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Eyewitness Identification Procedures: Recommendations for Lineups and Photospreads

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There is increasing evidence that false eyewitness identification is the primary cause of the conviction of innocent people. In 1996, the American Psychology/Law Society and Division 41 of the American Psychological Association appointed a subcommittee to review scientific evidence and make recommendations regarding the best procedures for constructing and conducting lineups and photospreads. Three important themes from the scientific literature relevant to lineup methods were identified and reviewed, namely relative-judgment processes, the lineups-as-experiments analogy, and confidence malleability. Recommendations are made that double-blind lineup testing should be used, that eyewitnesses should be forewarned that the culprit might not be present, that distractors should be selected based on the eyewitness's verbal description of the perpetrator, and that confidence should be assessed and recorded at the time of identification. The potential costs and benefits of these recommendations are discussed.

INTRODUCTION

In 1996, the Executive Committee of the American Psychology/Law Society (AP/LS) appointed a subcommittee to draft good-practice guidelines for constructing and conducting lineups and photospreads for eyewitnesses to crimes.[8] The members of the subcommittee are the authors of the current article. In March 1998, the Executive Committee of AP/LS voted to accept the current article as an official Scientific Review Paper of the AP/LS.

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Problem Being Addressed

The moment in which an eyewitness views a lineup and identifies a criminal suspect is a significant juncture in the possible criminal prosecution of that person.[9] Over the last two decades, psychological scientists have devoted considerable effort to the study of eyewitness identification and have articulated concerns about the accuracy of eyewitness identifications under certain conditions. Relying primarily on controlled experiments (e.g., staged crimes), psychologists have developed a deeper understanding of the ways in which lineup procedures can have a great deal of impact on the accuracy of the identifications that are obtained. We use the term *lineup procedures* to refer to a variety of methodological components of lineups. These include both the structural properties of lineups (e.g., appearance characteristics of lineup members) and the procedural properties of lineups (e.g., instructions given to eyewitnesses prior to viewing).

The scientific study of eyewitness testimony has been one of the most successful applied research topics in scientific psychology over the last two decades. Research on eyewitness testimony has covered considerable ground in the last 25 years, including such problems as how jurors evaluate eyewitness testimony (e.g., Cutler, Penrod, & Stuve, 1988; Wells, 1984a), the effectiveness of legal safeguards (e.g., Stinson, Devenport, Cutler, & Kravitz, 1996), the abilities of children relative to adults to recall witnessed events (e.g., Ceci, Toglia, & Ross, 1987), the reconstructive aspects of eyewitness memories (e.g., Loftus, 1979), the effects of stress (e.g., Christianson, 1992), techniques for interviewing eyewitnesses (e.g., Fisher, 1995), individual differences in eyewitness abilities (e.g., Hosch, 1994), the development of false autobiographical memories (e.g., Lindsay & Read, 1995; Loftus & Ketcham, 1994), and the extent of agreement among eyewitness experts regarding various findings (Kassin, Ellsworth, & Smith, 1989). Although the study of eyewitness testimony has been quite broad, the study of eyewitness identification has been a particularly strong focus for eyewitness researchers because false identification can directly incriminate an innocent suspect. An eyewitness who says "That is the person I saw pull the gun" is providing direct evidence of guilt in the sense that the criminal act and the defendant are directly linked. In contrast, physical evidence such as fingerprints indicate only that the suspect touched a given surface at some point in time, perhaps for reasons unrelated to the crime, and hence is circumstantial evidence.

Scientific eyewitness researchers have shown that certain methods of conducting lineups are particularly likely to promote false identifications of innocent suspects by eyewitnesses. The idea that these methodological variables are under the control of the criminal justice system has been a guiding idea in the psychological literature for 20 years (Wells, 1978). The experimental literature has shown the role of these system variables in facilitating false eyewitness identifications in a variety of different ways.

[9]For the purposes of this review and these recommendations, no distinction is made between live lineups and photospreads unless otherwise noted.

In addition to the experimental literature, cases of proven wrongful convictions of innocent people have consistently shown that mistaken eyewitness identification is responsible for more of these wrongful convictions than all other causes combined (e.g., Borchard, 1932; Brandon & Davies, 1973; Frank & Frank, 1957; Huff, Rattner, & Sagarin, 1986). More recently, the introduction of forensic DNA testing procedures in the United States has allowed for the testing of people who were convicted of crimes prior to the development of such tests. In cases where people were convicted prior to the introduction of forensic DNA analysis and for which DNA-rich evidence (e.g., semen, blood) was preserved, a subset has now been analyzed and has resulted in the exoneration of people who were innocent of the crime for which they were convicted. A recent report commissioned by the National Institute of Justice (Connors, Lundregan, Miller, & McEwan, 1996) examined 28 cases of DNA exoneration. For purposes of the present Scientific Review Paper we have added 12 more cases. All 40 of these cases represent innocent people who were convicted of serious crimes and served time in prison, five on death row, until DNA analyses in the 1990s were able to prove their innocence. Table 1 lists the 40 cases.

Noteworthy in this sample is the number of cases for which eyewitness identification was used to convict the innocent person. Of these 40 cases, 36 (or 90%) involved eyewitness identification evidence in which one or more eyewitnesses falsely identified the person. One person was identified by five separate eyewitnesses. It is important to note that the 40 cases in Table 1 were not selected because they happen to have eyewitness identification as the primary evidence. Instead, these cases are simply the first 40 cases in the United States in which DNA was used to exonerate a previously convicted person. Hence, the kind of evidence that led to these wrongful convictions could have been anything. The fact that it happens to be eyewitness identification evidence lends support to the argument that eyewitness identification evidence is among the least reliable forms of evidence and yet is persuasive to juries.

Although it was juries who convicted these innocent people, our recommendations are not being addressed to juries (except to the extent that they may hear discussion of them in expert testimony). We are not recommending, for instance, that juries should become more skeptical of eyewitness identification evidence. Although there are many eyewitness experts who contend that just such a warning is in order, our approach is to make the eyewitness identification evidence more reliable rather than make juries more skeptical.

The U. S. judiciary has not been oblivious to the problem of mistaken identification. In *Simmons v. United States* (1968), the Supreme Court recognized the role that procedures might play in facilitating a false identification when the police show a photograph of a single individual to a witness for identification purposes:

> It must be recognized that improper employment of photographs by police may sometimes cause witnesses to err in identifying criminals. A witness may have obtained only a brief glimpse of a criminal, or may have seen him under poor conditions. This danger will be increased if the police display to the witness only the picture of a single individual who generally resembles the person he saw, or if they show him the pictures of several persons among which the photograph of a single individual recurs or is in some way emphasized. The chance of misidentification is also heightened if the police indicate to the witness that they have other evidence that one of the persons pictured committed the crime (*Simmons v. United States*, 1968, p. 967).

Table 1. A Sample of 40 Cases in Which DNA Evidence Exonerated Persons Wrongfully Convicted of Crimes

Name	Charges	Year convicted	Original sentence/ years served	Evidence producing conviction
Adams, Kenneth	Two counts murder, rape	1978	75/16	**Witness id**
Alejandro, Gilbert	Sexual assault	1990	12/4	Blood evidence testimony; **victim id**
Bloodsworth, Kirk	Murder, rape	1985	Death, reduced to life/9	**Five witness ids;** self-incriminating statements
Bravo, Mark Diaz	Rape	1990	8/3	**Victim id;** blood analysis; misrepresentation
Brison, Dale	Rape, kidnapping	1991	18–42/3.5	**Victim id;** hair analysis; weak alibi
Bullock, Ronnie	Aggravated sexual assault	1984	60/10.5	**Two victim ids; police id;** proximity of residence
Callace, Leonard	Sodomy, sexual abuse	1987	25–50/6	**Victim id;** blood analysis; weak alibi
Chalmers, Terry Leon	Rape, sodomy	1987	12–24/8	**Victim id;** weak alibi
Cotton, Ronald	Rape (2 counts)	1985, 1987 (2nd trial)	Life+ 54/10.5	**Victim id;** similarity of shoes and flashlight
Cruz, Rolando	Murder, kidnapping, rape	1985	Death/11	Alleged "dream visions" of the murder; witness statements
Dabbs, charles	Rape	1984	12.5–20/7	**Victim id;** blood analysis
Davis, Gerald Wayne	Kidnapping, sexual assault (2 counts)	1986	14–35/8	**Victim id;** semen analysis
Daye, Frederick Rene	Rape (2 counts), kidnapping	1984	Life/10	**Victim id; witness id;** blood analysis; misrepresentation
Dotson, Gary	Rape, aggravated kidnapping	1979	25–50/8	**Victim id;** semen analysis; **hair analysis**
Green, Edward	Rape	1989	Never sentenced/ 9 months	**Victim id;** blood analysis
Hammond, Ricky	Sexual assault, kidnapping	1990	25 and 3 probation/2	**Victim id;** victim id of car; hair analysis; weak alibi
Harris, William O'Dell	Sexual assault	1987	10–20/7; 1 home	**Victim id;** semen analysis
Hernandez, Alejandro	Murder, kidnapping, rape	1985	Death/11	Self-incriminating and inculpatory statements; witness statements

Table 1. Continued

Name	Charges	Year convicted	Original sentence/ years served	Evidence producing conviction
Honaker, Edward	Rape, sexual assault, sodomy	1985	3 life + 34/10	**Victim id; witness id;** hair analysis; similarity of clothing
Jimmerson, Verneal	Two counts murder, 2 counts aggravated kidnapping, rape	1978	Death/11	**Witness id**
Johnson, Richard	Armed robbery, sexual assault	1990	36/6	**Two victim ids,** semen analysis, fingerprints
Jones Joe C.	Rape, aggravated kidnapping	1986	Life + 10–25/6.5	**Victim id;** proximity to crime scene; similarity of pants; **2 witness ids**
Kotler, Kerry	Rape (2 counts)	1982	25–50/11	**Victim id;** non-DNA genetic analysis
Linscott, Steven	Murder rape	1982	40/3 in prison; 7 on bond	Blood analysis; hair analysis; "dream confession"
Mitchell, Marvin	Forced intercourse w/minor, unnatural sex w/minor	1990	9–25/8	**Victim id,** semen analysis, self-incriminating statement
Motto, Vincent	Rape, robbery, deviate sex, criminal conspiracy	1987	12–24/9	**Victim id**
Nelson, Bruce	Murder, rape	1982	Life/9	Testimony of codefendant, self-incriminating
Oritz, Victor	Rape, sodomy, deviate intercourse	1984	12.5–25 concurrent/ 12	**Victim id,** semen analysis
Piszczek, Brian	Rape	1991	15–25/4	**Victim id; weak alibi**
Rainge, Willie	Two counts murder, 2 counts aggravated kidnapping, rape	1978	Life/18	**Witness id**
Scruggs, Dwayne	Rape	1986	40/7.5	**Victim id;** similarity of boots
Shephard, David	Rape	1984	30/10	**Victim id;** blood analysis; **weak alibi**
Smith, Walter	2 counts rape	1986	78–190/11	**Victim id**
Snyder, Walter (Tony)	Rape, sodomy	1986	45/7	**Victim id;** similarity of clothing; blood analysis; **weak alibi**
Toney, Steven	Sodomy, rape	1982	Two consecutive life/14	**Victim id, witness id**

Table 1. Continued

Name	Charges	Year convicted	Original sentence/ years served	Evidence producing conviction
Vasquez, David	Murder, rape	1985	35/5	**Witness id**; no alibi; confession; hair analysis
Web, Thomas	Rape	1983	70/13	**Victim id**
Williams, Dennis	Two counts murder, 2 counts aggravated kidnapping, rape	1978	Death/18	**Witness id**
Woodall, Glen	Sexual assault, kidnapping	1987	2 life + 203–335/4, then 1 home	Blood analysis; hair analysis; **victim id**; similarity of clothing

The Court's ruling in *Simmons* was later undermined by several rulings in the 1970s that changed dramatically the level of analysis by which courts should evaluate eyewitness identification evidence. Central among these was the ruling that even highly suggestive procedures are not per se reasons for exclusion because they do not necessarily undermine the reliability of the identification (*Neil v. Biggers*, 1972; *Manson v. Braithwaite*, 1977). The Court in *Biggers* and *Braithwaite* stressed five criteria: (a) the opportunity of the eyewitness to view the offender at the time of the crime, (b) the witness's degree of attention, (c) the accuracy of the witness's prior description of the offender, (d) the level of certainty displayed by the witness at the identification procedure, and (e) the length of time between the crime and the identification procedure. These five criteria have been criticized by eyewitness researchers on a number of grounds (Wells & Murray, 1983). For example, accuracy of description is a rather poor predictor of accuracy of identification (Piggot & Brigham, 1985; Wells, 1985). Even more problematic is that biased lineup procedures can actually lead eyewitnesses to overestimate how good of a view they had of the perpetrator as well as lead them to develop false confidence (Wells & Bradfield, 1998). Nevertheless, the five *Biggers* criteria remain the primary guides used in U.S. courts today. In another important ruling, the U.S. Supreme Court stated that there is no right to counsel at photoidentification procedures (*United States v. Ash*, 1973).

In legal theory, various safeguards are presumed to be operating within the justice system to prevent miscarriages of justice in the form of mistaken identification. These safeguards, however, fail to provide the intended protection. The safeguards include presence of counsel at live lineups (if they are postindictment), opportunities for motions to suppress identifications, cross examination of identifying eyewitnesses, and expert testimony about the factors that influence eyewitness memory. These safeguards, however, fail to provide protection in a variety of ways (see Devenport, Penrod, & Cutler, 1998, for a review). For instance, most identifications of criminal suspects are from photos rather than live lineups and the U.S.

Supreme Court has ruled that there is no right to counsel at photoidentification sessions (*United States v. Ash*, 1973). Even with the rare presence of counsel, there is little guarantee that attorneys will serve as an effective safeguard (Stinson et al., 1996). Opportunities to make motions to suppress eyewitness identifications can be submitted, but research shows that judges, like attorneys, are not particularly adept at evaluating eyewitness identification procedures (Stinson, Devenport, Cutler, & Kravitz, 1997). Cross-examination, a marvelous tool for helping jurors discriminate between witnesses who are intentionally deceptive and those who are truthful, is largely useless for detecting witnesses who are trying to be truthful but are genuinely mistaken (e.g., Wells, Lindsay, & Ferguson, 1979). Expert testimony, while useful for enhancing jurors' appreciation of some variables affecting eyewitness identification testimony, may not serve as well to increase jurors' appreciation of factors influencing lineup suggestiveness (Devenport et al., 1998). Furthermore, expert testimony regarding eyewitness issues frequently is not allowed by many judges, who have broad discretion on the issue of admissability. Even when expert testimony is allowed, it is an expensive type of safeguard that is available only to very small fraction of the conservatively estimated 77,000 suspects per year in the United States who become defendants based on their being identified by an eyewitness. Although the Unites States has the most eyewitness experts in the world, these experts in total could not account for more than perhaps 500 cases per year.

The Case for Guidelines

The practice of obtaining identifications of criminal suspects from lineups has operated largely independently of the emergence of a science of eyewitness identification. Although expert testimony by eyewitness scientists in individual criminal cases in recent years likely has raised awareness of the procedural issues in some jurisdictions (Leippe, 1995; Penrod, Fulero, & Cutler, 1995), there is no set of legal rules of procedure for obtaining eyewitness identifications that law enforcement investigators must follow. The procedural recommendations that we propose in this paper represent an emerging consensus among eyewitness scientists as to key elements that such a set of procedures must entail. We believe that such procedures can dramatically reduce the risk of mistaken identification.

The idea of drafting guidelines for identification procedures is not new. In 1955, an article in the *UCLA Law Review* called for procedural safeguards against mistaken identification by eyewitnesses (Comment, 1955). By the 1960s, many legal commentators were beginning to call for police departments to adopt detailed sets of written guidelines to follow in photospreads and lineups. For example, in 1975 the American Law Institute (ALI) published a *Model code of pre-arraignment procedures* (American Law Institute, 1975) which provided some general guidelines for identification procedures but which also called for mandated adoption of detailed regulations by local law enforcement agencies. By the time that the ALI published this call for procedures, a number of local police departments had done this already. Read (1969) published the regulations for New York City, Oakland, California, Washington, D.C., and Clark County, Nevada, as appendices to a *UCLA Law Review*

article. (The Clark County guidelines are discussed in more detail below.) A 1970 Note in the *Columbia Journal of Law and Social Problems* discusses regulations in Los Angeles, New Orleans, and Richmond, Virginia (Note, 1970). A 1967 *University of Pittsburgh Law Review* article discusses Pittsburgh's regulations (Comment, 1967). In Arizona, the Arizona State University School of Law put together a group that published *Model rules: Eyewitness identification* in April 1974 (Project on Law Enforcement Policy and Rulemaking, 1974).

The earliest set of published recommendations for lineup identifications is to be found in a 1967 article in the *American Criminal Law Quarterly* (Procedure for line-up identification, 1967). The article outlines a joint memorandum from the Offices of the District Attorney and the Public Defender in Clark County, Nevada, directed to "all law enforcement agencies" in the county, which contains the city of Las Vegas. According to the memorandum, "a study of the procedures used and pertinent case law" was made and a checklist for lineup procedures was then developed.

The checklist includes 15 items. The items include statements that no lineup identifications should be made without discussing the "legal advisability" of such lineup with the District Attorney's office, and that no lineup should be held without a representative of the District Attorney and the Public Defender or other defense attorney present. Mention is made that "insofar as possible," all persons in the lineup should be of the same general age and racial and physical characteristics. Any movements, gestures, or verbal statements that are necessary should be done "uniformly." All conversation between the law enforcement officer and the witnesses should be "restricted to only indispensable direction," and in all cases "nothing should be said to the witness to suggest the suspect is standing in the particular lineup." If more than one witness views the lineup, they should not be allowed "before the completion of all witnesses' attempted identification" to discuss among themselves anything about the lineup, or their identifications, or nonidentifications. Witnesses should not be allowed to see the suspect in custody or handcuffs "or in any manner that would indicate . . .the identity of the suspect." Only one witness at a time should be present in the lineup room. "All efforts should be made" to prevent a witness from viewing any photographs of the suspect prior to giving the lineup. A lineup photo is to be taken and developed "as soon as possible" and given to the defense attorney "immediately." A lineup report should be prepared and also given to the defense. Finally, a lineup identification form is presented. It instructs the witness to put an X in one or more of seven numbered squares "if you have previously seen one or more of the persons in the lineup" and then to sign the sheet and give it to the police officer.

Buckhout (1975; Buckhout & Friere, 1975; Ellison & Buckhout, 1981) developed two so-called "reliability checklists," one for lineups and one for photospreads. The checklist for lineups contains 21 items, and the checklist for photospreads contains 30 items. The items are phrased in question form (e.g., "are there less than six people in the lineup?"). A number of items focus on the existence of differences among the participants in such things as skin tone, age, height, stature, dress, and hair, or among the photos in size, color, contrast, or other characteristics. Others focus on whether witnesses have already seen photos of the suspect or prior lineups or photospreads. Other questions include whether or not there is more than one witness, or whether the witnesses had an opportunity to discuss the events. Specific

mention is made of whether any of the lineup or photospread participants differ from the original description given by the witnesses, whether or not the witness was made aware by the police that there is a suspect present in the lineup or photospread, whether the law enforcement officer knows who the suspect is and what position he is in, and if any suggestion or emphasis was made regarding one participant by the police "in word, gesture, tone, or number." One item states that there is a preference for a written response rather than a verbal one, and that the form used should contain an explicit "zero choice" representing a nonidentification. Finally, one item asks if the witness was "told in any way that he or she was 'correct' or incorrect' in making an identification." For each item or question, there is a "yes," "no," or "unknown" choice, and Buckhout instructed that a "total sources of unreliability" score be calculated by summing the total of "yes" responses.

In Canada and Great Britain, similar calls were being made. In 1983, the Law Reform Commission of Canada published a study paper authored by Neil Brooks that included 39 specific numbered recommendations along with extensive discussion of each (Law Reform Commission of Canada, 1983). By that time, guidelines had been set forth by law enforcement departments in a number of Canadian cities, including Toronto, Edmonton, Vancouver, Montreal, and Guelph. The Law Reform Commission study paper recommendations are wideranging, and include specific directions on obtaining descriptions, the use of sketches and composites as well as mugshots, and even include discussions of blank lineups and the use of sequential presentations.

Meanwhile, in England, the Home Office published two important documents, one in 1976 and in 1978. The 1976 document was entitled *Report to the Secretary of State for the Home Department of the Departmental Committee on Evidence of Identification in Criminal Cases* (Home Office, 1976). For obvious reasons, it became known under its abbreviated title as the "Devlin Report." This report is nearly 200 pages long, divided into eight chapters and a number of appendices. The first chapter is an introduction to the problem of "honest but mistaken identification." The second and third chapters are detailed discussions of two specific cases. The fourth chapter addresses the use of identification evidence at trial, and contains lengthy discussion and then rejection of a requirement for corroborating evidence of a different kind in identification cases, as well as discussion of how to handle in-court or "dock" identifications and the judge's instructions in identification cases. Chapter 5 is most relevant here, as it addresses pretrial identification procedures including lineups and photospreads. The appendices include a 1969 *Home Office Circular No. 9* which contains rules for "identification parades," as well as various forms used in identification procedures.

The 1978 document was known as *Home Office Circular 109* (Home Office, 1978). It contained two separate codes, one for the use of lineups or "identification parades" and one for the use of photos. Each was divided into rules and a detailed narrative for the law enforcement called "Administrative Guidance." Unfortunately, none of the recommendations in the Home Office Circulars or in the Devlin Report carried the force of law, although eventually Parliament passed the Police and Criminal Evidence Act 1984 setting forth specific statutory identification procedures that applied to all identification procedures conducted after April 1985.

Prosecutors, too, have begun to address the issue of appropriate lineup and photospread procedures, at times in response to cases in which police techniques have been criticized by psychologists in expert testimony. Thus, for example, Collins (1989), the prosecuting attorney in Tuscarawas County, Ohio, published an article in *The Ohio Prosecutor* in which he discusses identification techniques and provides a "short list of advice" for law enforcement officers, including not showing a single suspect or photograph to a witness, making the individuals in a lineup or photospread "fairly close to the suspect" in age, race, hair length, and other aspects of appearance, not using photos with police number or height charts, and doing both a live lineup and a photospread "with some individuals common to both procedures."

The most detailed set of recommendations published thus far is contained in Wells' 1988 book, *Eyewitness identification: A System Handbook* (Wells, 1988). This text, published in Canada, was part of a law enforcement series directed at police officers, and it contains nine chapters with a series of 131 specific procedural recommendations along with the research evidence and rationale for each.

Each of these attempts to set forth guidelines or recommendations was well intentioned. Often, they were started out of genuine concern for the possibility or even the actuality in some cases of misidentification. Unfortunately, they were all plagued with serious problems that limited their usefulness. Buckhout's checklists were empirically weak, and the use of a numerical "score" gave it the appearance of a reliable and valid test of a photospread or lineup that was unwarranted. The early attempts by individual police agencies or legal bodies were generally incomplete, failing to address some of the more serious and subtle matters involved in identification. They were often contradictory across jurisdictions. Most of the time, they did not have the force of law and could not be cited or used in that way, so that violations of the guidelines had no consequences beyond their potential source as a line of cross-examination of an investigating officer. Even more serious than this was the fact that often recommendations were made that either were or are inconsistent with what we know now based on empirical research findings, particularly since most of the recommendation sets were issued prior to the publication of the best research on the factors that affect eyewitness accuracy done in the 1980s and 1990s. (As an example, the Canadian guidelines, in Rule 801, state that a police officer may arrange a "confrontation" or showup between a suspect and a witness if "the witness was unable to identify the suspect in a lineup, photographic display, or informal viewing.") Finally, the recommendation sets were either too vague or sometimes too specific or detailed to yield any practical effects on police practice. It is our intent here to set forth a small and practical set of important recommendations along with a detailed rationale for each. We believe that by doing this, we will maximize the chances that these recommendations will be adopted and thus lead to real change.

Bases of Recommendations

The current recommendations are based on psychological theory about human memory and social influence, scientific findings in eyewitness experiments, and the

scientific logic of testing. Theories of memory, findings from eyewitness experiments, and scientific logic are far too extensive to review here in any general sense. Memory theory, for example, involves one of the largest literatures in all of psychology, eyewitness experiments number in the hundreds, and the scientific logic of testing is something to which entire graduate-level courses are directed. Accordingly, we restrict our review to the issue at hand, namely, the ways in which theory, data, and logic bear on the question of lineup identification procedures.

For example, we rely heavily on relative-judgment theory, which describes a process by which eyewitnesses make lineup identifications. It is this theory that helps us to understand, among other things, why eyewitnesses should be instructed in particular ways. As for experimental data, we rely almost exclusively on data in which one procedure for obtaining identifications is compared to another procedure. In other words, we focus on experiments that manipulated system variables. It is this focus that permits us to make conclusions about the consequences of selecting lineup distractors using one method versus some other method, for instance. As for the scientific logic of testing, we rely primarily on the logic of experimental methods as articulated in the analogy between conducting a lineup and conducting an experiment (Wells & Luus, 1990a). It is this analogy that leads us to recommend double-blind procedures for conducting lineups, for example.

We believe that the use of all three bases (i.e., relevant theory, experimental data, and scientific logic) for our recommendations serves as the foundation for consensus in the scientific community. Hence, we begin with a review of the pertinent literature on which our recommendations are based. In the following sections, we describe the process of relative judgments and the empirical evidence for the operation of this process, the lineups-as-experiments analogy and the logic of its application to lineup procedures, and empirical evidence relating to the issue of eyewitness confidence.

Following this review of research and theory on false eyewitness identification, we propose four rules or guidelines that flow naturally from the scientific literature. The four rules concern who should conduct the lineup, how distractors should be selected, how eyewitnesses should be instructed prior to viewing the lineup, and how and when confidence should be assesses.

RESEARCH AND THEORY ON FALSE IDENTIFICATION

Relative Judgment Processes

There is good empirical evidence to indicate that eyewitnesses tend to identify the person from the lineup who, in the opinion of the eyewitness, looks most like the culprit relative to the other members of the lineup (Wells, 1984). This simple observation might seem at first glance to be both obvious and benign. Although it might be obvious, it is far from benign. The problem becomes readily apparent when we consider how such a process works when the actual culprit is not present in the lineup. Under such conditions, the relative judgment process will nevertheless yield a positive identification because there will always be someone who looks more

like the culprit than do the remaining lineup members. The problem with the relative judgment process, therefore, is that it includes no mechanism for deciding that the culprit is none of the people in the lineup.

Relative judgments can be contrasted with absolute judgments in which the eyewitness compares each lineup member to his or her memory of the culprit and uses some type of criterion threshold to decide whether or not the person is the actual culprit (see related treatments by Cutler & Penrod, 1988; Dunning & Stern, 1994; Gonzalez, Ellsworth, & Pembroke, 1994; Lindsay, Lea, & Fulford, 1991; Lindsay and Wells, 1985; Sporer, 1993; Wells, 1984b, 1993). There are numerous empirical observations that lead to the conclusion that the relative judgment process exerts a significant influence in eyewitness identifications. These include the behavior of eyewitnesses under the removal-without-replacement procedure, the effects of warnings that the actual culprit might not be in the lineup, the effects of manipulations to relative similarity, patterns of eyewitness responses using the dual lineup procedure, and the performance of eyewitnesses using the sequential presentation procedure. Each of these is reviewed in turn.

Removal without Replacement

Perhaps the best evidence for the operation of relative judgment processes comes from a simple experiment using the "removal without replacement procedure." In this procedure, some eyewitnesses to a staged crime are shown a lineup in which the actual culprit is present and the rate at which he is identified is recorded. Other eyewitnesses are shown the exact same lineup except that the culprit's photo is removed and not replaced with any other photo. If identifications of the culprit in the culprit-present lineup are a product of true recognition, then the percentage of eyewitnesses who indicate "none of the above" in the culprit-absent lineup should be equal to the percentage who say "none of the above" in the culprit-present lineup *plus* the percentage who identified the culprit. In a test of this idea, 200 eyewitnesses to a staged crime were shown either a culprit-present lineup or a lineup in which the culprit was removed without replacement. All eyewitnesses were told that the actual culprit might or might not be present. Table 2, taken from Wells (1993), shows that most of the 54% who identified the culprit in a culprit-present lineup would simply have identified someone else if the culprit had not

Table 2. Rate of Choosing Lineup Members when a Target Is Present versus Removed-without-Replacement condition

	Percent of identification of lineup members 1–6						
	1	2	3	4	5	6	No choice
Target (lineup member 3) present	3%	13%	54%	3%	3%	3%	21%
Target (lineup member 3) removed without replacement	6%	38%	—	12%	7%	5%	32%

Source: Wells (1993).

been present. This is the essence of what is meant by the relative judgment process; eyewitnesses tend to select whomever looks most like the perpetrator regardless of whether the actual perpetrator is in the lineup.

Warnings That the Culprit Might Not Be Present

If eyewitnesses are prone to making relative judgments, then instructions that warn them explicitly that the culprit might not be present in the lineup should help them to recognize the fact that they should not rely solely on a relative-judgment process. Malpass and Devine (1981) were the first to demonstrate empirically the importance of the "might or might not be present" instruction. Following a staged crime, eyewitnesses were either led to believe that the culprit was in the lineup and were not given a one-of-the-above option on their identification form or were told that the culprit might not be in the lineup and had an explicit none-of-the-above option on their identification form. Failure to warn the eyewitness that the culprit might not be in the lineup resulted in 78% of the eyewitnesses attempting an identification from the culprit-absent lineup. This false identification rate dropped to 33% when the eyewitnesses were explicitly warned that the culprit might not be in the lineup. Importantly, warning the eyewitnesses that the culprit might not be in the lineup still resulted in 87% of the eyewitnesses making accurate identifications when the culprit was in the lineup, indicating that this instruction does not merely reduce eyewitnesses' willingness to identify someone. Results of this type reveal that eyewitnesses will simply select the person in the lineup whom they perceive is *relatively* more similar to the culprit than are the other lineup members if they approach the lineup with the presumption that the culprit is among the set. A recent meta-analysis of instruction effects shows that the "might or might not be present" instruction has the effect of reducing identifications when the perpetrator is absent from the lineup while having no effect on identifying the perpetrator when the perpetrator is in the lineup (Steblay, 1997). The instruction seems to lead eyewitnesses to use the relative judgment process somewhat less than they would otherwise, but even with such instructions there is a tendency for eyewitnesses to make relative judgments.

Relative Similarity and Rates of Choosing

To the extent that the decision to identify someone from a lineup is governed by a relative judgment process, it might be expected that the resemblance of the lineup members to the culprit would affect the identification choices and the confidence that the eyewitnesses have in their identifications. This seems to be precisely what happens. Wells, Rydell, and Seelau (1993) manipulated the extent to which the members of a lineup fit the general description of the culprit in a staged-crime experiment. Instructions emphasized that the perpetrator might or might not be present. We focus here on the conditions in which the eyewitness viewed a culprit-absent lineup. Whether all members of the lineup fit the general description of the culprit or only one person fit the description had no effect on overall rates of se-

lecting someone, as the relative-judgment process would predict. However, the rate of selecting an innocent person who fit the description of the culprit increased dramatically when others in the lineup did not fit the description, a result that had been demonstrated earlier by Lindsay and Wells (1980). In addition, the confidence with which the eyewitnesses identified the innocent person depended on the extent to which others in the lineup fit the description. When the innocent suspect was the only person who fit the description, the confidence of the eyewitnesses in their identification was greater than when others in the lineup also fit the description. Hence, relative judgments affect not only who is identified, but also the confidence with which the identification is made.

Dual Lineups

A fourth type of evidence about the operation of relative judgments comes from the dual lineup procedure. If some eyewitnesses are especially prone to making relative judgments then it should be possible to screen out those eyewitnesses who are merely making relative judgments with the use of a *blank* lineup. A blank lineup is conceptually distinct from a *culprit-absent* lineup. A culprit-absent lineup includes a suspect, but the suspect is not the actual culprit. A blank lineup, on the other hand, does not include a suspect at all; every member of a blank lineup is known to be innocent of the offense in question. When using a blank lineup procedure, the eyewitness is not told that there is no suspect in the lineup, but instead is given standard instructions emphasizing that the culprit might not be present. A blank lineup can therefore be considered a type of control lineup (or a "lure") to see if the eyewitness is willing or able to resist the temptation to select someone when the actual culprit is not in the lineup.

Experimental work provides support for the idea that a blank lineup can weed out eyewitnesses who are prone to make mistakes. Following a staged crime, eyewitnesses who were shown a blank lineup and rejected it were far less likely to make a false identification on the subsequent (actual) lineup than were those who failed the blank lineup test or those who were not given the blank lineup test at all (Wells, 1984). This dual lineup procedure had little effect on the frequency of accurate identifications; its effect was almost totally restricted to reducing false identifications. The dual lineup procedure provides evidence that eyewitnesses are prone to make relative judgments even under conditions where they are told that the actual culprit might not be in the lineup. In a memory experiment, researchers often use blank trials to control for or estimate response biases. Even when there is only one eyewitness and one suspect, it is possible to use certain types of control lineups that serve the same functions in actual cases that control conditions serve in experiments.

Sequential Procedures

A fifth line of empirical evidence indicating that false identifications are partly the result of the relative judgment process comes from the comparison of sequential identification procedures with simultaneous lineup procedures. Lindsay and Wells

(1985) reasoned that the standard identification procedure, in which the eyewitness examines the full set of lineup members at once, allows for relative judgment processes in ways that a sequential procedure would not. A sequential procedure is one in which the eyewitness views one lineup member at a time, deciding whether or not that person is the culprit before seeing the remaining lineup members. Having not yet seen the remaining lineup members, the eyewitness is not in a position to make a relative judgment. Although the eyewitness could compare the person being viewed to those viewed previously, the eyewitness cannot be sure that the next person to be viewed will not be an even better likeness to the culprit. Hence, the eyewitness must rely more on an absolute judgment process.

The evidence in support of the sequential procedure for preventing relative judgments is rather impressive. Data from several independent experiments show that sensitivity to the presence versus absence of the culprit in the lineup is far greater with the sequential procedure than it is with the simultaneous procedure (Cutler & Penrod, 1988; Lindsay et al., 1991a; Lindsay, Lea, Nosworthy, Fulford, Hector, LeVan, & Seabrook, 1991; Lindsay & Wells, 1985; Sporer, 1993). In addition, Dunning and Stern (1994) collected data on eyewitnesses' verbal descriptions of the process that they used to make an identification decision. Eyewitnesses who described their decision process as one of elimination (relative comparisons of the photos to each other to narrow the choices) were more likely to have made a false identification than were those who reported that the face "just popped out at me." Lindsay et al. (1991b) reported similar results when they asked staged crime eyewitnesses to report on the process that they used in identifying a suspect from a lineup. Those who reported using a relative judgment process were more likely to have made a false identification than were those who reported using an absolute judgment process.

A wealth of anecdotal evidence could be brought to bear implicating the relative judgment process. This can be observed, for example, when witnesses say things such as "I know it can't be numbers 1, 2, 4, 5, or 6, so it must be number 3." It is not the anecdotal evidence or people's self-reports of their cognitive processes, however, that lead us to conclude that relative judgment processes are operating. Instead, it is the pattern of behavioral data surfacing in experiments using the removal-without-replacement procedure, instructional variations, manipulations to relative similarity, and the simultaneous versus sequential procedure that lead us to conclude that the identification process is influenced by relative judgment processes.

This theoretical understanding of the identification process assists us greatly in making procedural recommendations. We refer back to the relative judgment process when we discuss our specific recommendations for lineup and photospread procedures.

Lineups-as-Experiments Analogy

In addition to our understanding of the relative judgment process, our recommendations are bolstered by some fundamental principles in the logic of science. Particularly useful is the analogy between good methods for conducting scientific

experiments (e.g., Rosenthal & Rosnow, 1984) and good methods for conducting lineups (Wells, 1988). The analogy is a strong one and has been formally articulated in the psychological literature (Wells & Luus, 1990a).

The analogy can be stated as follows: A lineup is like an experiment: the police have a hypothesis (that the suspect is the culprit); they collect materials that could be used to test the hypothesis (e.g., picture of the suspect and filler pictures), they create a design (e.g., placing suspect's picture in a particular position in an array), instruct the subject(s) (eyewitness or eyewitnesses); run the procedure (show the lineup to the eyewitness), record the data (identification of suspect or not); and interpret the hypothesis in light of the data (decide whether the identification decision changes their assessment of whether the suspect is the culprit). The analogy is useful because it allows us to borrow from the well-developed methods in scientific experiments and apply these to lineups.

We recognize that there are ways in which police conducting a lineup cannot be likened to a scientific experiment. For instance, experiments make use of the law of large numbers and can run as many subjects as are necessary to help rule out chance interpretations of the results. In real criminal cases, there may be only one or a few eyewitnesses to the crime and police cannot simply choose to test more eyewitnesses. Nevertheless, there are many basic rules of science that help control for various alternative interpretations of results or "confounds" as they are commonly called. We argue in general that lineup procedures should be as controlled as experimental procedures to whatever extent possible.

Recognition of this analogy between a lineup and an experiment helps to clarify the fact that there is much that can go wrong with a lineup procedure just as there is much that can go wrong with a psychology experiment. Factors that can create interpretation difficulties for an experiment can create similar problems for a lineup. These problems include the presence of demand characteristics (e.g., pressuring the eyewitness to make a choice), the influence of confirmation biases (e.g., asking the eyewitness specifically about the suspect while not asking those same questions about the distractors), the facilitation of response biases (e.g., encouraging a loose recognition criterion threshold in the eyewitness), making inferences from small sample sizes (e.g., making strong judgments of validity based on only one eyewitness), not using control groups (e.g., failing to see if even people who did *not* witness the crime can identify the suspect), selective recording and interpretation of data (e.g., finding significance in an identification of the suspect but ignoring the outcome if the eyewitness makes a nonidentification), leaking of the hypothesis (e.g., making it obvious to the eyewitness which person in the lineup is the suspect), and a host of other possible confounds.

Some forms of forensic evidence, such as fingerprints, DNA, and firearms patterns, are subject to criticism for not following scientific principles in the collection and analysis of the evidence. We see no reason why eyewitness identification evidence should not be treated in a similar fashion. In fact, the analogy between eyewitness evidence and physical trace evidence is itself useful (Wells, 1995). Eyewitness evidence can be construed as a type of trace evidence except that, unlike blood or fingerprints, the trace is in the brain of a human observer in the form of a memory. This memory trace even has some physical properties in the sense of

being located as a neurological trace in the brain. Like physical evidence, the critical issue is how to extract the evidence in way that is maximally diagnostic of identity.

The lineups-as-experiments analogy represents an important conceptual framework in support of our recommendations. Accordingly, we refer back to the analogy when we discuss our specific recommendations regarding lineup procedures.

EYEWITNESSES' CONFIDENCE IN THEIR IDENTIFICATIONS

One of the most researched questions in all of the scientific eyewitness literature concerns the strength of the relation between the accuracy of an eyewitness's identification and the confidence that the eyewitness expresses in the identification. In the late 1970s, eyewitness identification researchers began to recognize the importance of eyewitness confidence. In particular, the argument emerged that false identifications per se would not be so problematic if the eyewitnesses were not so confident (Wells et al., 1979). Accordingly, a great deal of research has been directed at the question of the statistical relation between the accuracy of an eyewitness's identification and the confidence expressed by the eyewitness.

This focus on eyewitness identification confidence is more than just a theoretical interest. The U.S. Supreme Court explicitly listed eyewitness certainty as one of the five factors that should be considered in making judgments about the accuracy of an eyewitness identification *(Neil v. Biggers*, 1972, pp. 201–202): "As indicated by our cases, the factors to be considered in evaluating the likelihood of misidentification include the opportunity of the witness to view the criminal at the time of the crime, the witness' degree of attention, the accuracy of the witness' prior description of the criminal, the level of certainty demonstrated by the witness at the confrontation, and the length of time between the crime and the confrontation." Supreme Court justices are not the only ones impressed with the potential diagnostic value of witness confidence. Researchers have examined attorney and layperson beliefs about the confidence–accuracy relation using several methods including surveys, "postdiction" studies in which participants second guess the results of eyewitness identification experiments, and studies in which mock jurors are asked to assess the accuracy of eyewitness identifications.

Surveys Concerning Confidence and Accuracy

Based on survey techniques, it is clear that people believe that there is a strong relation between eyewitness identification confidence and eyewitness identification accuracy. Brigham and Wolfskiel (1983) surveyed 89 public defenders, 69 state prosecutors, and 77 private defense attorneys in Florida: 75% of prosecutors, but 40% of defense attorneys believed that witnesses who are more confident are more likely to be accurate. Rahaim and Brodsky (1982) conducted a similar survey of 42 practicing lawyers. Respondents were asked whether identifications by very confi-

dent eyewitnesses are most likely to be correct: 64% indicated that they believed this proposition. Similarly, surveys of the lay public in the United States (Brigham & Bothwell, 1983; Deffenbacher & Loftus, 1982), Canada (Yarmey & Jones, 1983), Germany (Sporer, 1983), Australia (McConkey & Roche, 1989) and England (Noon & Hollin, 1987) indicate there is a substantial cross-cultural belief that confidence predicts accuracy.

Postdicting the Confidence–Accuracy Relation

In a study by Wells (1984a), subjects read the procedure section of a study by Leippe, Wells, and Ostrom (1978) and were asked whether two witnesses had made correct identifications. In one instance, the eyewitness was "completely certain" about the accuracy of the identification; in the other, the eyewitness was "somewhat uncertain." Although Leippe et al. found that witness confidence was unrelated to actual identification accuracy, the subjects in Wells' study predicted a .83 probability of a correct identification by the "completely certain" versus a .28 probability for the "somewhat uncertain" witnesses. In short, subjects in Wells' study believed that confidence was very strongly related to accuracy.

Confidence and Accuracy in Mock-Jury Studies

There is consistent evidence to indicate that the confidence that an eyewitness expresses in his or her identification during testimony is the most powerful single determinant of whether or not observers of that testimony will believe that the eyewitness made an accurate identification (e.g., Cutler, Penrod & Dexter, 1990; Leippe & Romanczyk, 1987, 1989; Leippe, Manion, & Romanczyk, 1991; Lindsay, Wells, and O'Connor, 1989; Lindsay, Wells, & Rumpel, 1981; Turtle & Wells, 1988; Wells, Ferguson, & Lindsay, 1981; Wells et al., 1979; Wells & Murray, 1984). For example, Wells et al. (1979) created simulated cross-examinations of eyewitnesses who viewed a staged crime and attempted identifications. Mock jurors viewed these cross-examinations and made judgments about the eyewitnesses. Nearly 80% of the accurate eyewitnesses but only 20% of the inaccurate eyewitnesses were correctly classified as accurate or inaccurate by mock jurors (i.e., about four out of five mistaken identifications were believed). Why were the mock jurors making so many mistakes? The answer is that they were relying on witness confidence when making their accuracy judgments—but they should not have done this. Jurors' beliefs about witness accuracy correlated significantly ($r = .53$) with witness confidence, but non-significantly ($r = .05$) with the actual accuracy of the witness's decision. In other words, jurors were much more likely to believe confident eyewitnesses even though those witnesses were no more likely to be accurate than the less confident eyewitnesses.

A related study by Lindsay et al. (1989) tested whether cross-examination of eyewitnesses conducted by attorneys might aid jurors in differentiating between accurate and inaccurate witnesses. Unfortunately, mock jurors' beliefs in eyewitness accuracy were, once again, unrelated to witness accuracy. Witness confidence—as

gauged by mock jurors—was significantly correlated with verdicts (r = .29) although witness self-rated confidence was not (r = –.07).

Lindsay et al. (1981) staged three versions of a theft designed to produce low, moderate, and high levels of identification accuracy; of the eyewitnesses who made positive identifications, the accuracy rates were 33%, 50%, and 74%, respectively. Videotaped cross-examinations of eyewitnesses (drawn from each viewing condition) were then evaluated by mock jurors. Overall, 77% of confident witnesses were believed versus 59% of low-confidence witnesses. Jurors overbelieved the witnesses—62%, 66%, and 77% of the witnesses in the low, moderate-, and high-accuracy conditions were believed. Lindsay et al. found only a weak relation between witness confidence and witness accuracy (r = .26).

Overall, jurors also tended to ignore witnessing conditions when the witness was very confident, but gave the witnessing conditions some consideration when the witness was not highly confident. This result is worrisome because prosecutors may be hesitant to proceed with cases based on less confident witnesses and eliminate them in favor of more confident ones. As a result, jurors may be exposed only to highly confident witnesses and may reflexively rely on the high levels of witness confidence rather than critically considering the conditions under which identifications are made.

In short, it is clear that jurors do rely on witness confidence as an indicator of witness accuracy, even when, as in the studies just examined, circumstances do not support such reliance. These observations highlight the considerable importance that has been attached to the question of the extent to which eyewitness identification confidence is predictive of eyewitness identification accuracy. Although it might be tempting to conclude that the weak and null correlations between witness confidence and accuracy found in these studies is unrepresentative of the general pattern of results, that is not the case.

To What Extent Does Witness Confidence Predict Identification Accuracy? Should jurors (and attorneys and judges) rely so heavily on witness confidence as a guide to witness accuracy? Witness confidence can be assessed at various points: during crime-scene interviews, after identification attempts, in depositions and pretrial proceedings, and in court examinations. A substantial body of research has examined the association between the witness's confidence and the accuracy of the identification. A common statistical metric for the eyewitness identification confidence/accuracy literature has been to express the relation as a point-biserial correlation (with accuracy as a dichotomous variable and confidence as a continuous variable). Over the years, many studies have reported correlations that are not reliably different from zero, but the direction of the correlation has usually been positive. This is an ideal situation for a meta-analysis and several relevant meta-analyses have been conducted.

Preidentification Confidence and Accuracy

Cutler and Penrod (1989a) examined nine studies testing the relation between preidentification confidence and identification accuracy. For example, in several

studies (Cutler, Penrod, & Martens, 1987; Cutler, Penrod, O'Rourke, & Martens, 1986), witnesses viewed a videotaped robbery and later attempted identifications from lineups. After viewing the crime, but before attempting an identification, witnesses indicated how confident they were that they could (1) correctly identify the robber if the robber was in the lineup and (2) avoid making a false identification if the robber was not in the lineup. Across nine studies the preidentification confidence–accuracy correlation ranged from .00 to .20, which indicates that preidentification is a poor predictor of identification performance. These findings suggest that witnesses should probably be asked to attempt identifications irrespective of their confidence insofar as any resulting identifications might yield other evidence that would confirm any identifications made by low-confidence witnesses.

Postidentification Confidence and Accuracy

Over the past 15 years researchers have examined the results from the growing numbers of studies that measure both witness accuracy and witness postidentification confidence in an effort to arrive at a reliable estimate of the magnitude of their relation. Deffenbacher (1980) reviewed a set of studies conducted since the turn of the century and concluded that there was little support for a strong reliance on witness confidence as a guide to witness accuracy. Penrod (reported in Penrod, Loftus, & Winkler, 1982) examined 16 eyewitness studies in which confidence and accuracy relations were reported and found an average correlation (weighted for degrees of freedom) of $r = .23$. In a review of 31 studies Wells and Murray (1984) reported an average $r = .07$.

In the most exhaustive review to date, Bothwell, Deffenbacher, and Brigham (1987) meta-analyzed 35 studies involving staged incidents that yielded a statistically significant average postidentification confidence and accuracy correlation of $r = .25$ (with a 95% confidence interval of .08–.42). This finding suggests that witnesses who are highly confident in their identifications are somewhat more likely to be correct as compared to witnesses who display little confidence.

Factors Affecting Confidence, Accuracy, and Their Relation

There is evidence that the strength of the confidence–accuracy correlation depends on a variety of cognitive and social factors such as timing of the confidence judgment (Cutler & Penrod, 1989a; Sporer, 1992, 1993), the eyewitness's level of self-awareness (Kassin, 1985; Kassin, Rigby, & Castillo, 1991), the optimality of information processing conditions at encoding (Bothwell, Brigham, & Pigott, 1987; Cutler & Penrod, 1989a; Deffenbacher, 1980), the distinctiveness of the to-be-recognized persons (Brigham, 1990; Cutler & Penrod, 1989b); the match between a target's appearance at encoding and at retrieval (Read, Vokey, & Hammersley, 1990), and whether one considers "choosers" (witnesses who make positive identifications from lineups) versus "nonchoosers" (witnesses who reject the lineup).

The "chooser" versus "nonchooser" distinction is a forensically important one, because it is "choosers" (and the defendants they choose) who typically appear in

courtrooms. Nonchoosers are less likely than choosers to have an impact on the legal system because they are viewed as unreliable (particularly if they "missed" the suspect) and because nonidentifications do not support criminal prosecutions (Malpass & Devine, 1981; Wells & Murray, 1984).

Choosers versus Nonchoosers

Several studies have indicated that confidence-accuracy correlations for choosers may differ from those for nonchoosers. Fleet, Brigham, and Bothwell (1987) reported a significant relation between postdecision confidence and accuracy (.30), but this relation was substantially higher among choosers (.50) than among nonchoosers (.14). Sporer (1992, 1993) similarly found stronger relations among choosers (.58 and .59, respectively) than nonchoosers (.08 and .34, respectively). Brigham (1988) reanalyzed data from six of his experiments and found the mean correlation for choosers was $r = .37$, $N = 533$, versus $r = .07$, $N = 330$, for nonchoosers (for all participants combined, $r = .19$, $N = 863$).

More recently, Sporer, Penrod, Read, and Cutler (1995) analyzed 30 studies from their laboratories ($N = 4{,}036$ participants) that used staged crime scenarios. The overall confidence–accuracy correlation in these studies ($r = .29$) corresponds to that reported in previous reviews. The correlation was significantly higher for choosers ($r = .41$, $N = 2{,}467$) than for nonchoosers ($r = .12$, $N = 1{,}569$). These findings indicate that, when limited to witnesses who make positive identifications, confidence appears to be a modest predictor of accuracy, whereas among witnesses who reject lineups, confidence appears to be very weakly related to accuracy.

How does an $r = .41$ compare to other effects discussed here? Cutler and Penrod (1995) examined six studies with target-absent arrays and found that biased instructions produced a 78% identification rate versus a 39% identification rate under unbiased instructions. The difference, based on a total N of 417 per condition corresponds to an $r = .38$.

Unfortunately, it is not clear that the identification conditions that give rise to confidence-accuracy correlations such as those reported by Sporer et al. (1995) are likely to prevail in the real world. A major reason for such a concern is evidence that, under less than pristine conditions, witness confidence is highly malleable and may be "pushed around" in ways that weaken or destroy even the modest confidence-accuracy relation reported by Sporer et al. for choosers. We discuss this in greater detail in the later section called "confidence malleability."

Juror Reliance on Witness Confidence versus Other Factors

Some studies have focused on determining whether jurors are sensitive to factors—in addition to witness confidence—that are known to influence eyewitness accuracy. If jurors are sensitive to factors other than confidence that can affect witness performance, then their reliance on confidence might be less worrisome. In one study that addressed this issue, Cutler et al. (1990) presented a videotaped

trial simulation in which an eyewitness identification played a central role to samples of undergraduates and eligible and experienced jurors.

Ten variables related to the identification procedures and witnessing conditions were manipulated in the videotaped trial. One manipulation was that the eyewitness testified that she was either (a) 100% or (b) 80% confident that she had correctly identified the robber. This manipulation produced the only statistically significant effect on juror judgments. These results indicate that jurors are insensitive to some of the more important factors that influence identification accuracy. Testimony about disguise, weapon focus, violence, retention interval, instruction bias (the extent to which instructions encouraged witnesses to make a positive identification), and foil bias (the extent to which lineup members resembled the suspect) had minimal effects on mock-jurors' evaluations of identification evidence. In short, confidence was by far the most important consideration to jurors.

Taken together, the survey, postdiction and mock-juror experiments, and the confidence–accuracy studies converge on a worrisome set of conclusions: Jurors appear to overestimate the accuracy of identifications, fail to differentiate accurate from inaccurate eyewitnesses—because they rely so heavily on witness confidence, which is relatively nondiagnostic—and are generally insensitive to other factors that influence identification accuracy. Furthermore, this picture is even gloomier when one considers that eyewitness confidence proves to be highly malleable.

Confidence Malleability

Confidence malleability refers to the tendency for an eyewitness to become more (or less) confident in his or her identification as a function of events that occur after the identification. The confidence malleability problem is particularly important because actors in the legal system can contaminate the confidence of an eyewitness in ways that can make an eyewitness's in-court expression of confidence a meaningless indicator of the eyewitness's memory. An eyewitness who expresses high confidence in an identification is expressing a strong belief that the identified person and the culprit are the same person. Clearly, an eyewitness's belief that the identified person is the culprit can arise out of pure memory judgments, i.e., a perception of remarkable resemblance between the identified person and the eyewitness's memory of the culprit (Leippe, 1980; Wells et al., 1981).

Unfortunately, an eyewitness's belief that the identified person is the culprit can also arise for reasons other than the eyewitness's memory (Leippe, 1980; Wells et al., 1981; Luus & Wells, 1994; Wells & Bradfield, 1998). For example Hastie, Landsman, and Loftus (1978), in an early demonstration of confidence malleability, found that witnesses who were questioned repeatedly grew more confident about the accuracy of details in their reports (see also Shaw, 1996; Shaw & McClure, 1996; Turtle & Yuille, 1994).

Wells et al. (1981) demonstrated they could increase witness confidence simply by briefing witnesses about the types of questions they might encounter in an upcoming cross-examination. When cross-examined, the briefed witnesses (who were no more accurate than the unbriefed witnesses) were significantly more confident

about their identifications than were unbriefed witnesses and were believed more often by the jurors. Unfortunately, the briefing effect occurred among inaccurate eyewitnesses, whose levels of confidence rose dramatically, whereas confidence levels among accurate witnesses were unchanged. Perceived confidence was highly correlated with juror belief of witnesses (r = .58). The elevated levels of confidence among incorrect witnesses resulted in more incorrect than correct witnesses being believed by jurors (p = .08). Because jurors (and judges and attorneys) rely so heavily on witness confidence, one would expect that the principal effect of briefings would be to increase conviction rates for defendants identified by briefed, but inaccurate witnesses. A secondary effect would be to reduce the (already limited) correlation between witness confidence and witness accuracy—because the briefing introduces nondiagnostic noise into confidence judgments.

In a dramatic illustration of confidence malleability that is especially relevant to our recommendations concerning lineup practices, Luus and Wells (1994) used a staged crime to secure false identifications from 136 eyewitnesses. These eyewitnesses viewed a theft in pairs and were separated shortly after the theft. After being separated false identifications were obtained from the witnesses using a photospread (the eyewitnesses were unaware they had made a false identification). After making their identifications eyewitnesses were randomly assigned to one of several experimental conditions. In the control condition, eyewitnesses were told nothing about the identification decision of their co-witness. In various experimental conditions, eyewitnesses were given information that their co-witness ostensibly identified the same person, or that their co-witness identified someone else, or that their co-witness had indicated that the culprit was not in the lineup. They were then interviewed by an assistant to the experimenter (posing as a campus police officer) who solicited the witness' confidence levels (on 10-point scales) in the accuracy of their identifications. Each eyewitness was videotaped while giving statements to the police officer.

The results indicated dramatic increases in the confidence that eyewitnesses expressed in their false identifications in the condition in which they were told that their co-witness identified the same person (the average confidence on a 10-point scale was 8.8 vs. 6.9 in the no-information control condition). Confidence levels were quite high even among witnesses who were told that the co-witness had first identified the same individual, but then withdrew the identification (8.5) or switched to another individual (8.3). Witnesses who were given feedback indicating that the confederate had identified an implausible alternative from the photospread were significantly more confident (7.9) than the control witnesses who received no feedback. Witnesses who were told that the co-witness had identified a different person, but withdrew the identification were somewhat (though not significantly) less confident (6.1) than witnesses who received no feedback.

The lowest confidence levels were found among witnesses who were told that the co-witness identified someone else (4.7), witnesses who were initially told that the co-witness identified someone else, but switched to the same person identified by the witness (4.7), and witnesses who were told that the co-witness had indicated that the perpetrator was not in the array (3.6). When Luus and Wells had mock jurors evaluate the witness statements the pattern of ratings closely paralleled the

pattern of witness confidence levels (a result that is consistent with the research on juror reliance on witness confidence reviewed earlier). The manipulations had similar effects on juror ratings of the witnesses' quality of view, believability, and detail of description.

Even stronger and broader effects have been shown to emerge when eyewitnesses are told after their identification that they identified the suspect (versus being told nothing). Wells and Bradfield (1998) obtained 352 false identifications in an experiment and randomly assigned these eyewitnesses to receive feedback about their identification decisions. Some received confirming feedback ("Good, you identified the suspect"), some received disconfirming feedback ("Actually the suspect is number 4"), and some received no feedback. When later asked how certain they were at the time of the identification that they had identified the actual culprit, the eyewitnesses who received confirming feedback were much more confident than the witnesses with no and witnesses with disconfirming feedback. In addition, the confirming feedback witnesses distorted their reports of their witnessing conditions by exaggerating how good their view was of the culprit, how much attention they paid to the culprit's face while observing the event, and so on.

The facts that eyewitness identification confidence is given great weight by jurors, that confidence is only modestly related to accuracy under pristine conditions, and that confidence is malleable are all matters of considerable importance. In particular, these findings speak to the question of whether or not the confidence that an eyewitness expresses in his or her identification is a reflection of the "goodness" of the eyewitness's memory. What does it mean when an eyewitness says, "I am highly confident that the person I identified is the person who committed the offense"? One interpretation is that the eyewitness is saying "My memory of the culprit so closely resembles this person that I conclude that this person is in fact the culprit." However, the studies of the confidence–accuracy relation and the studies of confidence malleability show that high confidence does not necessarily denote high accuracy and that high levels of confidence can come from external sources, such as giving a witness feedback about their choices or information about the behavior of other eyewitnesses.

Confidence as a System Variable

System variables were defined 20 years ago as "variables that are (or potentially can be) under the direct control of the criminal justice system" (Wells, 1978, p. 1548). As the discussion of confidence malleability makes clear, eyewitness identification confidence can be directly manipulated by those who conduct the lineup via the timing and content of statements that they provide to the eyewitness. Externally provided information can strongly inflate an eyewitness's confidence in a false identification, which means that eyewitness confidence is at least partly controllable by the criminal justice system. Hence, eyewitness identification confidence has system-variable properties even though confidence has been considered traditionally to be merely an estimator variable. The fact that eyewitness identification

confidence has system-variable properties has implications for our recommended rules regarding the procedures for conducting lineups.

FOUR RECOMMENDED RULES

The evidence reviewed in the previous sections makes a strong case that some lineup identification procedures lead to increased risk of false identification or inflated confidence. These procedures are under the control of the criminal justice system. Hence, any role of these procedures in contributing to false identification or false confidence could be eliminated by controlling the procedures in critical ways. In the present section, we describe four simple rules of procedure that follow from the scientific literature that we argue could largely relieve the criminal justice system of its role in contributing to eyewitness identification problems.

Rule 1. Who Conducts the Lineup

The person who conducts the lineup or photospread should not be aware of which member of the lineup or photospread is the suspect. This rule follows closely from the lineup-as-experiment analogy. All of the reasons for using double-blind procedures in behavioral experiments (Harris & Rosenthal, 1985; Rosenthal, 1976) apply equally well to conducting lineups and photospreads.

Common practice at this time is for the detective involved closely in the case, who knows which lineup member is the suspect, to administer the lineup. This person contacts the eyewitness, tells the eyewitness about the impending lineup or photospread, instructs the eyewitness, maintains a physical presence with the eyewitness during the interview, answers questions that the eyewitness might have, asks the eyewitness to indicate a choice, records answers, and so on. This interaction between the lineup administrator and the eyewitness is a highly interpersonal process. Research on experimenter-expectancy effects shows how powerful such interpersonal processes can be, especially when close physical distance between the interactants allows for eye contact, visible facial expressions, and verbal exchanges (Harris & Rosenthal, 1985). The absence of video recordings in these interactions makes it difficult or impossible to know what role might have been played by the lineup administrator in leading the eyewitness to select a particular lineup member.

We need not assume that a lineup administrator's influence is conscious or deliberate in order to see the value of a double-blind recommendation. It is well established that people have natural propensities to test a hypothesis in ways that tend to bias the evidence toward confirming the hypothesis (e.g., Dawes, 1975; Fischhoff and Beyth-Marom, 1983; Klayman and Ha, 1987; Snyder, 1984; Snyder and Cantor, 1979; Wason and Johnson-Laird, 1972). The confirmation bias in human reasoning and behavior is the seed that gives birth to the self-fulfilling prophecy phenomenon in which a person's assumption that a phenomenon will happen leads to behaviors that tend to make the phenomenon happen (Plous, 1993). The

simple use of procedures in which the person collecting the evidence is unaware of the "correct" answer is an effective prevention for this powerful phenomenon.

We are aware of no studies indicating that lineup and photospread administrators are affecting the identification behaviors of eyewitnesses in actual cases. Hence, this rule has to be taken somewhat on face value and in conjunction with a few other observations. First, we know from experiments that a photospread administrator's behaviors such as smiling and nonverbal reinforcement of a particular photograph can lead eyewitnesses to falsely identify that person as the culprit (Fanselow & Buckhout, 1976). Second, we know that police sometimes conduct lineups in a manner that clearly shows how their knowledge of which person is the suspect can lead them to say things that focus the eyewitness on the suspect (Wells & Seelau, 1995). Third, we know that what the person administering the lineup says to the eyewitness at the time the eyewitness makes a selection has strong effects on the confidence of the witness, easily leading a tentative identification eyewitness to become quite positive in the identification, even when the identification is of an innocent person (Luus & Wells, 1994; Wells & Bradfield, 1998).

In actual cases it is difficult to obtain evidence of the lineup administrator focusing the eyewitness on a particular suspect because nonpolice overseers of the process of identification often are not present at photospreads and are rarely present at live lineups (a matter discussed in the next section on legal rulings). Furthermore, lineup and photospread identification procedures are rarely recorded on video or by any other means. Nevertheless, there are individual cases in which there seems to be no other explanation than the idea that the agent administering the photospread influenced the eyewitness. In *State v. Washington* (1997), for instance, a detective secured a photo of someone he thought was James Washington, his prime suspect in a robbery. Unknown to the detective at that time, however, he had been supplied with the wrong photo and the photo was actually of someone else who was not a suspect. He placed what he thought was the photo of his suspect in position 3 of a 6-person photospread and showed it to the eyewitness. Somehow, the detective managed to obtain an identification of number 3, the very person he thought was his suspect. [Later, when he learned of the error, he secured a picture of James Washington and created a new photospread with Washington in position 2. The eyewitness then identified Washington.] How did the eyewitness manage to pick the very person that the detective thought was his suspect in the first photospread? One possibility is that the innocent person in the first photospread coincidentally resembled Washington. In fact, however, this appears not to be the case. One of the authors of this article (GLW) gave the photo of Washington to 50 people and asked them to select a person from the first photospread who most closely resembled Washington. None of the 50 picked the person the eyewitness had picked. The only plausible explanation for the eyewitness's choice of photo 3 in the first photospread seems to be that the detective facilitated the identification.

Directing the eyewitness in a way that can lead the eyewitness to select the suspect is not the only problem addressed by Rule 1. Consider the practice of some lineup administrators of telling an eyewitness immediately after an identification "Yes, that's the guy" or "Good, that's who we thought it was" or "Yes, that's the guy who has a record for offenses of this type." Later, the eyewitness is asked how

confident he or she is that the person that he or she identified is the culprit. It is perhaps not surprising that an eyewitness would be highly confident at this point even if he or she was uncertain at the time of the lineup decision. Unfortunately, if a lineup administrator mentions other evidence against the identified person before asking about the eyewitness's confidence, then it can no longer be assumed that the confidence of the eyewitness is based solely on the eyewitness's own memory. Any confidence expressed at this point is hopelessly confounded with information provided externally rather than being a reflection of the eyewitness's memory for the culprit.

Suppose, however, the lineup administrator is blind as to whether the eyewitness's selection was of a suspect or a distractor. Under these conditions, the lineup administrator could not reveal to the eyewitness any "facts" about the person selected. Hence, we can assume that a confidence question that is asked of the eyewitness by an administrator who is blind to the identity of the suspect is a purer measure of the eyewitness's memory-based confidence. An eyewitness's confidence in the identification at that point should represent the eyewitness's confidence based on his or her own memory, not external information.

Rule 2. Instructions on Viewing

Eyewitnesses should be told explicitly that the person in question might not be in the lineup or photospread and therefore should not feel that they must make an identification. They should also be told that the person administering the lineup does not know which person is the suspect in the case. The first part of this rule follows from empirical data showing that eyewitnesses are less likely to identify an innocent suspect when they are warned that the actual culprit might not be present than when they are not so warned (e.g., Malpass & Devine, 1981; Parker & Caranza, 1989, Parker, Haverfield, & Baker-Thomas, 1986; Parker & Ryan, 1993). This is related as well to our general understanding that the dangers of false identification derive from a tendency for eyewitnesses to simply identify the person who best resembles the culprit relative to the others in the lineup (e.g., Wells, 1984). In effect, this instruction serves to alert eyewitnesses about the possibility that the actual culprit is not in the lineup, encourages eyewitnesses to not merely make relative judgments, and legitimizes the behavior of not identifying anyone. Implying in any way to eyewitnesses that the culprit is in the lineup or photospread (or that their task merely is to find the culprit among the set) is tantamount to asking eyewitnesses simply to select the person who most looks like the culprit relative to the others.

Empirical data show that an explicit warning to eyewitnesses that the culprit might not be in the lineup or photospread has a selective effect. Specifically, such instructions reduce the rate of incorrect identifications in culprit-absent lineups, but these same instructions produce no appreciable reduction of accurate identifications in culprit-present lineups [see Steblay (1997) for a meta-analysis of these instruction effects]. If it were the case that lineups always included the actual culprit, we would have little need for this rule and, relatedly, we would have little concern about eyewitnesses using a relative-judgment process. This cannot be assumed, how-

ever, because it presumes the truth of the very hypothesis being tested. The purpose of the lineup or photospread is to test the hypothesis that the lineup's suspect is the culprit. If investigators already know that the suspect is the culprit, what is the need for the eyewitness? There is no way to estimate the proportion of lineups for which the actual culprit is not present, but it could be a quite significant proportion. Police do not need any real evidence against a potential suspect, let alone strong evidence, in order to place that suspect in a lineup. Warning eyewitnesses that the actual culprit might not be in the lineup or photospread is thereby essential to prevent the eyewitnesses from assuming that the police have the actual culprit and that their task merely is to find the suspect among the members of the lineup or photospread. We note as well that the actual perpetrator was absent from the lineups for all of the real cases of false eyewitness identification listed in Table 1.

The second part of Rule 2 is related to Rule 1. The person who administers the lineup should not only be blind as to which person in the lineup is the suspect, but should also be *perceived* (by the eyewitness) to be blind as to which person is the suspect. The rationale is simply to prevent eyewitnesses from looking to the lineup administrator for cues as to which person to select or for cues as to whether the person they selected is the "right person."

Rule 3. Structure of Lineup or Photospread

The suspect should not stand out in the lineup or photospread as being different from the distractors based on the eyewitness's previous description of the culprit or based on other factors that would draw extra attention to the suspect. Although seemingly simple on initial inspection, Rule 3 is actually the most complex of all the recommendations. This recommendation follows from our understanding of the relative-judgment process, from the lineup-as-experiment analogy, and from direct empirical tests of the rule. Consider first the lineup-as-experiment analogy. In a behavioral experiment, it is important that the materials, instructions, and so on not convey to the research subject what the experimenter's hypothesis is because this could lead the subject to merely respond to the hypothesis itself rather than responding to the stimuli. Suppose that a lineup somehow reveals to the eyewitness which person is the suspect. Perhaps the suspect stands out because s/he is the only one who fits the verbal description that the eyewitness had given to police earlier (Lindsay and Wells, 1980), or because the suspect is the only one dressed in the type of clothes worn by the culprit (Lindsay, Wallbridge, & Drennan, 1987), or because the suspect's photo was taken from a different angle than the other photos (Buckhout & Friere, 1975). The presence of features that make the suspect stand out from the distractors confounds our ability to conclude that the selection of the suspect was due to true recognition versus some form of suggestion, demand, or inference.

It should be apparent from Rule 3 that "show-ups" (a procedure in which the eyewitness is presented with only one person and asked if that person is the perpetrator in question) fail to meet the requirements of this rule. In spite of one suggestion in the psychological literature that show-ups might be superior to lineups

because show-ups do not prompt relative judgment processes (Gonzalez, Ellsworth, & Pembroke, 1994), there is clear evidence that show-ups are more likely to yield false identifications than are properly constructed lineups (Dekle, Beale, Elliot, & Huneycutt, 1996; Lindsay, Pozzulo, Craig, Lee, & Corber, in press; Wagenaar & Veefkind, 1992; Yarmey, Yarmey, & Yarmey, 1996). Show-ups can be considered suggestive in the sense that they convey to the eyewitness which person is the suspect, thereby standing in contrast to the lineups-as-experiments analogy in which the test itself should not communicate the hypothesis of the tester. With a lineup or photospread, an affirmative response of the eyewitness (i.e., making an identification) can sometimes be classified immediately as a known error (when the eyewitness identifies a distractor), but a show-up does not have such a provision. We acknowledge that U.S. courts have often held show-ups to be acceptable in the sense that they have refused to exclude such identifications under some circumstances. In *Stovall v. Denno* (1967), for example, the Supreme Court held that a show-up in a hospital room was suggestive, but not "unnecessarily so" because it was unclear whether the victim-witness would survive long enough to make an identification under other circumstances. More generally, even show-ups that are not necessary (e.g., no need for immediate action) are held to be acceptable to the extent that it is decided that the suggestive procedure did not create a substantial likelihood of mistaken identification (*Neil v. Biggers*, 1972; *Manson v. Braithwaite*, 1977). There is some logic to this reasoning, which emphasizes the issue as likelihood of mistaken identification rather than suggestive procedure per se. Unfortunately, there are severe problems with the criteria for deciding whether there is substantial likelihood of mistaken identification. The criteria outlined in *Biggers* and *Braithwaite* have been shown to be deficient on numerous grounds (Wells & Murray, 1983). More recently, it has been shown that three of these five criteria (eyewitnesses' reports of their certainty, attention, opportunity to view) are actually influenced by suggestive procedures. For example, biased lineups (Steblay, 1997), biased instructions (Wells et al., 1993), or suggestions to the eyewitness as to which person is the suspect (Wells & Bradfield, 1998) serve to elevate eyewitnesses' certainty in their identifications. It is ironic, therefore, that the elevated certainty of a witness resulting from a suggestive procedure should then be used to dismiss the suggestive procedure on grounds that the witness displays high certainty. In general, we have grave concerns about the use of show-ups.

This extent to which Rule 3 has been met in a given lineup can be tested using a "mock witness" procedure (Doob & Kirshenbaum, 1973; Malpass, 1981; Malpass & Devine, 1983; Wells, Leippe, & Ostrom, 1979). Mock witnesses are people who have never seen the culprit but are given the eyewitness's verbal description of the culprit, shown a picture of the lineup or photospread, and asked to select the person they think is the suspect in the case. If Rule 3 has been sufficiently met, a mock witness should not be able to select the suspect at a level that exceeds chance expectations based on the number of choices (number of lineup members) that could have been selected. If mock witnesses can deduce who the suspect is under these circumstances, then a concern is raised about whether an eyewitness's selection was a product of true recognition memory or was due merely to the same deduction process that the mock witnesses apparently used.

The idea of using distractors who fit the eyewitness's prior verbal description of the suspect also follows from our earlier discussion of the relative-judgment process. Specifically, if the suspect is the only person who fits the eyewitness's verbal description of the culprit, then this sets a high prior probability that the suspect is relatively more similar to the culprit than are the other lineup members. Consequently, any propensities for the eyewitnesses to make relative judgments will be disproportionately focused on the innocent suspect. This, in turn, destroys the theoretical upper limit on the probability that an eyewitness will identify an innocent suspect because in this case errors would not have an equal chance of occurring across all members of the lineup.

Using distractors who fit the eyewitness' prior description of the culprit may be one of the most commonly misunderstood recommendations that eyewitness researchers have made. Recent writings have made clear that distractors should not necessarily be selected so as to look like the suspect, but instead should be selected so that they fit the description that the eyewitness had given of the culprit (Luus & Wells, 1991; Wells, Seelau, Rydell, & Luus, 1994). Selecting distractors so as to resemble the suspect is not a desirable practice. At some point, the use of a rule that says that the distractors should resemble the suspect reaches a point where the lineup would be composed of clones. Although selecting distractors so as to make them resemble the suspect can meet the standards of Rule 3 (and pass the mock-witness test), such practices might create undue homogeneity and interfere with recognition of the actual culprit. Using the fit-description criterion, on the other hand, preserves sufficient variability across lineup members because verbal descriptions of culprits tend to be quite general and allow many people of different overall appearances to fit the description. As a result, fitting distractors to the verbal description does not risk creating a lineup of clones, does not interfere with recognition of the culprit, and yet does not make an innocent suspect stand out in the lineup (Juslin, Olsson, & Winman, 1996; Wells et al., 1993).

Misfit between Suspect and Description

Although the fit-description strategy is the preferred method in constructing a lineup, we recognize some special circumstances that pose practical problems to this strategy. First, a suspect's appearance sometimes is dramatically different from an eyewitness's description of the culprit. This situation can arise when some other evidence implicates the suspect (e.g., possession of the crime weapon). Here, embedding the suspect among a set of distractors who fit the witness' description would likely call attention to him or her because the suspect is the only lineup member who does not fit the description. When the eyewitness's description of the perpetrator does not fit the physical characteristics of the suspect, we recommend a distractor selection strategy that considers both features the eyewitness mentioned in describing the culprit and features of the suspect. With this modification of the fit-description strategy, we still recommend that the physical features considered in selecting lineup members be limited to those mentioned in the witness' description. However, where there is a disagreement between the witness' description of a fea-

ture and that feature's appearance in the suspect (e.g., witness described a culprit with red hair, suspect is brunette) we recommend using the suspect as the standard for the feature(s) across lineup members. Luus and Wells (1991) provide an example of this method of lineup construction in which the witness describes the culprit as a white male, 21–25 years old, with a protruding chin, dark hair, around 165 pounds, and around 5'9" tall. Suppose that the suspect has these characteristics except that his chin is actually somewhat receding and he is (as well as appears to be) 32 years old. Here we call for a joint strategy. In this case, distractors should be white males, around 32 years old in appearance with slightly receding chins and dark hair, around 165 pounds, and around 5'9" tall. The strategy of selecting distractors who fit the eyewitness's prelineup description of the perpetrator is not a strategy to be followed mindlessly. The critical test of whether the distractors were selected properly is the mock witness test. Would mock witnesses select the suspect more frequently than the other members of the lineup if the mock witnesses were given the description of the culprit?

Unique Nondescribed Features of Suspect

Another potential problem for implementing the fit-description strategy arises if the suspect has some unique feature that the eyewitness did not mention in describing the culprit. If we follow the fit-description strategy and ignore the feature in selecting distractors, the suspect would likely stand out as distinctive from other lineup members. However, we do not see this as problematic because "each distractor undoubtedly will have some unique features that if isolated for analysis, makes the distractor stand out" (Luus & Wells, 1991, p. 54). Again, the critical issue is whether the suspect stands out as unique in such a way that mock witnesses would select the suspect more frequently than the other members of the lineup if the mock witnesses were given the description of the culprit. In short, if the eyewitness did not mention a unique feature of the suspect, we do not feel it is necessary to replicate that feature across all lineup members. Variation across features that the witness did not mention in his or her description of the culprit allows recognition memory to come into play as the witness views the lineup.

Common Nondescribed Features of the Suspect

The degree of detail a witness provides in describing the culprit is important. Certain common features might be remembered but not reported in a prelineup description if a witness believes there is a "default value" for those features (Lindsay, Martin, & Webber, 1994). For example, a witness might neglect to mention the absence of facial hair in describing a clean-shaven male. "This could result in an obviously biased lineup if the accused matched several default values not shared by the foils" (Lindsay et al., 1994, p. 529). For instance, the failure of the eyewitness to mention anything about facial hair could lead to a lineup in which the suspect is clean shaven whereas the distractors all have some facial hair. In one sense, it could be argued that all lineup members fit the description equally well because

facial hair is not a part of the description. However, Rule 3 says that the suspect should not stand out in the lineup or photospread as being different from the distractors based on the eyewitness's previous description of the culprit *or based on other factors that would draw extra attention to the suspect.* It is the latter part of Rule 3 that can be violated if default values are not taken into consideration. Again, the critical issue is whether the suspect stands out as unique in such a way that the mock witnesses would select the suspect more frequently than the other members of the lineup if mock witnesses were given the description of the culprit.

Unique Descriptions

A somewhat different problem arises when an eyewitness is so specific in describing unique physical features (e.g., a scar or tattoo) of the culprit that finding a reasonable set of distractors is impossible. In such a situation, we question the need to construct a lineup. A lineup is useful to the extent that the eyewitness's verbal description is vague enough that it leaves doubt as to the identity of the perpetrator. With a vague description, a lineup provides the witness with an opportunity to recognize physical characteristics that he or she had been unable to recall when providing a prelineup description of the culprit. A recognition memory task (i.e., a lineup) seems unnecessary when an eyewitness' recall is so complete that he or she describes specific idiosyncratic physical features of the culprit. Under such circumstances, we believe the police need only apprehend a suspect who fits the witness' description.

Multiple Eyewitnesses

Another possible problem arises when there is more than one eyewitness and the various witnesses' descriptions differ from one another. On such occasions we recommend constructing separate lineups for each eyewitness. Although an expensive and labor-intensive exercise, this strategy ensures that all witnesses are not similarly exposed to some bias in a lineup's structure [see Wells (1988) for further discussion of multiple witnesses and lineup identification]. If there is some feature of the lineup that biases one eyewitness to select the suspect, then it will tend to have the same effect on other eyewitnesses, thereby producing "correlated error." This follows from the lineup-as-experiment analogy described in an earlier section, and is similar to the practice in experimental research of counterbalancing or randomizing stimulus and test materials.

Number of Lineup Members

Our formal recommendation regarding lineup structure does not state how many people should be in the lineup. The reason for this is because it would be arbitrary to pick a number. Should we recommend 6, 10, 20? This is similar in many respects to the question of how many people should be on a jury. What we know

is that the probability of false identification is inversely related to the number of lineup members and that there is a diminishing return on this probability with the addition of each lineup member. If we assume that the suspect is innocent and the lineup is fairly constructed, the chances that the innocent suspect will more closely resemble the actual culprit more than will the other lineup members is simply $1/N$, where N is the number of lineup members (Wells & Turtle, 1986). Hence, the chances that the innocent suspect will stand out by mere chance (as the best choice in a relative judgment process) is 1/6 in a 6-person lineup, 1/10 in a 10-person lineup, and so on. Levi (1997) has made a convincing argument that real-world identification rates for eyewitnesses viewing culprit-absent lineups is around 60%. This means that the chances of an innocent suspect being identified in a 6-person lineup, even when it is constructed and conducted according to the rules in this article, are 10% (i.e., 1/6 of 60%), far higher than what would seem acceptable to the justice system.

Rule 4: Obtaining Confidence Statements

A clear statement should be taken from the eyewitness at the time of the identification and prior to any feedback as to his or her confidence that the identified person is the actual culprit. This recommendation is based on the observation that confidence statements from eyewitnesses can be affected dramatically by events occurring after the identification (postidentification events) that have nothing to do with the witness's memory. As noted earlier, the confidence that an eyewitness expresses in his or her identification during testimony is the most powerful single determinant of whether or not observers will believe the eyewitness made an accurate identification. By recording the eyewitness's confidence at the time of the identification, postidentification factors (which have little to do with the witness's memory) will not yet have influenced the confidence judgment. If the confidence that an eyewitness expresses at trial is noticeably higher than it was at the time of the identification, then fact finders should consider the possibility that this inflation of confidence came from sources other than the goodness of the eyewitness's memory.

Simply thinking about how to answer questions about one's identification of the suspect can produce inflated confidence (Wells et al., 1981), which further supports the idea of measuring confidence at the time of the identification. Inflated confidence can also arise after the identification if an eyewitness learns that a cowitness has identified the same person (Luus & Wells, 1994) or by simply being told that the person identified was the suspect in the case (Wells & Bradfield, 1998, and in press). After the identification decision has been made by the eyewitness, giving the eyewitness any further information that implicates the accused (feedback such as "That is the person we suspected of the crime") is likely to increase the confidence that an eyewitness has in his or her identification. Hence, the eyewitness's confidence on the witness stand at trial may not reflect the degree to which the eyewitness's memory is trustworthy, but instead reflect the timing, type, and extent of other evidence against the accused (of which the eyewitness has somehow become aware). It is critical to note that the research indicates that eyewitnesses are not able to report accurately on the effects that feedback had on their confidence (Wells &

Bradfield, 1998). In other words, asking eyewitnesses later to report how confident they were at the time of the identification (prior to feedback) does not yield answers that reflect how they actually felt at the time, but instead reflects how they feel now. The only way to know how confident the eyewitness was at the time of the identification is to have asked the eyewitness at the time of the identification.

It is possible to argue that this other evidence is a legitimate influence on the eyewitness's confidence to the extent that the other evidence reflects the real likelihood that the eyewitness was accurate. For instance, suppose an eyewitness makes a tentative (low confidence) identification of a suspect and later learns that the suspect s/he identified has a criminal record for similar offenses. Is it legitimate for the eyewitness to thereby undergo an increase in his or her confidence that s/he made a correct identification? In one sense the answer must be "yes" because this information probably does increase the actual likelihood that the eyewitness identified the correct person. From a legal policy perspective, however, this influence is problematic. It is up to the jury or judge to decide the evidentiary value, if any, of the defendant's prior record. The eyewitness should be giving testimony based on the goodness of his or her memory alone rather than incorporating other evidence that makes the eyewitness more or less confident about the identification decision.

Suppose that the prior record information was ruled inadmissible at trial. Because the prior record was not admissible, the jury would not hear about it. Indirectly, however, the prior record will work against the defendant at trial via inflating the confidence of the eyewitness, whose confident testimony, in turn, will influence the jury.

There are numerous means for the eyewitness to find out about evidence, admissible or not, against the accused prior to trial. Hence, there may be no way to prevent confidence inflation between the time of the identification and the trial. Nevertheless, a clean record could be made of the confidence of the eyewitness at the time of the identification. When this recommendation is combined with Rule 1 (lineup to be conducted by someone who does not know who the suspect is), there is reason to be optimistic that a measure of confidence taken at the time of identification is largely a memory-based judgment and has some utility as an index of identification accuracy. When this recommendation is violated by allowing events to intervene between the identification decision and the confidence judgment, however, the eyewitness's confidence in his or her memory becomes confounded with other sources of confidence.

ANALYSIS OF THE COSTS AND BENEFITS

Costs

We are mindful of the problems with making recommendations for which there are significant financial or other costs to the legal system. Accordingly, we think it important that the four recommendations that we have made are largely without financial cost to the justice system. It would be difficult to argue, for instance, the insertion of the statement that the person in question might not be in

the lineup (Rule 2) is something that would cost the police in time or money. Perhaps the only recommendation that might bring costs to mind is Rule 1 regarding the necessity of having the lineup administered by someone who does not know which person is the suspect. However, we see no reason why this could not be another detective, a dispatcher, or an office worker in the police department. Administering a lineup or photospread is not a task that requires extensive training or special skills. Instructions to the eyewitness can be read from a script. Larger departments might want to automate the procedure by having the eyewitness view the photospread on a computer or view the lineup on a monitor and respond to questions presented on the screen so that there is no other person in the room. However, it is not necessary to invest in new equipment in order to satisfy Rule 1. In general, we do not see the four recommendations as involving any significant cost increases in time or money.

There are other types of costs that could be imagined from adherence to the four recommendations. Central among these is the possible cost to convictions of guilty persons. To what extent, for example, does selecting distractors who fit the description of the culprit or explicit warnings that the culprit might not be present in the lineup affect the chances that the guilty party will be identified? We have taken great care to recommend procedures that do not serve to reduce the chances that the guilty party will be identified. Consider, for instance, Rule 2. The research clearly shows that the chances that an innocent suspect will be identified in a culprit-absent lineup are reduced by this instruction but that the chances that a guilty suspect in a culprit-present lineup will be identified is not reduced by using this instruction (e.g., Malpass & Devine, 1981). This same pattern of results is observed for Rule 3 in that the selection of distractors who fit the description serves to reduce the chances that an innocent suspect will be identified but does not reduce the chances of identifying the guilty suspect (e.g., Lindsay & Wells, 1980; Wells et al., 1993). As for Rule 1, we see no defensible argument by which adherence to this recommendation could reduce the chances that the guilty party will be identified. Allowing agents who administer lineups to know which person is the suspect could only assist the chances of identifying the actual culprit if the agent is in fact using this knowledge to influence the witness, which should be disallowed by matter of policy. Rule 4, regarding collecting the confidence statement from the eyewitness at the time of the identification, might have the effect of lowering the average confidence of eyewitnesses by eliminating the boost their confidence might have received from external sources. In this sense, the average incriminating power of eyewitnesses might be lowered somewhat. But this boost in confidence that eyewitnesses would otherwise receive is artificial. Hence, it would be more appropriate to argue that there is a cost to not following Rule 4, namely the introduction of false confidence.

Benefits

The absence of apparent costs to the four recommendations stands in stark contrast to the benefits of implementing the four recommendations. The most obvious benefit would be the protection accorded innocent suspects. Somewhat less

obvious, perhaps, would be the benefit to the credibility of the legal system for making strong efforts to remove the system itself as a contributor to the eyewitness identification problem. There will always be some risk of mistaken identification by eyewitnesses, but there is a qualitative difference between mistakes that come from coincidence over which no one had control (e.g., a remarkable resemblance between the culprit and the innocent suspect) and mistakes that are the product of the procedures that are used in the justice system (e.g., where the innocent suspect stood out because police used distractors who did not fit the description of the perpetrator). The credibility of the justice system depends on the latter much more than the former. In light of the recently discovered cases of false identification and wrongful convictions (see Table 1) and the many more to come, we believe that the credibility of the justice system could become at risk in the eyes of the public. Implementing the four rules would be a major step in sending a message that the justice system understands the nature of the problem and is doing what it can to reduce the extent to which the system itself is contributing to false eyewitness identifications.

The 36 cases of false identification that we list in Table 1 produced more victims than just the 36 falsely convicted people. In a real sense, the 46 eyewitnesses who identified them and stated with high confidence in court that this was the guilty person are victims as well. They must now live with the knowledge that their testimony resulted in an innocent person going to jail. It is always risky to take real cases and explain the outcomes or lay blame, but we can rest assured that the eyewitnesses themselves are not to blame. In some cases, the eyewitnesses were no doubt victimized by improbable coincidence in which the suspect and actual perpetrator were near clones. However, we can also observe that the instructions commonly failed to stress that the actual culprit might not be present, that the lineups commonly used distractors who failed to meet the criteria of Rule 2, that the officers who administered the lineups and photospreads always knew which person was a suspect in the case, that the eyewitnesses were commonly given feedback after their identifications, and so on. It could be argued that the system owes it to the eyewitnesses themselves to use the procedures that are deemed safest.

Another benefit of implementing the four recommendations is that procedural adherence to these recommendations can lessen the need for expert testimony by eyewitness scientists. Eyewitness identification experts often focus, sometimes exclusively, on the procedures that were used for obtaining the identification evidence. When these procedures are pristine, many eyewitness experts will not proffer expert testimony, many judges will not grant expert testimony, and motions to suppress eyewitness identification evidence lose most of their power. Using procedures that violate these recommendations, in contrast, invites participation by credible eyewitness experts in the case for the defense, places the prosecutor in the difficult position of having to defend the absence of good procedures, routinely elicits motions to suppress the identification evidence, and risks the jury acquitting the defendant because there is another explanation (the suggestive procedures) as to why the suspect was identified by the eyewitness.

OTHER PROCEDURAL IMPROVEMENTS

Our four recommendations do not exhaust the realm of ideas regarding how to improve lineup identification procedures. For instance, we could make recommendations regarding multiple-witness cases in which some eyewitnesses might be used for developing composites (e.g., sketch artist and computer-based likenesses), recommendations regarding the role of clothing, the use of blank lineups, the use of special instructions for child eyewitnesses, and the construction of object lineups, to name some.

A main reason why we are proposing only these four rules at this time is that because we think that proposing too many rules would produce resistance by police and legal policy makers and perhaps dilute the import of the four primary rules. There are, however, two potential recommendations that are particularly important to discuss at this point, namely a recommendation that lineups be sequential rather than simultaneous and a recommendation that lineups be videotaped. We recognize that there are many eyewitness scholars who believe that the sequential idea and the videotaping idea should be among the rules recommended in this article. After considerable deliberation, the subcommittee authors of this article decided against expanding the list of four rules to include these additional two at this time.

In the following two subsections, we discuss sequential lineups and videotaping and explain why they are not among the core rules we are proposing. It is important to keep in mind that we support the idea of sequential lineups and the idea of videotaping. However, we are not willing to elevate these two ideas to the status of core rules.

Sequential Lineups

Perhaps the most important procedural variation that we have not incorporated into the core rules at this time is the use of the sequential lineup procedure. In a sequential procedure, an eyewitness views only one lineup member at a time and makes a decision (that is the perpetrator or that is not the perpetrator) regarding each person before viewing another lineup member. When compared to the usual simultaneous procedure, it is clear that the sequential procedure produces a lower rate of mistaken identifications (in perpetrator-absent lineups) with little loss in the rate of accurate identifications (in perpetrator-present lineups). Since the time it was first introduced (Lindsay & Wells, 1985), there have been many replications in the United States, Canada, Germany, and the United Kingdom of the superiority of the sequential procedure over the simultaneous procedure. Furthermore, psychological science understands rather well at this point why the sequential procedure works better than the simultaneous procedure, namely it reduces the propensities for eyewitnesses to make relative judgments (Wells, 1984). Hence, were we to add a fifth recommendation, it would be that lineup procedures be sequential rather than simultaneous.

We believe, however, that there is considerable utility in advocating only these four rules at this time for several reasons. First, we believe that these four rules,

judiciously followed, take care of the vast majority of problems that plague current practices in eyewitness identification. The superiority of the sequential over the simultaneous procedure is evident primarily under conditions where Rule 2 (warning the eyewitness that the culprit might not be present) and Rule 3 (distractors fitting the description) are violated (see Lindsay, Lea, Nosworthy, Fulford, Hector, LeVan, & Seabrook, 1991). Second, the four rules we propose are relatively independent of one another. In other words, adopting any one (or any subset) of the four rules is desirable independently of whether any one (or any subset) of the other rules is adopted. We believe that this is not the case for sequential lineups. In particular, the adoption of sequential lineups without the adoption of double-blind testing (Rule 1) might be worse than using simultaneous lineups without double-blind testing. Although we do not have specific empirical evidence to support this view, we fear that the influence of the lineup administrator who knows which person is the suspect would be greater with the sequential procedure because the administrator could more easily discern which photo or lineup member was being observed by the eyewitness at a given moment than is true of the simultaneous procedure. Third, we believe that the four rules we recommend are readily understandable to justice people in terms of how they work and why they are necessary. Because our recommendations are directed at the legal system, we think that each rule should have this "self-evident" nature. The sequential procedure, however, relies on a more complex understanding of the problem based on the relative-judgment conceptualization that we do not think is a part of the intuitions of legal policy makers at this point. Fourth, the rules we recommend at this time do not require significant deviations from current police practices, which involve simultaneous presentations. The sequential procedure, on the other hand, calls for a set of operations that is quite different from the usual practices of police departments. Finally, the four rules that we recommend in no sense prevent police from using sequential procedures. If sequential procedures are used, the same four rules apply.

Videotaping the Lineup and the Eyewitness

It is very tempting to believe that there should be a rule that the lineup and the eyewitness be videotaped. We favor this idea in theory. Consider all the potential reasons for videotaping the identification session. Videotaping could allow an independent, electronic record of the instructions given to the eyewitness, a record of the actual appearance of the photospread or lineup, a record of possible suggestions (both verbal and nonverbal) that might have flowed from the lineup agent to the eyewitness, and a record of the witness's reactions to the lineup. In addition, the tape could be subject to discovery rules to be examined by the defense, defense experts, and perhaps even the jury. These are powerful reasons to support the idea of videotaping.

Although we encourage videotaping lineups, we are not willing to make videotaping one of the core rules at this time for several reasons. First, unlike the four rules we have proposed, videotaping is not, in and of itself, a procedure that lessens the chances of false eyewitness identifications. We know of no evidence that vide-

otaping leads eyewitnesses to be less likely to make identification errors, for instance. Instead, videotaping falls into a category of record keeping for the purpose of post hoc review.

Second, we do not believe that videotaping will be nearly as effective in detecting problems in actual practice as it is in theory unless there are at least three cameras operating in synchrony. Videos are very limited in their visual scope, so there would have to be one camera focused on the eyewitness, one on the agent administering the lineup, and one on the lineup itself. In order to link any nonverbal behaviors of the agent or the lineup members to the reactions of the eyewitness, the cameras must be synchronized. In addition, the audio portion of a video is routinely very poor when nonprofessionals are making it.

Also, unlike the four rules we propose, there is additional cost to law enforcement in time, equipment, and materials associated with videotaping. In this sense, it violates a significant premise of our rules, namely that they are not associated with increased costs to law enforcement.

It is also important to note that we are uncertain at this time as to what effect videotaping might have on the behaviors of eyewitnesses. At least some in law enforcement have suggested to us that eyewitnesses would become even more anxious knowing that they were being videotaped, some would refuse to attempt an identification under such conditions, and so on. In the absence of empirical evidence one way or the other, we think it best to not make this one of our core rules at this time.

A final reason for not including the videotaping idea among the core rules is the fear that law enforcement would skirt Rule 1 (double-blind testing) on the excuse that video is available to the defense to see if there was any suggestiveness in the procedures. The existence of video, however, is no substitute for double-blind testing because of the limitations of video for capturing such influences.

We acknowledge that video might actually help prevent suggestive influence practices by lineup agents who might fear what a video could reveal to outside observers. However, we believe that adherence to Rule 1 (double-blind testing) is the only effective way to prevent systematic influence of this type from the lineup agent. We also agree that having some video, even if it is poorly done, might be better than having no video at all. Hence, we encourage the use of video, even while not making it one of the core rules.

SUMMARY

The extensive scientific literature in eyewitness identification has led to a good understanding of the fact that eyewitness identification errors can arise out of the procedures that are used for obtaining those identifications. The psychological processes involved in eyewitness identifications from lineups and photospreads, especially the relative judgment process, require that eyewitnesses be warned that the actual culprit might not be in the lineup and that all members of the lineup fit the verbal description that the eyewitness had given of the perpetrator. The dynamic interaction between the person administering the lineup and the eyewitness, in con-

junction with what we know about interpersonal influence, necessitates that the administering agent not know which person in the lineup is the suspect. In addition, the primary role played by eyewitness confidence in the legal system's assessment of the credibility of the identification, in conjunction with clear empirical evidence of confidence malleability, demands that confidence statements be obtained at the time of the identification (before other variables begin to exert their influence on the eyewitness). The adoption of these four rules into lineup practices can remove a great deal of the contribution that the justice system itself contributes to the problem of mistaken identification.

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