The Diagnosticity of a Lineup Should Not Be Confused With the Diagnostic Value of Nonlineup Evidence

Gary L. Wells and C. A. Elizabeth Luus
Iowa State University

We argue that a lineup is diagnostic to the extent that it reveals information from recognition memory that was not available from the eyewitness's recall memory. Accordingly, lineup diagnosticity is defined as the probability that an innocent suspect is identified by the witness when lineup members resemble the eyewitness's prelineup description. Navon (1990) argued that this definition underestimates lineup diagnosticity because it ignores other evidence against the accused. We argue that these other forms of evidence, which Navon calls ecological parameters, have diagnostic utility but should not be confused with the diagnostic value of the lineup itself. Furthermore, although Navon correctly noted that experimenters' selections of innocent suspects in eyewitness experiments are not random, we argue that police select suspects in a similarly nonrandom manner.

Lineups and photospreads are important tools that police investigators use for obtaining identifications from eyewitnesses. But it was not until the 1970s that psychologists systematically began to conduct experiments with lineups and to study the statistical and structural characteristics of the lineup. Although the surprising frequency of false identifications in experiments has fascinated psychosocial scholars, statistical and structural issues have proved to be particularly important to the understanding of lineups.

Consider one such structural and statistical issue. Wells and Lindsay (1980) pointed out that, if a lineup is structured so that there is only one suspect, with the remaining lineup members being known-innocent foils, then it is ecologically invalid to count all errors of identification as false identifications. In an actual police lineup, the identification of a known-innocent foil will not result in charges being laid against that person; only identifications of a suspect will have such a result. Thus, many experiments may exaggerate the likelihood of a false identification because they sum all identification errors across lineup members.

Statistical studies of the single-suspect lineup structure have been important for understanding how the risk of false identification (of an innocent suspect) is not equivalent to chance of mistaken recognition (which includes foils or distractors). These statistical studies have also allowed researchers to explore how alternative lineup structures (e.g., the all-suspect lineup), which are sometimes used in police investigations, are associated with additive levels of risk (Wells & Turtle, 1986).

In this article, we respond to an argument made by Navon (1990) that the diagnosticity of a lineup must include ecological parameters that consider the likelihood that a suspect who is placed in a single-suspect lineup is in fact the culprit. We argue that these ecological parameters should not be confused with the diagnosticity of the lineup itself. They are forms of evidence against the accused, but they are nonlinear forms of evidence. Although ecological parameters must be combined with lineup diagnosticity for posterior or overall probabilities that the suspect is the culprit to be calculated, ecological parameters are not aspects of lineup diagnosticity.

The Diagnosticity Index

Wells and Lindsay (1980) proposed that the diagnosticity of a single-suspect lineup can be expressed as a ratio of two conditional probabilities. If an eyewitness identifies the suspect (IDS), then the diagnosticity of the identification is expressed as

\[
\frac{p(\text{IDS}|S=C)}{p(\text{IDS}|S \neq C)}
\]

Thus, lineup diagnosticity is represented as the probability of identification of the suspect given that the suspect is the culprit, divided by the probability of identification of the suspect given that the suspect is not the culprit. If the ratio is equal to 1.0, then the identification of the suspect is nondiagnostic because such an identification is no more likely to happen when the suspect is the culprit than when the suspect is not the culprit. A diagnosticity value of 3.0 indicates that the identification of the suspect is three times more likely if the suspect is the culprit than if the suspect is not the culprit, and so on.

Navon (1990) argued that the lineup-diagnosticity index fails to reflect the overall probability that the suspect is the culprit. We agree. But the lineup diagnosticity index should not be a measure of all evidence; lineup diagnosticity should reflect only the evidentiary contribution of the lineup. Suppose, for example, that a suspect in a rape case was linked to the culprit through genetic testing of body fluids and that there was a 99% likelihood that the suspect being viewed by the victim-witness in a lineup is in fact the culprit. In what sense does the presence or absence of genetic testing make the lineup more or less diagnostic? It certainly does not improve the witness's memory or vision. Although the genetic test must be considered in assess-

Correspondence concerning this article should be addressed to Gary L. Wells, Department of Psychology, Iowa State University, Ames, Iowa 50011.
ments of the probability that the suspect is the culprit, the genetic test is not and should not be part of a lineup-diagnosticity index. Indeed, it would be strange to consider a lineup to be highly diagnostic when there is a genetic test performed, not diagnostic if the genetic test results were somehow misplaced, and once again diagnostic if the genetic test results were later found.

We believe that Navon's (1990) treatment of diagnosticity is useful to the extent that it illustrates how the probability of a suspect's guilt depends on more than just the results of a lineup test. We disagree, however, that these other forms of evidence should be contained in the index of lineup diagnosticity. In the following sections, we describe distinctions between the diagnosticity of a lineup test, prior probabilities that derive from nonlineup evidence, posterior probabilities that result from the combination of lineup diagnosticity and prior probability, and the amount of information gained from a lineup test. We then present some hypothetical examples of lineup tests in which the witness could not possibly select the culprit except by chance, and yet Navon's analysis would attribute diagnosticity to the lineup. Finally, we discuss the problem of fixed resemblance, in which experimenters' strategies for selecting innocent suspects as stand-ins can affect the levels of diagnosticity obtained in research.

**Diagnosticity, Posterior Probability, and Information Gain**

In addition to lineup diagnosticity, Wells and Lindsay (1980) developed two other indexes. One of these, posterior probability, represents the way in which diagnosticity combines with other (nonlineup) evidence to yield an overall probability that the suspect is the culprit. The other index, information gain, represents the amount of change in probability that is required when the outcome of the lineup is considered versus when only the nonlineup evidence is considered. For example, if nonlineup evidence leads to a .75 probability that the suspect is the perpetrator, and if this probability increases to .90 following identification of the suspect in a lineup, then the difference of .15 represents the information-gain value of the lineup.

Unfortunately, Navon (1990) focused only on the diagnosticity index and ignored Wells and Lindsay's (1980) development of posterior probability and information gain, both of which involve considerations of nonlineup evidence against the suspect. Perhaps this explains why Navon considers nonlineup evidence to be part of the lineup diagnosticity index. In any case, it is important to note that Wells and Lindsay's Equation 4 (p. 779) defined the posterior probability that the suspect is the culprit, given an identification of the suspect, as

\[
p(S = C|IDS) = \frac{p(IDS|S = C)p(S = C)}{p(IDS|S = C)p(S = C) + p(IDS|S \neq C)p(S \neq C)}, \tag{2}
\]

where \(p(S = C)\) represents the probability that the suspect is the culprit based on prior or nonlineup considerations. Notice that the diagnosticity index from Equation 1 of this article is a component of the right-hand term of the Bayesian expression in Equation 2. In effect, Equation 2 specifies how diagnosticity should be combined with nonlineup evidence to derive a posterior probability that the suspect is the culprit.

The term \(p(S = C)\) is the "prior probability that the suspect is the criminal before learning whether the witness identified the suspect from the lineup" (Wells & Lindsay, 1980, p. 778). Perhaps some of the confusion that led Navon to claim that Wells and Lindsay (1980) ignored ecological parameters (i.e., other evidence) stems from the word _prior_. All that _prior_ means in this context is that the prior probability is derived from information or facts that are not aspects of the lineup (i.e., prior to considering any outcome or information gained from the lineup). Thus, if a witness describes the culprit as having physical features that are extremely rare and the suspect has these features, there is a high prior probability that the suspect is the culprit. But this is true whether or not a lineup test is conducted. The purpose of the lineup, and its diagnostic value, revolves around the question of what can be learned about the likelihood that the suspect is the perpetrator that could not be learned or was not known from other sources of evidence.

The amount of new information obtained from a lineup is measured by the difference between the prior probability and the posterior probability. Wells and Lindsay (1980) called this difference score _information gain_ and expressed it as

\[
|p(S = C) - p(S = C|IDS)|. \tag{3}
\]

If there is no diagnosticity (as measured in Expression 1), then information gain is 0. Whenever there is diagnosticity, however, there is information gain. A high prior probability tends to diminish the value of the information gain even if there is high diagnosticity. This simply reflects the fact that little more can be gained from a lineup identification when the perpetrator's identity is fairly certain to begin with (i.e., high prior probability). The _nonidentification_ counterparts for Equations 1, 2, and 3 in this article are fully presented elsewhere (Wells & Lindsay, 1980). Nonidentifications include the witness's selection of nonsuspects as well as the witness's rejection of all suspects. Nonidentifications are diagnostic of the innocence of the suspect under any set of conditions in which identifications are diagnostic of guilt. These two possible outcomes, identification and nonidentification, interact with the prior probability such that information gain is greatest when a nonidentification occurs in the face of high prior probability or identification occurs in the face of low prior probability.

**Ecological Parameters Should Not Be Confused With Lineup Diagnosticity**

Our analysis of lineup diagnosticity up to this point distinguishes between evidence obtained from the lineup test itself (lineup diagnosticity) and evidence obtained in other forms (e.g., through probing the witness's recall or finding the suspect in possession of stolen goods). Navon (1990) criticized Wells and Lindsay (1980) for not including these other forms of evidence in their definition of lineup diagnosticity. The primary form of other evidence that Navon argued should properly be part of lineup diagnosticity is _resemblance_. What Navon meant by resemblance is a similarity or agreement between the eyewitness's prelineup description of the culprit's physical characteristics and the physical characteristics of the suspect.
Navon (1990) called these other forms of evidence ecological parameters. Although Navon focused his analysis primarily on the resemblance between a suspect and the eyewitness's prior description, it is clear that ecological parameters could include any kind of data that make it likely that the suspect is in fact the culprit in question. Resemblance, therefore, could be treated quite broadly to mean such things as a match between a partial finger print and the suspect's prints (resemblance of physical attributes), the suspect's possession of an amount of money that is only a few dollars short of the stolen amount (resemblance of possessions), or the suspect's prior offense's matching the offense in question (resemblance of crime type). In effect, an ecological parameter represents other evidence that makes it likely that the suspect and the culprit are the same person. The parameters that Navon used in his examples involve some combination of an unusual characteristic (such as a facial scar) and circumstance (such as being the only person with a scar found in the area of the crime) so that, prior to conducting the lineup, the odds of the suspect and the culprit being the same person are extremely high. We fully agree that ecological parameters are necessary for calculating the probability that a culprit and a suspect are the same person. However, these ecological parameters should not be confused with the concept of lineup diagnosticity. As we illustrate in subsequent examples, ecological parameters can be set at such levels that even lineups that are totally nondiagnostic and useless in and of themselves can nevertheless yield high posterior probabilities that the suspect is the culprit.

Our first, and simplest, example involves a hypothetical lineup that we call a remote-viewing lineup, in which, just before conducting the lineup, we fly the eyewitness to Cincinnati and hold the lineup in Dallas. Even though the witness had a good view of the culprit at the time of the crime and gave an excellent description of the culprit, we have created a recognition task in which the witness cannot see the lineup. Thus, we restrict lineup performance to chance levels. We nevertheless ask the witness to attempt an identification. From the outset, it is clear that such a lineup is totally nondiagnostic of the suspect's guilt or innocence. It should not matter whether the eyewitness chooses lineup member Number 4, Number 6, or Number 1 or whether the eyewitness selects the suspect or selects a foil. According to Navon (1990), however, the lineup would still be diagnostic to the extent that the suspect matches the eyewitness's earlier description and was the only one in the area of the crime who matched that description. Again, we agree that the resemblance data and arrest-location data are relevant to the posterior and prior probabilities that the suspect and culprit are the same person, but the remote-viewing lineup itself is nondiagnostic because the witness is equally likely to choose an innocent or guilty suspect under these conditions.

We could just as easily use devices other than remote viewing to assure that the lineup itself has no diagnostic value. For example, suppose an eyewitness became completely blind just moments before viewing the lineup, or suppose we held the lineup in total darkness. Or, suppose that, instead of using the eyewitness who was actually at the crime scene and had described the scar, we use someone chosen randomly from the streets who was not an eyewitness at all. In each case, there is no possibility that the lineup session will have any true diagnostic value. Yet as long as there were ecological parameters indicating that the suspect and culprit were the same person, Navon would assign diagnostic value to the lineup test in proportion to those ecological parameters.

The point seems obvious; high posterior probabilities do not mean that a lineup test itself was diagnostic. Instead, there must be some change between the prior probability (i.e., without considering outcomes from the lineup) and posterior probability (i.e., after considering outcomes from the lineup) for the lineup to be diagnostic. In the case of the remote-viewing lineup, the prior probability is the same as the posterior probability—that is, \( p(S = C) - p(S = C|IDS) = 0 \). Such lack of information gain is a guaranteed outcome whenever the lineup itself is nondiagnostic.

Our examples are extreme, but they illustrate the problem with Navon's (1990) attempt to incorporate ecological parameters into the index of lineup diagnosticity. A lineup is a test of recognition memory, and its purpose is to find out something more than can be discerned from other evidence (such as the witness's previous description of the culprit). Although a detailed description of the culprit derived from the witness's verbal recall might be diagnostic of the suspect's guilt, an index of this would be called description diagnosticity or recall diagnosticity; it is not lineup diagnosticity. Similarly, although discovery of the suspect lurking around the scene of the crime or the suspect's being found in possession of stolen goods might be diagnostic of the suspect's guilt, an index of this would be a measure of the diagnosticity of circumstantial evidence. The presence or absence of diagnostic circumstantial evidence or a diagnostic verbal description does not properly belong in an index of lineup diagnosticity. A lineup is a test of recognition memory, and in our examples (of the remote-viewing lineup, the lineup with a blinded witness, the lineup held in a darkened room, and the lineup using nonwitnesses), recognition can occur only at chance levels. The fact that ecological parameters might make it highly likely that the suspect and culprit are the same person does not make these lineup tests diagnostic.

What Navon (1990) has done by collapsing ecological parameters (other evidence) into his conception of lineup diagnosticity is to make lineups appear to be informative even under conditions in which lineups have no true diagnostic value. Our point is analogous to a version of Kahneman and Tversky's (1973) taxicab problem, with which many readers may be already familiar. Assume that an eyewitness to a hit-and-run accident states that she saw a yellow cab hit a pedestrian. In the town, 90% of the cabs are yellow and 10% are gray. Exact lighting and distance conditions are reconstructed, and the eyewitness views yellow and gray cabs during a test conducted by trial attorneys. She is accurate only 50% of the time. In other words, the eyewitness confuses yellow cabs with gray cabs and gray cabs with yellow cabs on 50% of the tests. What is the posterior probability that she saw a yellow cab during the accident? The answer is 90% because, although her statement about the color of the cab is completely nondiagnostic (chance performance on the eyewitness test), other evidence (specifically, the baseline or prior probability) drives the posterior odds to a respectable level of certainty.

In the taxicab example, the eyewitness's identification cannot be labeled diagnostic. After all, the test proved that the
probability of the witness saying that the cab was yellow given that it was yellow was equal to the probability of saying that it was yellow given that it was gray—that is, $p(\text{say yellow} | \text{cab} = \text{yellow})/p(\text{say yellow} | \text{cab} \neq \text{yellow}) = 1$. However, the base rate for yellow cabs in this case is an ecological parameter. Thus, Navon (1990) would say that the eyewitness's identification of a yellow cab is in fact diagnostic because the posterior probability is so high. Again, we believe that it is imperative to keep separate the idea of posterior probability (which includes both ecological parameters and diagnosticity) from the diagnosticity of an identification per se.

Our point is not that lineups lack diagnostic value. We have used examples of nondiagnostic lineups in this article merely to make clear our argument that lineup diagnosticity must be considered separately from other evidence that is not a part of the test of recognition memory.

The Problem of Fixed Resemblance

We agree with Navon (1990) on one major point. Specifically, in an experiment, the choice of whom to use as the innocent-suspect lure in a target-absent lineup affects the overall value of the diagnosticity index obtained. However, the choice of an innocent suspect in an experiment is not as arbitrary as Navon suggested. Instead, innocent suspects are chosen because they match the general characteristics of the culprit as described by the witness(es). Thus, Navon's concern that experimenters can fix diagnosticity at a low level by selecting the twin brother of the perpetrator as the innocent suspect is true at one level, namely, if that is what an experimenter actually did. In other words, Navon's twin-brother example is appropriate only if experimenters are trying to find innocent-suspect substitutes who look like the confederate perpetrator. But the experimental requirement for an innocent suspect is merely that the innocent suspect be someone who matches the eyewitness's prelineup description. The rationale for this experimental requirement is clearly related to the ecology of actual police lineups. A person becomes a suspect in a crime in large part because that person resembles the eyewitness's description of the culprit. To the extent that the innocent-suspect lure in the target-absent lineup of an experiment is selected on this basis, such selections should not be considered arbitrary; indeed, this strategy seems quite representative of what happens in actual police investigations.

Consider some well-documented cases of mistaken identification: Father Pagano, a priest who was falsely identified as a bank robber (see Wrightsman, 1987); Robert Dillon, a freelance photographer who was falsely identified as a robber, kidnapper, and rapist (see Wells & Loftus, 1984); and Lenell Geter, an engineer falsely identified by several eyewitnesses as an armed robber (see Wrightsman, 1987). Would these men have been placed in lineups as suspects if they did not resemble the eyewitnesses' descriptions of the culprits? No. All were placed in lineups in large part because they resembled the descriptions. Thus, although Navon is correct in noting that an experimenter could obtain higher diagnosticity ratios if the innocent-suspect lure in a target-absent lineup did not resemble the culprit, such an experiment would not simulate the parameters that govern the process by which a person becomes a suspect in actual police investigations.

Our point about the fact that suspects commonly resemble the culprit (or they would not likely be suspects) has other implications for Navon's (1990) analysis. Specifically, the likelihood of a particular degree of resemblance between a suspect and the witness's description of the culprit cannot be calculated according to normal rules of chance probability. Instead, consideration must be given to the complex ways in which the witness's description shaped investigators' strategies for finding and selecting a suspect. The search for a suspect is selectively biased by police investigators' knowledge of the witness's description, and their suspicions are restricted primarily toward those who fit that description.

In effect, Navon's (1990) main argument is that researchers cannot fix resemblance between the culprit and the innocent suspect at some arbitrary level in an experiment and still claim to report meaningful experimental results. We have noted that these fixed levels in experiments are not as arbitrary as he portrayed them. But even if they were totally arbitrary, Navon's fixed-levels argument misses the point of why the diagnosticity ratio was developed and how it is used. It was developed to compare diagnosticity between experimental conditions. In an experiment, most variables are fixed at some level while others are varied systematically. In an eyewitness experiment, fixed variables might include duration of exposure to the culprit, lighting conditions, delay from witnessed event to the lineup test, and resemblance between the culprit-actor and the innocent substitute. Even if these variables are fixed at an arbitrary level, the effects of variables that are systematically manipulated can be inferred by measuring diagnosticity across conditions. Thus, having a fixed level of resemblance does not explain, for example, why sequential lineups produced greater diagnosticity than did simultaneous lineups when the same level of resemblance was used for both types of lineup (Lindsay & Wells, 1985). Thus, Navon's (1990) question, "Is there anything that can be safely concluded from these [quantitative diagnosticity ratio] studies beyond the not-very-surprising moral that false identification is possible?" (pp. 506–507), can clearly be answered yes. Using Lindsay and Well's (1985) research as but one of dozens of possible examples, we conclude that false identifications are more likely in simultaneous lineup procedures than in sequential lineup procedures.

One more point about Navon's (1990) treatment of the notion of resemblance must be made. We disagree with Navon's assertion that "the information about resemblance as unfolded in the judicial process is embodied in the lineup data proper [in that the court usually does not have any independent knowledge about resemblance]" (p. 509). Navon is claiming that the court considers only the lineup outcome (i.e., eyewitness-identified suspect) and does not also consider the resemblance between the witness's prelineup description and the physical appearance of the accused. This then becomes part of Navon's rationale for collapsing the evidentiary value of the prelineup description into the lineup diagnosticity index. We do not know if this assertion is true of Israeli courts, but American and Canadian courts dwell heavily on the resemblance between a witness's previous description and characteristics of the accused. In fact, such resemblance constitutes one of the five criteria set forth by the U.S. Supreme Court in the case of Neil v Biggers (1972) for evaluating the likelihood that the accused is
the culprit (see Wells & Murray, 1983). Police are routinely re-
quired, as is the eyewitness, to provide the court with the pre-
lineup description of the culprit given by the witness to the 
police. Much is made of these descriptions in court. Thus, 
judges and jurors have both the lineup-identification evidence 
and the resemblance evidence (as well as other evidence) at 
their disposal. Resemblance evidence should not be collapsed 
into the concept of lineup diagnosticity any more than should other 
evidence (e.g., fingerprints).

The Role of Distractors

Thus far, our analysis has focused only on the issue of resem-
bhance between the suspect and the culprit, but neither our 
analysis nor Navon's (1990) can be complete without articulat-
ning the role played by distractors (or foils, or what police often 
call fillers). The primary purpose of distractors is to ensure that 
yany positive identification of the suspect is based on recogni-
tion memory rather than recall. Information from the eye-wit-
ness's recall has already been obtained prior to the lineup (e.g., 
"it was a white male of average height with curly dark hair and a 
moustache"). The recognition (lineup) task is conducted be-
cause verbal recall (prelineup description of the culprit) is not 
sufficient to establish a case that the suspect and culprit are 
the same person. Presumably, the lineup (recognition task) will tell 
investigators something more than they already know from the 
prelineup description (recall task). This is why the distractors in 
the lineup should match the witness's prior description (recall) 
of the culprit. If the culprit was described as a white male with 
dark curly hair, and this description applied only to the suspect, 
the witness need not use any information beyond the prior 
recall to figure out which person to choose. Accordingly, the 
best test of whether a lineup has good distractors is to give 
nonwitnesses the witness's description of the culprit and to ask 
them to choose which lineup member is the suspect (Doob & 
Kirshenbaum, 1973; Malpass & Devine, 1983; Wells & Luus, 
1990). Choices of the suspect by nonwitnesses under these con-
ditions should not exceed chance.

For two reasons, it is important to consider the role of dis-
tractors in the context of how to properly construe lineup dia-
nosticity. First, it has been shown that eyewitness identifica-
tions of an innocent suspect increase (and diagnosticity de-
creases) as a function of making the distractors look less like the 
culprit (Lindsay & Wells, 1980). This leads to the conclusion 
that eyewitnesses are making relative judgments (Wells, 1984). 
In other words, false identifications cannot be attributed 
merely to a close absolute resemblance between the innocent 
suspect and the culprit. False identifications also can occur 
when the suspect only moderately resembles the culprit but the 
distractors do not resemble the culprit at all, so that the inno-
cent suspect has a good resemblance to the culprit relative to the 
distractors' resemblance to the culprit.

The second reason it is important to consider the role of dis-
tractors in the understanding of diagnosticity is that there 
has been some confusion about how far investigators should go 
In trying to select distractors who resemble the suspect. On the 
one hand, distractors ought to be similar enough to the suspect 
to be viable choices for the witness; on the other hand, if they 
were too similar to the suspect they would be clones. One re-
viewer suggested that the relationship must be curvilinear, per-
haps an inverted U-shaped function. Elsewhere (Luus & Wells, 
in press), we have addressed this issue in detail. The answer is 
actually quite simple once the premise on which the seemingly 
paradoxical argument is based is rejected. Specifically, what 
must be rejected is the idea that one should choose distractors 
on the basis of resemblance to the suspect. Instead, distractors 
who resemble the eyewitness's prelineup description of the cul-
prit should be chosen. In other words, the proper strategy is to 
select distractors who match the description, not to select dis-
tractors who look like the suspect. This distinction is of para-
mount importance. Attempts to make distractors look like the 
suspect serve merely to make the task of recognition difficult 
or, at the extreme (e.g., clones), impossible. Distinctive features 
(heterogeneity of test set) are necessary for the operation of 
recognition memory (Gibson, 1969); common features (homo-
genivity of test set) should exist only for features that were part of 
the earlier recall. When distractors are chosen to match the 
prrior description and nonrecalled features are left to vary 
across lineup members, the requirements of fairness to the ac-
cused are met while reliable cues to perceptual discrimination, 
which are necessary for recognition, are also preserved.

A more detailed analysis of how the selection of distractors 
affects diagnosticity, as well as proposed solutions to unique 
problems (e.g., suspect doesn't match description), are presented 
elsewhere (Luus & Wells, in press). For current purposes it is 
sufficient to note that distractors should match the witness's 
description of the culprit (to hold down false-identification 
rates when the suspect is not the culprit) but that distractors 
should not resemble the suspect beyond the level of specificity 
contained in the witness's prior description (lest witnesses be 
unable to discriminate between the suspect and distractors 
when the suspect is the culprit).

We should note that Navon (1990) was careful in his treat-
ment of the role of distractors and that we have no clear dis-
agreement with him on these points. For example, when Navon 
claimed that "the conclusion must be that the suspect, if identi-
fied, looks very much like the perpetrator" (p. 508), he was 
worried to place the condition that this would be true only if the 
distractors were chosen after an elaborate search for people 
who would match the culprit's presumed appearance. Thus, the 
primary purpose of this section on the selection of distractors is 
to further clarify the role of distractors rather than to articulate 
a disagreement with Navon (1990).

Conclusion

The diagnosticity of a lineup must be expressed as a statisti-
cal likelihood associated with the conditional probability that 
the eyewitness would decide to identify or not identify a suspect 
given that the suspect is or is not the true culprit. Evidence of 
other types, including resemblance between the witness's pre-
lineup description of the culprit and the characteristics of the 
suspect, should not be confused with the diagnosticity of the 
lineup task itself. These other types of evidence affect the poste-
rior probability that the suspect is the culprit but are not, in and 
of themselves, aspects of lineup diagnosticity. Our hypothetical 
example of a remote-viewing lineup, in which the eyewitness is 
restricted to chance performance on the identification task,
clearly illustrates a completely nondiagnostic lineup that is associated nevertheless with a high posterior probability of guilt. A lineup test is conducted to reveal information in the eyewitness's recognition memory that was not available in recall (verbal description) memory. Resemblance information obtained from the witness's prelineup verbal description of the culprit may have significant diagnostic value in many cases, but neither the description nor any other extralineup evidence should be confused with the diagnosticity of the lineup itself.

Although the obtained value of diagnosticity in an experiment depends on the experimenter's choice of an innocent-suspect substitute for the culprit in a culprit-absent lineup, reasonably high levels of resemblance between the innocent suspect and the culprit are justified on ecological grounds. Specifically, police most often choose suspects because those suspects match the eyewitness's description of the culprit. Moreover, obtained levels of diagnosticity depend not only on resemblance between the innocent suspect and the culprit but also on the selection of distractors among whom the suspect is embedded.

Navon's (1990) attempt to give formal treatment to the concept of resemblance is laudable, and the more general treatment of ecological parameters is useful. Although we maintain that the diagnosticity index developed by Wells and Lindsay (1980) is the appropriate measure of lineup diagnosticity, Navon's analysis forces us to more carefully consider the rules that should govern the selection of the innocent-suspect substitutes in the target-absent lineups of eyewitness experiments. In addition, although ecological parameters, such as resemblance, should not be confused with lineup diagnosticity, we encourage further work showing how ecological parameters can be combined with lineup diagnosticity ratios to yield posterior probabilities of guilt.

References


1991 APA Convention "Call for Programs"

The "Call for Programs" for the 1991 APA annual convention will be included in the October issue of the APA Monitor. The 1991 convention will be held in San Francisco, California, from August 16 through August 20. Deadline for submission of program and presentation proposals is December 14, 1990. This earlier deadline is required because many university and college campuses will close for the holidays in mid-December and because the convention is in mid-August. Additional copies of the "Call" will be available from the APA Convention Office in October.