimony shifted from whether expert testimony on particular topics influenced jurors to whether jurors—and other legal decision makers—had the ability to evaluate the quality of scientific and other expert evidence. The primary impetus for this shift was the Supreme Court ruling in *Daubert v. Merrell Dow Pharmaceuticals* (1993), which addressed the legal debate over the rules governing the admissibility of scientific evidence—including psychological evidence—in federal court. In the *Daubert* decision, the Court established a two-prong test for the admissibility of scientific evidence. First, evidence has to be relevant to an issue to be decided in the case. Second, the evidence must be reliable; specifically, the methodology used to produce the science must be scientifically valid. The Court specified a nonexhaustive list of criteria for determining the reliability of scientific evidence, including whether the theory on which the expert relied and the hypotheses being tested were falsifiable, whether a known error rate is associated with the topic of the expert testimony, whether the research had been subjected to peer review, and whether the research findings were generally accepted by the relevant scientific community. Judges in federal courts and the many state courts that have adopted the *Daubert* standard are tasked with serving as the gatekeepers, determining whether scientific evidence proffered in a case meets these standards of admissibility. Those states that have not adopted *Daubert*-like standards likely follow the *Frye* standard, which holds that judges should admit novel scientific evidence if it is generally accepted by the

relevant scientific community. A subsequent Supreme Court decision clarified that the *Daubert* standard should be applied to the evaluation of nonscientific expert evidence as well (*Kumho Tire Co. v. Carmichael*, 1999).

Several psychological assumptions underlie the *Daubert* decision. First, the Court assumed that judges are capable of identifying flawed evidence and barring its admission from evidence. Although the Court presumed that judges were up to the task of identifying flawed science, the justices acknowledged that there might be circumstances under which invalid science is admitted into evidence. The second assumption made by the court is that procedural safeguards already in place—cross-examination, opposing experts, and judicial instruction on the standard of proof—were sufficient to educate the jury, allowing them to evaluate the validity of science and weight it appropriately. These assumptions clearly rest on the ability of judges and jurors to recognize invalid science but also on the abilities of attorneys because it is attorneys who will file motions to exclude expert testimony—in which they must be able to highlight the inadequacies of the science for the judge, cross-examine expert witnesses, and make decisions about when it is appropriate to call their own expert to discredit the other side (Kovera, Russano, & McAuliff, 2002). Do judges, attorneys, and jurors have the capabilities to evaluate the quality of the scientific evidence that the courts presume that they have?

Because most judges, attorneys, and jurors have not received any formal training in the scientific method (Lehman, Lempert, & Nisbett, 1988; Kovera & McAuliff, 2000; McAuliff & Kovera, 2008), it is likely that their scientific reasoning abilities are similar to the abilities of other laypeople. Laypeople often fail to recognize flaws in research (Nisbett, 1993). People do not understand the importance of a control group when testing the effect of a variable, and they do not apply statistical concepts when reasoning about common social behaviors (Jepson, Krantz, & Nisbett, 1983; Nisbett, Fong, Lehman, & Cheng, 1987; Tversky & Kahneman, 1974). Jurors are laypeople and therefore should perform no better than other laypeople on these tasks unless something about trial procedure (e.g., cross-examination) or evidence (e.g., opposing experts) helps to educate them. Law school education does not improve people's ability to reason about methodology or statistics (Lehman et al., 1988). Thus, social psychological evidence suggests that the legal decision makers may not have the abilities that the *Daubert* decision presumes; however, there may be factors present in the legal environment that improve the abilities of these decision makers. To test this possibility, researchers have been actively investigating the ability of legal decision makers to evaluate scientific evidence.

Judges' Evaluations of Scientific Evidence

Law school curricula rarely include scientific training (Lehman et al., 1988), but through their experience on the bench evaluating expert evidence or through continuing legal or judicial education, judges may have been exposed to scientific principles often enough that they have developed an ability to differentiate between flawed and valid expert evidence. Surveys and archival studies suggest that judges are not engaging in high-level scientific reasoning when making admissibility decisions about scientific evidence. Judges' self-reports suggest that they rely on experts' credentials and experience and not the content of their testimony when evaluating expert testimony (Shuman, Whitaker, & Champagne, 1994). Another survey of judges revealed that only a small percentage (4%–6%) of them accurately understand the *Daubert* criteria of falsifiability and error rates (Gatowski et al., 2001). Despite mentioning *Daubert* in their appellate decisions about the admissibility of expert evidence, judges do not write about specific features of reliability in those decisions (Groscup, Penrod, Studebaker, Huss, & O'Neil, 2002).

There has been only one experimental test of whether judges are sensitive to variations in the methodological quality of scientific evidence (Kovera & McAuliff, 2000). Embedded in a survey sent to Florida circuit court judges was a description of the fact pattern in a hostile work environment sexual harassment case and the expert testimony that the plaintiff intended to proffer at trial. Some judges received a description of a valid study about which the expert would testify, whereas others received an altered version of the study that introduced one of three internal validity flaws: a missing control group, a confound, or a confederate who was not blind to experimental conditions. The study's internal validity did not affect judges' ratings of the study quality or their willingness to admit the testimony (Kovera & McAuliff, 2000). Judges with some self-reported training in the scientific method did rate the internally valid study more positively than did untrained judges. Untrained judges rated the study with the confound more positively than did trained judges, and their responses to open-ended questions indicated that their appreciation of this particular version of the study stemmed from their misunderstanding of appropriate scientific method (Kovera et al., 2002). Training did not increase judges' sensitivity to missing control groups or the potential for experimenter expectancy effects from a confederate who was not blind to condition.

Taken together, these studies suggest that methodological quality exerts little to no influence on judges' ratings of scientific quality or their admissibility decisions. What

variables do influence these judgments? Whether a study had been peer-reviewed did not influence judges' decisions in one study (Kovera & McAuliff, 2000). Other research suggests that judges' sociopolitical attitudes influence their decisions to admit expert evidence, with judges more likely to admit findings that support their political orientation toward a particular issue (Redding & Reppucci, 1999).

Attorneys' Evaluations of Expert Evidence

Attorneys need to be able to identify flawed research if they are to argue effectively to judges that they should exclude invalid research from evidence or to cross-examine expert witnesses in a manner that highlights its flaws for jurors. Motions *in limine* to oppose the admission of a particular topic of expert testimony into evidence is one method through which judges could receive information that would help them evaluate the methodological quality of proffered science. A motion *in limine* is a pretrial motion submitted by an attorney, and in the context of expert evidence, it would contain arguments for or against the admission of a particular expert or topic of expert testimony. If the motion *in limine* is unsuccessful and invalid research is admitted into evidence, attorneys need to be able to understand scientific methodology to be able to demonstrate problems with invalid science during cross-examination or to know that it would be helpful to hire an expert to present evidence on why the science presented by the other side has methodological problems. Whether motions *in limine* can serve to educate judges about the characteristics of valid and invalid science, whether attorneys can craft a cross-examination that highlights scientific flaws, or whether attorneys hire opposing experts to help combat flawed science entered into evidence by the other side is predicated on attorneys' ability to recognize when and why science is invalid.

At the end of law school, law students still lack the ability to reason about methodology in everyday situations (Lehman et al., 1988). A national sample of attorneys specializing in employment law responded to the same expert evidence used in the survey of judges described earlier, including the manipulation of whether the study described in the expert testimony was valid, missing a control group, confounded, or had a confederate who was not blind to condition (Kovera & McAuliff, 2009). The study also manipulated the general acceptance of the research; in one set of conditions, the relevant scientific community had generally accepted the expert's findings, and in another set, the research had just been completed, which did not allow time for the findings to become generally accepted. The manipulations of internal validity did not influence attorneys' ratings of the quality of the expert's

study, but the manipulation of general acceptance did, with attorneys responding more positively to the generally accepted research. The study characteristics also had no effect on attorneys' reports of whether they would file a motion to exclude the expert evidence—close to 95% reported that they would—and their reports of the grounds on which they would base their objection to the testimony rarely mentioned the three methodological flaws contained in the study. Half of the attorneys who did cite one of these flaws did so when the flaw was not present, suggesting that attorneys might well provide incorrect information to judges who are already struggling to evaluate the scientific evidence without the proper skills and abilities that would allow them to do so.

Juror Decisions About Expert Evidence

Jurors also have difficulty evaluating expert scientific evidence. Mock jurors who watched a trial simulation containing an expert who testified about research she had conducted on the influence of exposure to sexually suggestive material in the workplace noticed a manipulation of construct validity, but the manipulation did not affect their ratings of the quality of the study, the expert, or their verdicts (Kovera et al., 1999). In contrast, manipulations of the representativeness of the expert's sample and general acceptance did influence jurors' trial judgments. Another study found that whether the expert's study contained a control group did not affect the reactions of community members who had reported for jury duty to a trial summary containing expert testimony unless the community member was high in Need for Cognition (McAuliff & Kovera, 2008). In a follow-up study, the presence of a control group, but not the presence of a confound or a nonblind confederate, affected jury-eligible community members' judgments of study quality and the expert's credibility (McAuliff, Kovera, & Nunez, 2009).

These studies raise concerns about jurors' abilities to recognize methodological flaws in scientific evidence. These findings are consistent with other research showing that jurors have difficulty reasoning about statistical evidence (Schklar & Diamond, 1999; Smith, Penrod, Otto, & Park, 1996; Thompson & Schumann, 1987). Of course, jurors do not make these decisions in a vacuum, and procedural safeguards such as cross-examination, opposing expert testimony, and judicial instruction may sensitize jurors to flaws that could exist in scientific evidence entered into evidence.

Instruction on the Standard of Proof

In the majority opinion of *Daubert*, the Court held that careful instruction on the standard of proof could assist

jurors in assigning appropriate weight to unreliable expert evidence. There are currently no published studies that directly tested this assumption. However, the logic underlying the argument seems faulty given that the instructions provide no guidance on how to determine which evidence is reliable and which is not; the instruction on the standard of proof should only serve to make jurors more or less skeptical of expert testimony rather than sensitive to variations in validity. Moreover, a survey of previously impaneled jurors suggests that most jurors do not understand the standards of proof even after receiving instruction on them (Reifman, Gusick, & Ellsworth, 1992). Other research testing people's understanding of proof instructions similarly finds they have poor comprehension (Strawn & Buchanan, 1976). Given the available data, increased skepticism of scientific expert evidence may be the most reasonable expectation to hold for judicial instructions on the standard of proof.

Cross-Examination

For cross-examination to be an effective procedural safeguard, two conditions must be met. First, attorneys must be able to identify methodological flaws in scientific research and then pose the expert questions about those flaws during cross-examination. Second, cross-examination must make jurors sensitive to those methodological flaws rather than merely make them skeptical of all expert evidence, irrespective of whether it is valid or invalid. The currently available evidence does not support either of these conditions.

Regarding the first condition, as noted previously, variations in internal validity did not affect attorneys' ratings of a study's methodological quality (Kovera & McAuliff, 2009). In their study of attorneys' reactions to variations in the internal validity of a study, attorneys generated cross-examination questions that they would use to cross-examine the hypothetical expert described in the scenario they read (Kovera & McAuliff, 2009). Attorneys rarely mentioned internal validity issues in the questions they crafted, and when they did, they were equally likely to mention internal validity characteristics when cross-examining the expert presenting the valid study than when cross-examining the expert presenting one of the studies with a serious flaw. Rather than focusing their cross-examination questions on issues of methodology, the attorneys tended to focus on the expert's qualifications or potential sources of bias, such as the expert's fees or history of testifying on behalf of plaintiffs.

So it is unlikely that attorneys will highlight methodological flaws in their cross-examinations of experts presenting flawed science, but what would happen if jurors heard an attorney attack the methodological foundation of an expert's research? Would a scientifically informed

cross-examination educate jurors so that they could identify flawed science and weight it appropriately? Two studies have examined the effects of cross-examination on jurors' evaluations of clinical versus actuarial testimony on future dangerousness. The first manipulated the basis of the expert's testimony—clinical opinion versus actuarial prediction—on the risk that the defendant would be dangerous in the future (Krauss & Sales, 2001). Despite the stronger scientific basis for the actuarial expert's prediction, jurors preferred the clinical expert. Although cross-examination did produce less positive ratings of the expert and reduced the perceived future risk of dangerousness, even after exposure to cross-examination, the jurors responded more favorably to clinical than to the more scientifically based actuarial testimony. The addition of jury deliberations to the design did not improve cross-examination's efficacy in countering clinical testimony (Krauss & Lee, 2003).

In a different study, mock jurors heard evidence from an expert that varied in its construct validity; an attorney questioned that expert using a scientifically naive cross-examination that only addressed the expert's qualifications and credibility or a scientifically informed cross-examination that also addressed the construct validity of the expert's study (Kovera et al., 1999). Although manipulation checks indicated that participants noticed the differences in the expert's study, the differences did not affect their trial judgments, even after exposure to the scientifically informed cross-examination. On the basis of the research to date, cross-examination does not sensitize jurors to the methodological quality of expert scientific evidence, at least not as it is currently practiced.

Opposing Experts

Perhaps one of the reasons that cross-examination does not influence jurors' evaluations of scientific evidence is because the methodological criticisms come from a partisan advocate who is not a trained scientist. Would criticism conveyed by another expert more effectively sensitize jurors to methodological flaws in scientific evidence? For the opposing expert safeguard to be effective, attorneys would need to consult their own expert when the testimony proffered by the other side contains methodological flaws. Yet manipulations of internal validity had no effect on attorneys' self-reported intentions to hire their own expert. Attorneys' perceptions of the quality of the expert evidence—which were unrelated to the manipulations of internal validity—were correlated with their willingness to hire an expert but in an unexpected way (Kovera & McAuliff, 2009). Attorneys reported being more likely to hire their own expert when they believed that the other side's expert was providing

higher quality testimony, perhaps believing themselves up to the task of discrediting an expert providing lower quality testimony.

Can an opposing expert help jurors better evaluate the validity of the other expert's testimony? One jury simulation study that manipulated (a) the internal validity of research on child suggestibility presented by a defense expert and (b) whether an opposing expert testified for the prosecution and, if so, whether the opposing expert attacked the defense expert's willingness to generalize from laboratory studies to real-world contexts only or also testified about the internal validity of the defense expert's study. In this research, the opposing expert did little to sensitize jurors to the quality of the defense expert's study, even when directly addressing its internal validity (Levett & Kovera, 2008). Instead, the opposing expert served to make the jurors skeptical of the defense expert, irrespective of the quality of that expert's testimony and whether the opposing expert addressed that quality. The skepticism effect created by the opposing expert appears to be mediated by juror judgments about the general acceptance of the original expert's testimony (Levett & Kovera, 2009). Specifically, when an opposing expert was present, jurors inferred that scientists did not generally accept the original expert's findings, and this inference led to more guilty verdicts (i.e., verdicts more in line with the opposing expert's position).

In sum, research conducted since the late 1990s suggests that legal decision makers do not have the abilities to evaluate scientific evidence that courts presume they have. Moreover, the legal safeguards designed to assist jurors in their evaluations of unreliable scientific evidence—especially cross-examination and opposing expert testimony—appear ineffective. This lack of efficacy appears due in part to attorneys' inability to deploy them effectively and in part because even if the safeguards are deployed, they do not seem to sensitize jurors effectively to methodological flaws.

SUMMARY

The lawyer and the judge and the juryman are sure that they do not need the experimental psychologist. They do not wish to see that in this field preeminently applied experimental psychology has made strong strides They go on thinking that their legal instinct and their common sense supplies them with all that is needed and somewhat more; and if the time is ever to come when the jurist is to show some concession to the spirit of modern psychology, public opinion will have to exert some pressure.

(Munsterberg, 1908, pp. 10–11)

At the beginning of and late into the 20th century, Hugo Munsterberg's analysis of the law's skepticism about psychological science was widely shared. Yet as the applications of social psychology to the legal context covered in this chapter suggest, this skepticism has begrudgingly yielded to a greater openness to the "spirit" and reality of "modern psychology." Not only has theory and research from social psychology been applied to a variety of legal questions and problems, but theory and research in the field has been stimulated by the legal context. This type of reciprocal influence is surely a hallmark of interdisciplinary research more generally. However, as the research discussed in this chapter suggests, substantive applications of social psychology to various legal contexts, more so than theory development per se, characterize the relationship between law and social psychology even now in the 21st century.

Several critical and complex questions face social psychology and law researchers at this juncture. How should existing social science research, including but not limited to theory and research from social psychology, be used in litigation and policy formulations? Under what circumstances will general social science findings help courts and juries? What sorts of use-specific opinions should courts allow experts to give, and how should experts proffer these opinions? These questions have spawned a lively and wide-ranging debate among researchers and legal scholars at the intersection of social psychology and antidiscrimination law (e.g., Krieger & Fiske, 2006; Lane, Kang, & Banaji, 2007; Mitchell & Tetlock, 2006; Tetlock & Mitchell, in press; Wax, 2008). Crucial to addressing all of these questions is the basic tenet that quality, peer-reviewed science should be the foundation for any expert testimony. Quality science provides the best scientific context for understanding how people make sense of each other (Fiske & Borgida, 2008). For social scientists, the adversarial context also has the potential to raise vital scientific issues and to enrich theory development by posing empirically testable research questions (e.g., see special issue of *Industrial and Organizational Psychology: Perspectives on Science and Practice*, December 2008).

Quality research presented at the aggregate level (general causation) can inform fact finders without any testimony necessarily making specific causal claims (specific causation) in a given case (Faigman, 2008; Faigman & Monahan, 2005). This distinction between general and specific causation arises from medical causation and toxic tort cases (Faigman, Saks, Sanders, & Cheng, 2007). *General causation* concerns whether causality between two factors exists at all (e.g., is there a relationship between exposure to media violence and interpersonal aggression?), and *specific causation* refers to whether the phenomenon of interest occurred in a particular context (e.g., did exposure to media

violence cause a specific homicide?). State and federal courts are increasingly accepting of research presented in the form of a *social framework* that uses science to provide context and to educate fact finders (Monahan, Walker, & Mitchell, 2008); however, some scholars have called for social framework testimony to be limited to testimony that educates fact finders about general causation but not specific causation issues (Monahan et al., 2008). There also has to be appropriate "fit" between the scientific evidence that is presented and the specific legal issues(s) to which it is directed, with some scholars arguing that there is a lack of fit between extant aggregate-level research findings and specific employer decisions in employment discrimination disputes (Faigman et al., 2008).

But this question of social frameworks and their "linkage" to the specific facts of a case has emerged as perhaps the key question for understanding the application and use of research in criminal and civil law contexts. Some have argued that using science to provide a context for the fact finder is appropriate, but making specific applications to the facts of the case is inappropriate and should be precluded in all instances (Monahan et al., 2008). Others do not agree about the categorical exclusion of "linkage" between general causation and specific causation and *how* social framework testimony should be offered, arguing that Federal Rule of Evidence 702 (on expert testimony) is flexible and not subject to categorical exclusions and that expert testimony may properly include opinions on facts at issue in a case (Hart & Secunda, in press). "What the Supreme Court decisions, as well as numerous opinions from the lower courts, demonstrate is that the admissibility of expert opinion testimony linking a field of knowledge with the case facts is an open domain, sensitive to the circumstances" (Hart & Secunda, in press). On the other hand, social framework testimony may not always be permitted. If judicial determinations about linkage will be at the district court level, then decisions about "linkage" testimony will and should continue to be individualized (Hart & Secunda, in press).

Another approach to this question of general versus specific causation from a scientific perspective relies on an analogy from medical science (Borgida, Eagly, & Deason, 2009). Typical physician diagnosis behavior, for example, does not follow this "general causation" model. Instead, physicians offer diagnoses as if they were applied scientists. Because symptoms can arise from several diseases, physicians begin by *ruling out* some diseases that can produce the symptoms (Borgida et al., 2009). Based on experience and scientific expertise, the physician then would evaluate the plausibility of each of the most likely causes.

Although physicians are certainly not perfect (see Groopman, 2007), they generally have the superior knowledge of the science that is relevant to diagnosing a

patient's condition. This expertise is a valuable resource in a complicated world where casual ad hoc explanations are more plentiful than correct conclusions. After the physician offers a diagnosis, the patient has many options. They may accept or refuse the recommended treatment, seek the opinion of other physicians, consult a faith healer, adopt a strategy of watchful waiting, engage in psychological denial, or adopt a wide variety of other strategies. The physician's judgment informs the patient without usurping his or her role as the ultimate judge of which disease most plausibly explains the symptom.

The social science expert serves a similar purpose when, for example, attempting a "diagnosis" of the causes of a negative workplace outcome or the factors that could contribute to a mistaken identification or false confession (Borgida et al., 2009). Although the social scientist's judgment is not perfect, it is likely to be superior to uninformed judgment and can assist the trier of fact without usurping the decision-making role (Borgida & Fiske, 2008). Social scientists may be able to rule out some possible causes of the focal event (e.g., job termination, eyewitness identification, confession) and rule *in* one or more other causes, much like a physician applying his or her knowledge to a particular patient and set of symptoms. Analogous to physicians' ability to correctly diagnose a patient's physical symptoms, scientists' ability to correctly discern the linkage between general and specific causation for the individual case depends on the quality of the scientific evidence and the scientist's expertise and ability to reason based on this evidence.

The adversarial nature of the court system seldom gives social scientists the opportunity to freely invoke the power of the available science. Scientific experts are retained either by those who wish to rule in a cause or by those who wish to rule it out; a full scientific analysis is rarely welcomed by either side of the typical courtroom exchange. The often proposed idea that the courts should retain neutral science advisors to achieve some sort of scientific consensus remains appealing though without any practical momentum (Saks & Faigman, 2005). In the end, it is the responsibility of the scientists who bring their insights into court to resist these adversarial pressures to present the science in a selective and partisan manner and to present fact finders with quality science.

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