What is This?
Police Lineups as Experiments: Social Methodology as a Framework for Properly Conducted Lineups

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Research findings over the last decade have given rise to guidelines about how to minimize the likelihood of false identifications in police lineups and photo spreads. It is argued here that experimental social psychology's common understanding of factors that contaminate research experiments, such as demand characteristics, experimenter bias, and lack of control groups, has been the principal framework leading to hypotheses about how to improve police-conducted lineups. The analogy between a methodologically sound social psychology experiment and a properly conducted lineup has guided eyewitness identification research implicitly; in this article the analogy is expanded and made explicit. Lineup research examples deriving from the lineup-as-experiment analogy, such as the mock-witness control group and the blank-lineup control, are described. Finally, it is argued that research findings that are modeled on the lineup-as-experiment analogy are natural system variables that might be especially immune to some of the criticisms that have been launched against expert testimony on eyewitness matters.

The identification by an eyewitness of a suspect can be an event of profound consequences. On the one hand, it might be the key event that saves a community from subsequent rapes, muggings, or murders by a repeat offender. On the other hand, it might be the event that seals a conviction of an innocent person, committing that person to a life of incarceration while the guilty person remains free.

It is our contention that experimental social psychology has contributed to meaningful improvements in how to reduce errors in eyewitness identification tasks. The Law Reform Commission of Canada, for example, relied heavily on the published psychological literature and on social psychologists as consultants in developing its recommendations for pretrial identification procedures in 1983. More recently, Eyewitness Identification: A System Handbook (Wells, 1988), a culmination of the experimental research that has been conducted in the 1970s and 1980s directed at practical procedures for obtaining reliable eyewitness identifications, has been adopted in police training centers. And psychologists are now conducting workshops

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for judges, police officers, and attorneys in which there is a focus on the social psychological dynamics of lineup and photo-spread procedures.

In this article we describe some of the findings that have made the eyewitness identification literature useful for applied purposes and the role that has been played by experimental social psychology. It is our contention that although substantive theory in social psychology has had some impact on the practical research literature on eyewitness identification, it has been the theory of what constitutes a sound social psychology experiment that has directed most of the advances in this area. Thus, the focus of this article is on the relationship between experimental methodology and the technology of lineups. From the outset we acknowledge that substantive theory has played a role in the eyewitness area. For example, self-perception theory has been postulated to account for the weak correspondence between eyewitness identification accuracy and eyewitness confidence (e.g., Leippe, 1980; Kassin, 1985), attitude polarization theories have been used to account for confidence inflation by eyewitnesses (e.g., Wells, Ferguson, & Lindsay, 1981), cue degradation has been implicated in identification accuracy (Cutler, Penrod, & Martens, 1987a, 1987b), and various eyewitness researchers have made effective use of social psychology’s knowledge about suggestibility, schematic biases, credibility effects, and so on. But we argue that the contributions of experimental social psychology to the development of procedures that improve eyewitness identification have been derived primarily from a somewhat different source—a source that we call the “lineup-as-experiment analogy.”

LINEUPS AS EXPERIMENTS

A lineup is a legally recognized police investigation procedure in which a suspect is embedded among distracters. The eyewitness’s task is to decide whether or not the culprit in question is in the lineup and, if so, which person is the culprit. Almost all components of the lineup task can be likened to elements of a social psychology experiment. The lineup-as-experiment analogy can be described as follows: In the image of a social psychology experiment, the officer conducting the lineup is like an experimenter, the eyewitnesses are the subjects, instructions to the eyewitnesses can be likened to an experimenter’s protocol, the suspect is a stimulus and the selection of lineup members and the positioning of the suspect in the lineup are part of the design. As well, police have a hypothesis (e.g., that #4 is the guilty party) and have created a design and procedure to test the hypothesis. The eyewitnesses’ choices or identification behaviors constitute the data from which the validity of that hypothesis will be evaluated by police and possibly a prosecutor, judge, and jury. Some components of this analogy are illustrated in Table 1. The analogy can be extended even further by noting that an outcome of an experiment, even if it is methodologically flawless, can be considered only a statistical truth, as there remains some probability that chance factors were at play. Similarly, a positive identification, even if it is from a flawlessly conducted lineup, might be a false identification. Thus, both an experiment and a lineup are merely probabilistic in their discovery of truth.
### TABLE 1  Some Components of the Lineup-as-Experiment Analogy

<table>
<thead>
<tr>
<th>Experiment Term</th>
<th>Lineup Counterpart</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Experimenter’s hypothesis</td>
<td>That the suspect is the perpetrator</td>
</tr>
<tr>
<td>2. Null hypothesis</td>
<td>That the suspect is not the perpetrator</td>
</tr>
<tr>
<td>3. Experimenter</td>
<td>The lineup administrator</td>
</tr>
<tr>
<td>4. Subjects</td>
<td>The eyewitness(es)</td>
</tr>
<tr>
<td>5. Stimulus</td>
<td>The suspect</td>
</tr>
<tr>
<td>6. Confederates</td>
<td>The foils in the lineup</td>
</tr>
<tr>
<td>7. Design</td>
<td>The suspect is embedded among distractors, or foils</td>
</tr>
<tr>
<td>8. Possible manipulations</td>
<td>Number of foils, appearance of foils, presence or absence of suspect</td>
</tr>
<tr>
<td>9. Procedure</td>
<td>The instructions to eyewitnesses regarding their task</td>
</tr>
<tr>
<td>10. Dependent measures</td>
<td>Recognition, certainty of recognition</td>
</tr>
<tr>
<td>11. Outcome</td>
<td>Identify suspect or identify foil or identify no one</td>
</tr>
</tbody>
</table>

This simple analogy, seemingly obvious after giving it some thought, is extremely rich in its implications. Experimental social psychologists know a great deal about an assortment of factors that create problems for interpreting experiments. These include demand characteristics, response bias, inadequate sample size, experimenter effects, unreliable measures, lack of control groups, and a host of possible confounding variables. Indeed, with the aid of some well-written chapters and books on experimental artifact (e.g., Aronson & Carlsmith, 1968; Rosenthal, 1966), methodological expertise often is the first and most important training given social psychology graduate students. And after graduate school social psychologists continue to exercise these skills of detecting, criticizing, correcting, and avoiding experimental artifact in their activities as researchers and peer reviewers. It is perhaps safe to argue that most experimental social psychologists have come to think in ways that are sophisticated in the realm of experimental reasoning (see Lehman, Lempert, & Nisbett, 1988). And as we have seen in recent years of research on attribution and on judgment and decision processes, human intuition does not pattern itself naturally on scientific reasoning (e.g., Ross, 1977). Therefore, to the extent that there is a real-world task being conducted by nonscientists that mimics the logic of experimental methodology, we should be able to translate the task, at least metaphorically, into experimental terms and thereby help assure the validity of outcomes resulting from that task.

The analogy between a good experiment and a good lineup fits so well that we believe that experimental social psychologists are almost perfectly suited to the task of testing and defining the best ways to conduct lineups. Discipline-accrued knowl-
edge of how to avoid such problems as confoundings and demand characteristics in a social psychology experiment can be used to formulate hypotheses about what might be a good or poor lineup procedure. Using the staged-crime paradigm, in which lineup designs and procedures can be manipulated systematically, allows us to study the impact of variations in lineup designs and procedures on rates of false and accurate identifications.

In effect, experimental social psychology has a *theory of methodology*, which, it might be fair to say, is more widely accepted than any substantive theory in social psychology. Although not all these features of experimental methodology are unique to social psychology, some (such as experimenter effects and demand characteristics) have been developed specifically in social psychology in ways that are related directly to the types of problems encountered in an eyewitness identification task. Indeed, we argue that the concept of a methodologically sound social psychology experiment has been the guiding heuristic behind the development of research hypotheses aimed at specifying the “properly conducted lineup.”

In the next section, we describe research examples that have yielded useful improvements in the structures and procedures involved in police lineups, and we illustrate how each of these contributions represents a simple extension of our conception of a well-designed social psychology experiment. As such, these studies speak to the value of our collective methodological wisdom in social psychology for helping solve applied problems.

**RESEARCH EXAMPLES**

In a typical police lineup, a suspect is placed among nonsuspects (distracters, or foils) and paraded before the eyewitness(es). One of the problems with the operation of police lineups that follows directly from the lineup-as-experiment analogy is the lack of a control group. There have been two general approaches to this problem. One approach is to have subjects who have not been exposed to the culprit (i.e., are not eyewitnesses) try to identify the suspect from the lineup (see Wells, Leippe, & Ostrom, 1979). The other approach is to show the eyewitness a lineup that does not contain a suspect before showing the eyewitness a lineup that does contain a suspect (Wells, 1984). The former control procedure (called the mock-witness control) is a between-subjects type of control that is meant to be used a priori to create a good lineup, but it can also be used after the fact to indicate whether there are problems in interpreting the eyewitness’s identification. The second control procedure (called the blank-lineup control) is a within-subjects type of control that is designed to screen out eyewitnesses who have a poor memory for the perpetrator or who are prone merely to choose the lineup member who looks most like the perpetrator.

Generally, police have not recognized the need for either the mock-witness control or the blank-lineup control. Instead, it has been assumed that most problems are handled adequately by the use of distracters, or foils. Distracters, or foils, are known-innocent members of a lineup or photo spread. In theory, the presence of foils can help protect the accused against the witness who is merely guessing. In other
words, the prospect of a chance identification of the accused is assumed to diminish proportionately to the number of known-innocent foils present in the lineup. Indeed, this simple principle has been well recognized by legal scholars and courts, as they have routinely argued against the value of “show-ups.” A show-up is an identification of the accused based on a one-to-one confrontation between the accused and the witness (Sobel & Pridgen, 1981).

Social psychologists have not been content with the assumption that the use of foils necessarily controls for chance identifications of the accused (Doob & Kirshenbaum, 1973; Wells et al., 1979; Malpass & Devine, 1984). Part of the problem stems from the difficulty of defining adequately the qualities of a good foil. Doob and Kirshenbaum (1973) identified this problem by showing that “mock witnesses”—that is, people who had not witnessed the event and culprit in question—could identify the suspect from an actual police lineup at a rate greatly exceeding chance on the basis of merely having read a general description of the culprit. Wells et al. (1979) used this mock-witness paradigm to develop a metric that specifies the number of functional foils (called functional size) and, as such, helps define a given lineup’s ability to control for chance. Other metrics have emerged (e.g., see Malpass & Devine, 1984), each using mock witnesses as a type of control condition for studying the distribution of naive observers’ choices of lineup members. Data from mock-witness control subjects can be used in actual criminal cases to show that the suspect ran a greater-than-mere-chance risk of being chosen for reasons other than true recognition. This mock-witness procedure was used recently by Neil Vidmar and the first author of the current article in a British Columbia Supreme Court criminal trial in 1987.

The logic of mock-witness control conditions extends beyond the idea of helping to define and control chance. Mock witnesses, who observe the actual lineup or a photograph of the lineup, can be used to help determine whether the witness was able to discern the police investigator’s hypothesis. As in a good social psychology experiment, for which the subject should not know the experimenter’s hypothesis, the police investigator’s hypothesis (regarding which member of the lineup is the culprit) should not be apparent from the structure or composition of the lineup. The lineup task is based on the premise that the eyewitness will choose someone from the lineup solely on the basis of the witness’s memory of the culprit, not the witness’s deductions about which person in the lineup is the one suspected by the police. If mock witnesses who have no actual memory for the culprit are able to identify the suspect, then the police investigator’s hypothesis (e.g., that the culprit is #4 in the lineup) must have been obvious to the witnesses. In social psychology experiments, we have learned that a subject’s knowledge of the experimenter’s hypothesis contaminates the interpretation of the subject’s response to subsequent dependent measures; so, too, an eyewitness’s knowledge of which lineup member is the suspect contaminates the interpretation of any identification of that suspect.

The second conception of a control condition for lineups that can be used in actual cases has been tested experimentally and advocated recently—namely, the blank-lineup control (Wells, 1984). In the vein of many social psychology experiments, the blank-lineup control involves what might be construed as an implied deception. A
blank lineup is one that contains no suspect; that is, it is composed completely of foils. The eyewitness is shown a blank lineup prior to the actual lineup. The implied deception involved in the blank-lineup control stems from the fact that the eyewitness is led to believe that the first (i.e., blank) lineup includes a suspect in the case when in fact it does not. If the eyewitness chooses someone from the blank lineup, then it is a harmless error in the sense that there are independent sources to verify that the identified person was not the culprit. In effect, the blank lineup can be a powerful way to detect any response bias tendencies of the eyewitness to choose someone or tendencies to choose the person who most looks like the culprit relative to the other lineup members. The efficacy of the blank-lineup control has been demonstrated using staged-event experiments. The results indicated that the blank lineup effectively discredited many eyewitnesses because they identified someone from the blank lineup; presumably they were overly eager to identify someone or their memory of the culprit was exceptionally poor. Importantly, the rate of misses (i.e., failure to identify the culprit in the second lineup) was not significantly affected by the use of the blank lineup (Wells, 1984).

The finding that the blank-lineup control procedure has little or no effect on miss rates is critical to the development of an applied literature on lineups. In effect, any procedure that reduces false identifications without a corresponding increase in the rate of misses is a net improvement akin to an increase in $d$ prime in a signal detection task. If the goal were merely to reduce false identifications, many procedures could meet this goal, some of which are absurd and unacceptable. Eyewitnesses could be instructed not to choose anyone, for example, and this would guarantee that no false identifications would result. Of course, it would also guarantee a high rate of misses. Importantly, however, manipulations of high versus low functional size and the blank-lineup versus no-blank-lineup control have been shown to affect false identifications without corresponding changes in miss rates. In lineup tasks, it is possible to reduce false identification rates without increasing hit rates, because the identification of a foil, or distracter, is not a false identification. Although the identification of a foil is an error, it is not an error that results in charges against the identified person, because foils are, by definition, known by the authorities to be innocent of the offense in question. In other words, false identification rates can be reduced by procedures that shift identifications of an innocent suspect toward identifications of a known-innocent foil. Thus, with lineups there are procedures that can reduce false identifications while not affecting miss rates, even though these procedures do not involve actual improvements in the eyewitnesses’ memories and do not require changes in signal-to-noise ratios. This concept is discussed again (below) with respect to Wells and Turtle’s (1986) analysis of lineup models.

It is easy to see how the lineup-as-experiment analogy has led to the development of hypotheses about how to improve on current police practices. The two developments described above, the mock-witness control and the blank-lineup control, are merely extensions of the logic of the good social psychology experiment. The lineup-as-experiment analogy implies that certain other types of control are needed as well, such as the double-blind procedure. Research by Buckhout (1974), for example,
demonstrated that a lineup administrator can influence an eyewitness’s decision about which photograph to choose in a photo spread by nodding, smiling, and leaning forward when the eyewitness is looking at a particular picture. This experimental observation derives directly from the idea that a good social psychological experiment requires control for experimenter bias. Other research developments of considerable applied importance that derive from the lineup-as-experiment analogy include experimental evidence that lax criteria (in the signal detection sense) promote false identifications and, therefore, cautions against guessing must be part of the eyewitness instructions (e.g., Egan & Smith, 1979).

Wells and Turtle (1986) extended the lineup-as-experiment analogy even further in their treatment of lineup models (or designs) and error rates. They noted that although the most common design for police lineups involves placing one suspect among several known-innocent foils (single-suspect lineup design), some police departments put together lineups in which all members of the lineups are suspected of being the person who committed the offense (all-suspect lineup design). The distinction between a single-suspect lineup and an all-suspect lineup is a critical one because the former allows for known errors (“I’m sorry, Mrs. Jones, but you identified Officer Miller”), whereas the latter does not. In parallel with the distinction between “error rate per comparison” and “experiment-wise error rate,” Wells and Turtle used data from the staged-crime paradigm to show that the “lineup-wise error rate” is dramatically higher with all-suspect lineups than with single-suspect lineups, whereas the “error rate per suspect” is largely unaffected.

The lineup-as-experiment analogy has also been applied to the interpretation of the outcome. As Greenwald (1975), among others, has noted, researchers often treat null effects inappropriately, as though they were uninformative about the hypothesis in question, and commonly see these outcomes as “nonevents.” Wells and Lindsay (1980), using the lineup-as-experiment analogy, noted that nonidentifications (i.e., failures by eyewitnesses to positively identify a suspect in a lineup) are treated largely as nonoccurrences by police, attorneys, and courts (also see Leippe, 1985). In fact, however, experiments using the staged-crime paradigm show that the probability of a nonidentification varies robustly as a function of whether the suspect in the lineup is the actual culprit. Using the same kind of Bayesian estimation formulas that can be used in a social psychology experiment, Wells and Lindsay demonstrated that the posterior probability that the suspect is the culprit (i.e., the probability that the police investigator’s hypothesis is true) ought to be lowered by the incidence of a non-identification. Thus, the same kind of prejudice that experimenters exhibit against the null hypothesis seems to be exhibited by police against nonidentifications. Note that, for purposes of this article, we framed the nonidentification issue as part of our lineup-as-experiment analogy by drawing a parallel to null effects. But there is also a substantive theory that would have predicted that nonidentifications are underutilized. This can be seen in the theoretical work on the problems that people generally have with negative instances or nonoccurrences (e.g., see Fazio, Sherman, & Herr, 1982; Wason & Johnson-Laird, 1965).
EXPERT TESTIMONY ON EYEWITNESS IDENTIFICATION

There is an interesting characteristic of research in the lineup-as-experiment tradition that allows it to escape one of the devastating criticisms launched against expert testimony on eyewitness matters. Critics have charged that eyewitness research findings are poorly suited to the task of estimating population parameters for real cases coming before the courts. Pachella (1986) notes, and we agree, that eyewitness experiments are hypothesis-testing procedures involving isolated variables within a “fixed effects” logic. This is not well suited to generalizing to real cases, because in real cases, unlike experiments, other (unmeasured and unmanipulated) variables are not necessarily “randomized out.” Pachella did not provide a clear-cut example of this problem, but recent research by Yuille and Cutshall (1986) seems to illustrate this type of problem. Experimental research indicates that high stress lowers eyewitness accuracy (see Deffenbacher, 1983). Contrary to the conclusion of these fixed-effect experiments, however, Yuille and Cutshall found in their analyses of 13 eyewitnesses to an actual crime (a shooting incident) that the witnesses who had higher levels of stress were the ones with greater, rather than lesser, accuracy. The explanation for this discrepancy between the experimental results and the real-case results appears to rest with the fact that the eyewitnesses who were most stressed from the shooting incident were also the ones who were closest to the shooting activity and, therefore, had a better view of the shooting incident. Thus, whereas experiments might prove that higher levels of manipulated stress produce lower levels of eyewitness accuracy when other variables (such as opportunity to view) are equated through randomization, in real cases the variable in question (e.g., stress) might be correlated with other casual variables (e.g., opportunity to view). In this sense, it might be misleading for an eyewitness expert to testify in an actual case that stressed witnesses are less likely to be accurate than unstressed witnesses.

Variables that can be manipulated in actual cases (such as the functional size of lineups and instructions to witnesses) have been labeled system variables, whereas those not under the manipulative control of police in actual cases (such as stress levels or opportunity to view at the time of witnessing) have been labeled estimator variables (see Wells, 1978). Variables derived from the lineup-as-experiment analogy are natural system variables and, as such, may be much less susceptible than estimator variables to the criticism launched by Pachella (1986). In particular, the fixed-effects hypothesis methodology in eyewitness research is directly applicable to real-world cases for system variables (such as lineup instructions), less so for estimator variables (such as stress). The reason is that police can control, “fix,” or independently determine the level of system variables in actual cases in manner that allows other variables (such as witnessing conditions) to remain uncorrelated with the system variable. Thus, whereas our independent variable manipulations of an estimator variable, such as stress, might not allow us to say that stressed witnesses are less likely than unstressed witnesses to be accurate in actual cases, we can say that a system variable, such as biased instruction to a witness, is more likely than an unbiased instruction to produce error in a witness in actual cases. The difference has to do with the fact that
estimator variables are independent manipulations in our experiments but are contaminated by covariates in actual cases, whereas system variables are independent manipulations both in our experiments and in actual cases.

Undoubtedly, individual researchers will need to decide whether to provide expert testimony on the basis of a wide variety of considerations (see Goldman, 1986; Konechi & Ebbesen, 1986; Loftus, 1983, 1986; McCloskey & Egeth, 1983; Wells, 1978, for discussions of these multiple considerations). It is not our intention to argue that researchers should avoid discussion of estimator variables in court. But we argue that there is at least one class of variables, system variables, for which the logic of our fixed-effect methodology is directly applicable to the behavior of these variables in real-world cases.

CONCLUSIONS

The creation of a useful, applied science of police lineup administration has been aided by substantive theory but has derived a great deal of its direction from the analogy between a good lineup and a good social psychology experiment. Research and recommendations concerning the mock-witness control, the blank-lineup control, double-blind lineup procedures, lineup designs, and interpretation of nonidentification outcomes are part of an applied research program that has made use of our methodological and statistical technology in experimental social psychology.

The research developments on how to improve lineups briefly described in this article constitute only a small sample of total developments over the last decade. The total pool of accumulated knowledge has been substantial enough to yield a 139-page handbook written for police and attorneys that includes 131 procedural recommendations (Wells, 1988). Some courts are already acknowledging these recommendations by admitting expert testimony contrasting and comparing how a lineup was conducted according to these research-based recommendations. Table 2 lists a few examples of these recommendations for conducting lineups and photo spreads, with a brief statement of their relationship to the idea of a methodologically sound social psychology experiment. One of these recommendations, for example, is that eyewitnesses should be separated as soon as possible when the officer first arrives at the scene of a multiple-eyewitness incident. This follows from the logic of a good social psychology experiment in that it helps prevent subjects from influencing one another. If it were a social psychology experiment on group influence, of course, this recommendation would not apply. Loftus and Greene (1980), in fact, demonstrated with a staged-event experiment that eyewitnesses can be misled about the facial characteristics of a confederate perpetrator if they overhear false information from another witness prior to viewing a lineup.

Although these recommendations for a properly conducted lineup seem obvious in the context of our lineup-as-experiment analogy, they have been violated frequently and questioned only seldom in actual police practice. They are obvious to us as social psychologists because the idea of what constitutes a good social psychology experiment is well ingrained in our thinking to a level that is intuitive and perhaps even automatized. Graduate education in psychology has, in fact, sharpened our method-
TABLE 2  Examples of Recommendations for the Conduct of Lineups and Photo Spreads

<table>
<thead>
<tr>
<th>Examples</th>
<th>Experiment Analogue</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Witnesses should be separated as soon as possible</td>
<td>If subjects interact prior to responding to the dependent measure, then their data cannot be analyzed as though they were independent</td>
</tr>
<tr>
<td>2. At no time should a witness be led to believe that the actual perpetrator is in the set of mugshots</td>
<td>Experimenter protocols should be worded in a way that does not create demands on the subject to respond in a particular way</td>
</tr>
<tr>
<td>3. The officer conducting the photo spread should not be knowledgeable of whom the police suspect in the case</td>
<td>Experimenter who come into contact with subjects should be kept blind about the condition to which the subject is assigned</td>
</tr>
<tr>
<td>4. If there is more than one witness, the position of the suspect in the photo spread should be changed for each witness</td>
<td>Stimulus presentation order should be randomized or counterbalanced across subjects</td>
</tr>
<tr>
<td>5. No cues of any kind should be given to the witness concerning whether or not the identified person is the suspect in the case (at least until a statement of certainty is obtained from the witness)</td>
<td>Debriefings regarding the experimenter’s hypothesis and related matters should not be given until all dependent measures have been collected</td>
</tr>
</tbody>
</table>

ological reasoning (see Lehman et al., 1988), and it might now be difficult for us to appreciate fully the extent to which these seemingly intuitive ideas about the need for control groups are not a part of most people’s general knowledge. Indeed, it is interesting to note that the first formal treatment of the need for control groups did not occur until the 19th century, when the subject was discussed by Mill (1851). In fact, Boring (1954) notes that the use of control groups in experimental work is a 20th century phenomenon, coming quite late in the history of human thought. It should not be particularly surprising, then, that the notion of a control group for police lineups has only recently been advocated and that it has not been a part of intuitive forensics up to this point.

Recently, police training academies as well as continuing education programs for practicing trial judges and attorneys have started to recognize that lineup identification designs and procedures are lax. Accordingly, these organizations have called on eyewitness researchers in psychology to give workshops on eyewitness identification. If the experience of the first author of this article can be generalized, these workshops are well received as long as use of the experimental terms (such as those used in Table 1) is avoided and their lineup counterparts used instead. One then needs merely to articulate the concepts of confoundings, experimenter effects, control groups, and...
so on, in common language with concrete examples in order to communicate a useful set of recommendations for consumption by police, judges, and attorneys.

NOTES

1 This article is the first to formally recognize and articulate the lineup-as-experiment analogy. To our knowledge, however, it was Anthony N. Doob who first mentioned the possible utility of such an analogy while delivering a talk at the Alberta Eyewitness Research Conference in Edmonton, June 1980.

2 The blank-lineup control is more of a social psychological invention than the mock-witness control because of the way it takes advantage of mild deception, and the validity of the procedure depends critically on the success of this deception. And, as with many of our social psychology experiments, public knowledge of the ruse would create formidable problems. If eyewitnesses knew, for example, that the first lineup never contains the suspect, they would merely wait for the second, and the efficacy of the blank-lineup control would be lost.

REFERENCES


