Public Interest Law Reporter

Volume 11
Issue 2 *Summer 2006 Symposium Issue*

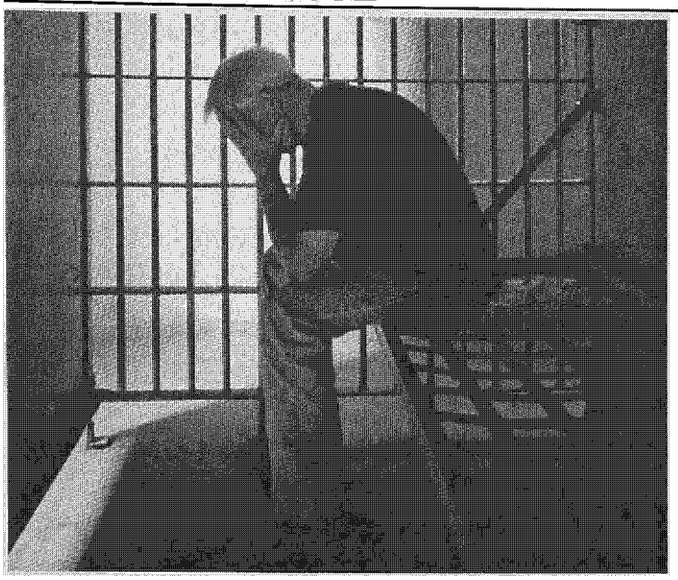
Article 4

2006

Notes on the Illinois Pilot Program on Sequential Double-Blind Identification Procedures

Roy L. Malpass
Prof. Dept. of Psychology and the Criminal Justice Program, University of Texas, El Paso

SYMPOSIUM ISSUE



Malpass: The Illinois Pilot Program study shows that simultaneous lineups lead to more suspect identifications.

Notes on the Illinois Pilot Program on Sequential Double-Blind Identification Procedures

Roy S. Malpass[1]

As a result of recommendations made by the Illinois Governor's Commission on Capital Punishment, the Illinois Legislature charged the Illinois State Police with conducting a pilot program to evaluate the effectiveness of the sequential, double blind identification procedure in the field. Sheri H. Mecklenburg was appointed Director of the Illinois Pilot Program and undertook to design the Illinois Pilot Program, seeking comments and approval from eyewitness researchers in the process. Reporting forms were developed, police personnel were given training on the new procedures and procedures were developed for deciding which lineups would be presented according to traditional or new procedures. These matters and much more are detailed in the *Report to the Legislature of the State of Illinois: the Illinois Pilot Program on Sequential Double-Blind Identification Procedures* ("the Report").[2]

The author was approached by Mecklenburg, asking for our participation as analysts. I agreed to act in this capacity with the assistance of Laura A. Zimmerman, Stephen J. Ross, Lisa D. Topp, Vanessa Uribe, Dannette De Leon, Sarah Ramirez and Jessica Belisle, all members of the Eyewitness Identification Research Laboratory at the University of Texas at El Paso. Periodically we received sets of case reports from the three participating jurisdictions. We were given a free hand to structure our analysis in our own way. We constructed the code book[3] and implemented an analysis.[4] While we contributed our analysis of the data, we did not participate in writing the Report. [5]

Professor Ebbesen of the University of California, San Diego also agreed to serve as an analyst for the Pilot Program. Professor Ebbesen and his research group received the same case reports and constructed their own way of coding and analyzing the data. Professor Ebbesen's group and the Eyewitness Lab at University of Texas at El Paso reached the same conclusions, although our conventions for coding the raw field reports for analysis differed in some respects, leading to somewhat different numbers. Ebbesen and Malpass never discussed anything about their task - had no conversation whatever – until they met during the Symposium held at the Loyola University of Chicago Law School on April 21, 2006.

Design

The study was designed to determine whether or not a new eyewitness identification procedure (a particular variant of double-blind sequential lineup) is superior to the simultaneous lineup procedure in current use. The specifics of implementation of the design are discussed in the Report.[6] This study was not the extension of an academic research program and was not undertaken to untangle theoretical issues.

Results

The major results are displayed in Table 1, for the total sample, aggregating the results across the three jurisdictions. There are three outcomes possible in this study: suspect identifications, filler identifications and non-identifications. It is important to note that suspect identifications cannot be interpreted as either correct or false identifications, and non-identifications can not be interpreted as missing the offender or as rejecting a lineup that does or does not contain the actual offender. It is not known, for any lineup in this study, whether the suspect in the lineup is the actual offender. This can be known in laboratory studies, but not in the field without a considerable amount of additional research. We will return to this matter below.

The major results are these:

(**Malpass**, *continued on page 6*)

(**Malpass**, *continued from page 5*)

Table 1. Effects of Simultaneous and Sequential Lineups on Three Outcome Variables.

n=548	Simultaneous(319)	Sequential(229)
Suspect ID	59.9%	45%
Filler ID	2.8%	9.2%
No ID	37.6%	47.2%

- Witnesses who viewed a simultaneous lineup identified the suspect more often than those witnesses who viewed a sequential lineup (suspect identification rates of 59.9 percent and 45 percent respectively).
- Witnesses who viewed a simultaneous lineup chose a filler less often than those who viewed a sequential lineup (filler identification rates of 2.8 percent and 9.2 percent respectively).
- Witnesses who viewed a simultaneous lineup were less likely to choose no one than were those who viewed a sequential lineup (no identification rates of 37.6 percent and 47.2 percent respectively).

As noted above, these results cannot be interpreted directly as accurate or erroneous responses. Nonetheless, assuming that the increase in non-identifications for sequential lineups compared with simultaneous lineups reflects a proportionate increase in correct rejections in a culprit-absent lineup, and that the decrease in suspect identifications from simultaneous to sequential lineups is proportionate with a decrease in correct identifications in a culprit-present lineup, then the sequential advantage for culprit-absent lineups will be more than offset by the sequential disadvantage for culprit-present lineups. This comparison is worsened if one considers that culprit-present lineups are probably the more frequent. It seems implausible that on the average law enforcement does not do better than a .5 probability of getting the right person in the lineup.

Reasonable people can begin with different assumptions, however. The proportions of suspect identifications contributed to correct and false identifications can be argued, and various probabilities that the culprit is actually in the lineup can be entertained.

Stability of the findings across jurisdictions is a matter of interest from the perspective of application. These findings are displayed in Table 2.

- For *simultaneous lineups,* suspect identifications vary over a range of 10.7 points, from 57.0 to 67.7, and non-identifications vary over a range of 10.1 points, from 32.3 to 42.4.
- For *sequential lineups,* suspect identifications vary over a range of 42.7 points, from 25.9 to 68.6, and non-identifications vary over a range of 34.4 points, from 28.6 to 63.0
- The *difference* between simultaneous and sequential lineups also varies considerably, from + 41.8 to -7.3.

Sequential lineups appear to be more sensitive to differences in jurisdiction / location / context / background conditions, although it is not clear exactly what conditions these might be.

(**Malpass**, *continued on page 7*)

Table 2: Effects of Simultaneous and Sequential Lineups on Three Outcome Variables, by Jurisdiction.

	Chicago		Evanston		Joliet	
	Sim.	Seq.	Sim.	Seq.	Sim.	Seq.
Suspect ID	57	43.1	67.7	25.9	61.3	68.6
Filler ID	0.7	10.2	0	11.1	5.8	2.9
No ID	42.4	48.5	32.3	63	33.6	28.6

(**Malpass**, *continued from page 6*)

Discussion and Interpretation

The Illinois Pilot Program[7] is a landmark eyewitness identification study, even among field studies:

- The Illinois Pilot Program makes a direct comparison between the traditional, intact simulattaneous lineup procedures and a version of double-blind sequential lineups.
- It uses multiple jurisdictions.
- It contains more than 700 individual identifications.
- The criminal cases cover the entire range of crimes committed during the period of the study.

Field data are inherently noisy. Field studies are known for variability, and this is why laboratory studies are sometimes called "controlled" studies by way of contrast. There are many investigators, many contexts and many jurisdictions, and these lead to many variations in implementation. In some respects the noisy background may obscure relationships in the data that might be found under otherwise more controlled conditions. On the other hand, strong effects showing through the background variation would be robust. Additionally, confounding factors outside of the research design proposed as having an effect on study outcomes would also have to be strong (substantial empirical effect size), consistent and detectable to be taken seriously. Further, the noisy study environment is a valid reflection of the environment of application because it IS the environment of application.

It may take some time to frame new questions arising out of our attempts to interpret these results. The questions will lead to new and more informative research - certainly in the laboratory - and hopefully in field studies carried out in association with law enforcement. This is a very rich intellectual welfare program for researchers.

Clearly the problems with eyewitness identifications have not been solved, and as Barry Scheck pointed out in his remarks on the eve of the Loyola Conference, we should move forward to develop other areas of lineup reform while we clarify the contribution of sequential lineup presentation. Working relationships between law enforcement and academic researchers should be strengthened to study a range of identification questions.[8]

It has to be said that not much good news will be found in the Illinois Pilot Program for those who advocate sequential lineups to law enforcement and governmental organizations.

Transposition from field categories to the categories of laboratory studies. An important thing to note in the interpretive process is that the three outcome categories of the field study cannot easily be disaggregated into the six (at least) categories of laboratory studies.

It would be possible to disaggregate the field study categories under two conditions: If we make assumptions about (1) the proportions of each field category to be distributed to each of the two cognate lab categories, and (2) the proportion of those figures to be considered, reflecting the *a priori* probability of the suspect being the perpetrator, or not.

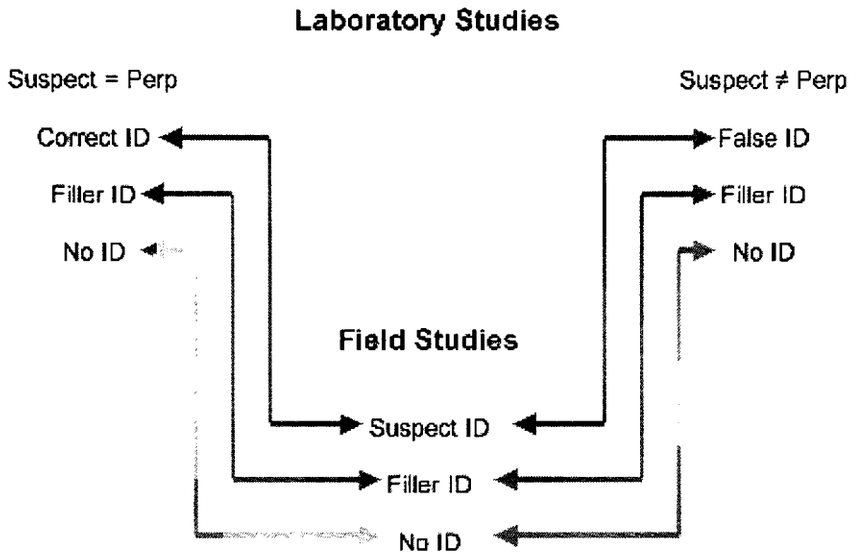
Filler Identifications. Filler identifications are the only responses that have an apparent clear interpretation. The only thing that can be said, really, is that sequential lineups attract a non-trivially greater frequency of filler identifications, overall. This is descriptively true for all three jurisdictions, but statistically reliable for only two. The absolute percentage of filler identifications is small. There does not appear to me to be a theoretically solid way to use this result to make inferences about the interpretation of the real interest of this study: the accuracy of suspect identifications and non-identifications.

Double-blind simultaneous lineups as a comparison. The purpose of the study, as stated above, was to determine whether or not a new eyewitness identification procedure (a particular variant of double-

(**Malpass**, *continued on page 8*)

(**Malpass**, *continued from page 7*)

Figure 1. Transitions from Field Outcome Categories to Laboratory Outcome Categories.[1]



blind sequential lineup) is superior to the simultaneous lineup procedure in current use. However, there are some nuances to the question.

First, to evaluate the effects of a change in practice against existing practice is a completely appropriate research strategy. To have changed the existing simultaneous lineup practice for purposes of comparison against a new practice would have given no guidance to law enforcement at the end of the study because it would have had little to do with their current practice. This was a simple and straightforward study design for a straightforward question.

Second, perhaps there is an interest in the ultimate question of whether some version of a double-blind sequential presentation is superior to some version of a double-blind simultaneous presentation. That is a different question, and it is not the one asked in the Illinois Pilot Program. To answer that question would require different research design strategies. But more than that, it would place law enforcement agencies in Illinois in the position of doing research to answer our (academic researchers) questions rather than their own. I would like it very much if I could establish the Field Study Unit of the Eyewitness Identification Research Laboratory within the law enforcement community of the State of Illinois, but this project could not serve in that capacity.

The comparison between blind sequential and non-blind simultaneous lineups is confounded. A confounded comparison exists when there is at least one difference between two research conditions in addition to the difference introduced for study. In the Illinois Pilot Program, sequential lineups are uniquely blind while simultaneous lineups are uniquely not blind. So there are two factors (at least) differentiating them: mode of presentation and blind vs. not blind. The question is to what degree the blind vs. not blind difference hinders interpretation of the presentation mode difference. On the one hand, confounded comparisons are the rule for field studies, and when the very powerful research strategies possible with true experiments are not present, investigators are left with trying to reason their way through the interpretation of observed differences. Un-confounding requires that the important factors are known and either controlled or measured for post-hoc analysis. There may be many confounding factors in this study with effects that cannot be anticipated or estimated. It was not possible to monitor the administration of even a

(**Malpass**, *continued on page* 39)

The Illinois Pilot Program data may show evidence of investigator influence and eyewitness bias, including racial bias.

In Response to the Illinois Pilot Program on Simultaneous v. Sequential Lineups[1]

Ebbe B. Ebbesen[2] and Kristin M. Finklea

The true level of eyewitness accuracy in the legal system has been debated by researchers and laypersons alike. Specifically, inaccurate eyewitness identification is thought, by some, to be the primary cause of false convictions.[3] The goal of the legal system is to maximize the number of convicted guilty suspects while minimizing (and, in theory, eliminating) the number of convicted innocent suspects. To minimize the rate of mistaken identifications, researchers have suggested that eyewitness identification evidence be collected using modified lineup procedures. Two of the major changes in protocol include: 1) using blind lineup administrators and 2) displaying the lineup photographs sequentially.[4] In order to effect policy change in a scientifically reasonable manner, researchers should compare a proposed new policy against the established policy already in place. Without this comparison, we will never know whether the new policy is any better than the old. Such a comparison is no different than that used in the medical field to evaluate the effectiveness of a new treatment against the current method. The Illinois Pilot Program[5] was designed utilizing this philosophy to compare the proposed sequential double-blind lineup procedure against the traditional simultaneous (non-blind) lineup procedure.

To date, findings from laboratory research suggest that witnesses make fewer selections from lineups presented sequentially than they make from lineups presented simultaneously.[6] As a consequence, both fillers and "suspects" are identified less often in sequential compared to simultaneous lineups. Based on these findings, some researchers have advocated that policy makers adopt sequential over simultaneous lineups to minimize the potential for eyewitnesses to mistakenly identify innocent suspects. Unfortunately, research has not adequately addressed whether adopting a sequential procedure in practice would also reduce the rate at which guilty suspects are identified.[7] If the results of laboratory studies can be generalized to witness responses in actual criminal cases, both filler and suspect choice rates should decrease as a result of moving from a simultaneous to a sequential lineup procedure.

Summary of Major Findings from the Pilot Program

To assess these predictions, we can look at the results from the Illinois Pilot Program[8] (Table 1). The entire sample contained a total of 367 different cases, in which researchers identified a total of 741 lineups. Of these, a total of 521 *unique* lineups were identified, as some investigators presented the same suspect in the same position with the same fillers to more than one witness. Across three jurisdictions investigators conducted a total of 366 standard simultaneous, single-suspect lineups and a total of 271 sequential, double-blind, single-suspect lineups. Witness/victims chose the suspect in 244 (or 67 percent) of all of the simultaneous lineups and in 154 (or 57 percent) of the sequential/blind lineups. Witness/victims chose fillers a total of 8 times (or 2.2 percent) when viewing the simultaneous lineups and 18 times (or 6.6 percent) when viewing sequential lineups (the difference in choice rates between the simultaneous and sequential lineup procedures was statistically significant). Including multiple suspect lineups in the analysis did not change the basic pattern of results. Thus, overall, the suspect choice rate was higher and the filler choice rate was lower for the simultaneous than sequential lineup procedure.

The tendency for witnesses to choose suspects more frequently and fillers less frequently given a

(Ebbesen/Finklea, *continued on page 10***)**

(**Ebbesen/Finklea**, *continued from page 9*)

Table 1: Number and Percent of Suspect and Filler Choices for Known Single Suspect Lineups as a Function of Lineup Procedure

Lineup Procedure	Number of Choices			Percent of Total Choices		
	Suspects	Fillers	No Choice	Suspect	Filler	No Choice
Simultaneous	244	8	114	66.7	2.2	31.1
Sequential	154	18	99	56.8	6.6	36.5

simultaneous compared to a sequential lineup presentation was replicated for two of the three jurisdictions, Chicago and Evanston. In Chicago, suspects were chosen 64 percent of the time with simultaneous lineups and 49.5 percent of the time with sequential lineups. In addition, no fillers were chosen with simultaneous lineups, but with sequential lineups 6.3 percent of the choices were of fillers. Similarly, in Evanston, suspects were chosen 72.1 percent of the time with simultaneous lineups and 44.2 percent of the time with sequential lineups. Again, no fillers were chosen with simultaneous lineups, but 13.5 percent of the choices in sequential lineups were fillers. In Joliet, the pattern was slightly different. The suspect choice rates were 61.7 percent for simultaneous and 69.4 percent for sequential lineups, with filler choice rates at 4.4 percent and 6.5 percent, respectively. In summary, across all three jurisdictions, the known error rate (i.e. foil identifications) was higher with sequential than simultaneous lineups, and suspect choice rates were higher in simultaneous than sequential lineups for two out of three of the jurisdictions. It is clear that the results from this field program are in direct contradiction to the generalizations from laboratory research findings.

Alternative Explanations for the Findings

Two different classes of explanations might account for the differences in the pattern of results seen in the field project and that in laboratory simulations. We can focus on the differences in how lineups are constructed in laboratory research and how they are typically constructed in the legal system. Alternatively, we can focus on the fact that the two lineup procedures differed not only in terms of the presentation of lineup "alternatives" (simultaneously v. sequentially) but also in terms of whether the investigators conducting the lineups knew the suspect's location in the lineup.

Lineup Construction

The discrepancy between the Illinois Pilot Program[9] results and the laboratory findings can be explained by considering that the characteristics of the lineups employed in the field might have differed from the characteristics of the lineups employed in the laboratory. First, the base rate of guilty suspects appearing in lineups might have been higher in the field compared to laboratory studies. In order to accurately assess the generalizability of laboratory research to eyewitness identifications in actual criminal cases, researchers should evaluate the true rate of target present ("TP") and target absent ("TA") lineups.[10] In the typical laboratory study, there are equal proportions, 50:50, of TP and TA lineups. However, researchers have yet to determine if this proportion is representative of the rate at which guilty and innocent suspects appear in actual lineups. The burden of suspect choice errors is often overlooked in applied research, assuming that suspect equals culprit. The most problematic error an eyewitness can make is that of selecting an innocent suspect (not that of selecting a known-innocent foil) from a lineup. Some have concluded that in the laboratory, the largest difference in identification outcomes across the two lineup procedures occurs in the identification of "innocent" compared to "guilty" suspects.[11] That is, although both guilty and innocent suspects are chosen more often in simultaneous lineups, the difference found in innocent suspect identifications is larger.[12] Therefore, some have suggested that switching to a sequential lineup will reduce innocent suspect choices more than guilty

(**Ebbesen/Finklea**, *continued on page 11*)

(**Ebbesen/Finklea**, *continued from page 10*)

suspect choices. However, if guilty suspects are present in real world lineups more often than are innocent suspects, using a sequential lineup procedure will suppress the hit rate more than the false alarm rate in actual cases. As such, the higher rate of suspect choices in simultaneous compared to sequential lineups conducted in Illinois may have resulted from the fact that the large majority of suspects presented to witnesses for identification were actually guilty culprits rather than innocent suspects.

Second, the relatively low rate of filler choices in actual lineups compared to laboratory studies might be explained by the difference in the way fillers are selected for actual lineups compared to lineups constructed in a laboratory. In a typical controlled experiment, the guilty suspect is removed and replaced with an innocent look-alike. Foils, consequently, remain high in similarity to both the guilty (TP) and innocent (TA) suspects. In the real world, however, an innocent suspect who is apprehended may look nothing like the actual culprit. Hence, when foils are selected for the lineup based on their degree of similarity to the innocent suspect, these foils will have a high degree of similarity to the innocent suspect, but a low degree of similarity to the actual culprit. As a result, a witness may be less likely to select a foil from such a TA lineup than a lineup containing the culprit. This could explain why the filler choice rates are so much lower in the Illinois Pilot Program[13] than in laboratory studies.

Investigator Bias

i) Conditions that Might Suppress or Enhance Investigator Bias

Based on the observed difference in choice rates between the data from the Illinois Pilot Program[14] and the predictions grounded in laboratory research, we wanted to examine whether specific variables that might make it easier or harder for investigators to influence witness choices (in the non-blind simultaneous lineup) had the predicted effects on choice rates. One such variable is the relationship that existed between the witness and the suspect prior to the crime. One might expect witnesses who knew the suspect prior to the crime would be more difficult for investigators to influence than witnesses who were attempting to identify a stranger. Therefore, not only should strangers be identified less often than acquaintances (regardless of lineup procedure), but the investigators conducting simultaneous lineups should be able to influence witnesses to pick the suspect more often and the fillers less often when the suspect and witness were strangers. This should not occur with blind, sequential lineups.

We examined the prior relationship predictions in photo lineups, as most live lineups contained stranger relationships. With simultaneous photo lineups, 90.3 percent of the witnesses chose the suspect when a prior relationship existed but only 53.6 percent chose the suspect when they were strangers. With sequential photo lineups, these percentages were 76.3 percent and 43.8 percent respectively. Thus, the difference in choice rates between sequential (blind and no influence) and simultaneous (with influence) lineups was larger for the acquaintance choices (76.3 percent v. 90.3 percent) than for the stranger choices (43.8 percent v. 53.6 percent), exactly opposite to prediction.

[A]cross all three jurisdictions, the known error rate (i.e. foil identifications) was higher with sequential than simultaneous lineups, and suspect choice rates were higher in simultaneous than sequential lineups for two out of three of the jurisdictions. It is clear that the results from this field program are in direct contradiction to the generalizations from laboratory research findings.

Considering filler choices from simultaneous photo lineups, none of the witnesses or victims chose a filler when a prior relationship existed; but, 1.3 percent of the witness/victims chose a filler when they were strangers. With sequential photo lineups, these

(**Ebbesen/Finklea**, *continued on page 12*)

(**Ebbesen/Finklea**, *continued from page 11*)

percentages were 2.6 percent and 9.4 percent respectively. As expected, fillers were chosen more often when a "stranger" relationship existed in both lineup types, but were investigators who conducted simultaneous lineups better able to influence witnesses to avoid choosing fillers in stranger lineups (compared to acquaintance lineups) than were investigators who conducted sequential lineups? The shift from sequential to simultaneous lineups caused a small decrease (from 2.6 percent to 0) in filler choices when the suspect was an acquaintance but a bigger decrease (from 9.4 percent to 1.3 percent) when the suspect was a stranger. These results are consistent with the investigator bias explanation.

Consequently, although the filler choice rates for strangers and acquaintances might be explained by investigator bias, the suspect choice rates are inconsistent with this hypothesis. Since the investigator influence hypothesis assumes that investigators would be simultaneously directing witnesses away from fillers and towards suspects, the pattern of results seems inconsistent with the investigator bias explanation.

We also analyzed whether the status of witnesses (as a victim of the criminal act or simply a witness to the action) had any effect of choice rates. Because the consequences of making a choice are different for the two types of witnesses, we might expect those who were victims of the crime to be more likely to make a selection purely for the sake of conviction. Investigators could take advantage of this tendency when they know who the suspect is in the lineup, an argument for a potential benefit of instituting a (sequential) double-blind procedure. If so, we may expect victims to be less likely to select foils and more likely to choose suspects, but only when presented with the simultaneous procedure – a procedure in which the investigators knew who the suspect was. Results of the analysis are inconsistent with this view. Given a simultaneous lineup procedure, victims and witnesses selected suspects at equal rates (63.49 percent and 67.69 percent, respectively) and chose fillers at equal rates (2.07 percent, 1.83 percent). The same pattern held in sequential lineups for victims and witnesses selecting suspects (51.17 percent, 53.96 percent) and fillers (8.72 percent, 6.47 percent). In essence, the effect of lineup procedure on choice rates was unchanged for victims and witnesses.

ii) Witness Confidence

The double-blind procedure was included in the Illinois Pilot Program,[15] in part, because researchers have suggested that without it, there is a possibility that investigators may consciously or inadvertently influence witness selections from a lineup.[16] Were this to happen, we might expect those witnesses who agreed with the investigator to be more confident that they were right in selecting the suspect (the same person the investigator believed was guilty) and less confident when they disagreed with the investigator and selected a filler. To examine this notion, we first analyzed suspect and filler choice rates as a function of the confidence that witnesses expressed in their identifications (Table 2). Confidence could be assessed for 31 percent of the simultaneous lineups and 63 percent of the sequential lineups based on the investigators' written assessments of witness confidence at the time the lineup identification was conducted. High and moderate confidence choices were more associated with higher suspect choice rates than were low confidence choices. Moderate and low confidence choices were associated

(**Ebbesen/Finklea**, *continued on page 27*)

Table 2. Number and Percent of Suspect and Filler Choices as a Function of Expressed Confidence for all Lineups with Known Suspect Structure

Confidence	Number of Choices			Percent of Choices	
	Suspect	Filler	No Choice	Suspect	Filler
High	186	7	64	72.4	2.7
Moderate	32	9	2	74.4	20.9
Low	7	4	18	24.1	13.8
Not Known	204	12	174	52.3	3.1

SYMPOSIUM ISSUE

The Street, The Lab, The Courtroom, The Meeting Room

James M. Doyle[1], Steven Penrod, Ph.D., Margaret Bull Kovera, Ph.D. and Jennifer Dysart, Ph.D.

The Mecklenburg Report[2] documenting the results of the Illinois Pilot Program on Sequential Double-Blind Identification Procedures will frustrate the most violent partisans on both sides of the debate over the future of eyewitness investigations.

Sadly, the Mecklenburg Report will also disappoint a broad audience of practitioners in the middle that hoped for guidance—for something to do (or avoid doing) to minimize the number of imprisoned innocents and untouched criminals that the DNA exoneration cases warn us eyewitness memory can produce. The Mecklenburg Report represents a taxing effort, its author and the participating officers and departments deserve our thanks, but the Report does not succeed in combining the perspectives of the street investigator, the laboratory scientist and the courtroom litigator into a working synthesis. In the Report's aftermath, it is clearer than ever that all three perspectives, their potentials and their limitations must be recognized before there can be a basis for action informed by science.

If the Mecklenburg Report convinces the criminal justice system's practitioners—investigators, prosecutors, defenders and judges—that they cannot wait around for legislatures to act, but must get themselves to the table together, engage the scientists, and work to find answers, then it can be a positive contribution. But until that happens, the Mecklenburg Report will leave us not far from where we were when the National Institute of Justice issued its path-breaking *Eyewitness Evidence: A Guide for Law Enforcement*[3] ("the Guide") in 1999. We still have a substantial body of laboratory results arguing for the procedures that the Guide identified as good (pre-line-up instructions), or good but not preferred (sequential lineup display), or simply potential (double-blind) options. We now have a number of satisfied jurisdictions around the country (including New Jersey, Boston and Minneapolis) that have instituted double-blind, sequential routines with apparent success. Even so, the Guide did place the burden of proving the superiority of those procedures in operation on their advocates, and the Mecklenburg Report's numbers certainly do not lighten that burden. In fact, at least on the surface, the Report's numbers seem to tend in the opposite direction: against innovation. But if we look beneath the surface, we find that even if the numerical results noted in the Mecklenburg Report's field study had been *reversed* the Report *still* would not have proved the superiority of double-blind sequential procedures. The Mecklenburg Report reveals a study that simply was not set up to test under scientific control either double-blind, or sequential procedures. Nor did it test scientifically any differences between blind and not-blind simultaneous procedures.

These gaps are doubly unfortunate because five years from now we will not be handling eyewitness identifications in the same way that we handle them now. Our arrays of dog-eared mug shots and hastily improvised station house lineups are certain to be supplemented—and are almost certainly doomed—by a digital revolution that promises us quick, cheap, convenient and comprehensively documented identification procedures. We will have—some departments now have—photo lineup capability on laptops in squad cars. The capacity to present photo-arrays on Palm Pilots has already been studied in the labs. There soon will be many alternatives to dragging victims to the precinct house in the middle of the night and hiring line-up fillers from the homeless shelters in order to test witness memory.

But to say we will have new equipment doesn't tell us what we should do with it. What should we show the witnesses on our laptops? "Sequential" displays? How should we show it? With "double-blind" techniques? What is the best procedure for the future? The Mecklenburg Report does not really answer these questions; in fact, the study it recounts does not really ask them.

In hindsight it is clear that the Mecklenburg Report reveals a crippling misunderstanding at the heart of the field study it describes.

The Illinois Legislature issued a directive to pursue a specific goal: compare a traditional technique of eyewitness evidence gathering (the "simultaneous"

(**Doyle**, *continued on page 14*)

(**Doyle**, *continued from page 13*)

display of suspect and fillers by an officer who is aware of which lineup member is the suspect) with a proposed improvement (the "sequential" display of suspect and fillers by a "double-blind" administrator). But the Mecklenburg Report is pervaded by an unexplained determination to treat the Legislature's statement of a *goal* as if it dictated a *method*. The failure to acknowledge the distinction between goal and method affected not only the Report on the study, but the design of the study itself, and it imposed serious handicaps. The muddle of method and goal explains why, in the words of United States Attorney Patrick Fitzgerald, the study "raises more questions than it answers."[4]

In fairness to the Illinois Legislature, it did what it could to signal that the studies of eyewitness procedures that should be conducted were *not* the crude "traditional v. double-blind sequential" test the Mecklenburg Report describes. The Legislature sought an empirical answer to an empirical question by the use of study instruments: "[D]esigned to elicit information for comparative evaluation purposes, and . . . *consistent with objective scientific research methodology*."[5] An appropriate objective scientific research methodology exists, but the Mecklenburg Report shows plainly that the study it discusses stopped short of applying that methodology.

Four psychologists are mentioned prominently in the Mecklenburg Report. Two, (Dr. Nancy Steblay and Dr. Gary Wells) are bitterly critical of the report and allege that their participation is exaggerated by the Report's author; two (Dr. Ebbe Ebbesen and Dr. Roy Malpass) consider the Mecklenburg Report on the field study to be a valuable document. But *all four* of these scientists disclaim any responsibility for *designing* the study. In fact, the study, like the Report, is the product of a single hard-working lawyer for the Chicago Police without formal training in social science methods. The differences between legal and social science practice show. For example, the Report describes as "random" assignment methods which in the legal world might be accepted as meriting the term, but which no social scientist would recognize as true random assignment. The result of this absence of a science-based design is that the Mecklenburg Report forfeits lessons that a truly scientific approach might have taught. Because the requirements of scientific methodology were not imposed in the design of the study, we now know much less than we could.

To begin with, a comparison of the new "double-blind sequential" photo arrays with traditional "simultaneous, not-blind" lineups, which merely lays the two side-by-side, could never have been informative in any scientific way. To properly assess the "sequential" photo-arrays against "simultaneous" procedures either *both* "simultaneous" and "sequential" would have to be "not-blind," or *both* would have to be double-blind—only then could we gauge which factor was creating the effect we see. To properly weigh the impact of "double-blind" procedures *both* simultaneous and sequential procedures would have to be run in "double-blind" *and* "not-blind" conditions before the impact of "blindness" on investigations could be assessed. In other words, a fatal "confound" is built into the design of the Report, making it impossible as a matter of method, to retrieve authoritative answers to the question the Legislature posed.

The Mecklenburg Report speaks for many when it suggests that we expand our inquiries and address further questions. But, real improvements in justice system processes based on science will only occur if cops, prosecuors and defenders take responsibility for framing the right questions informed from the beginning by scientific advice.

Besides, treating the question posed by the Legislature as a methodological directive while ignoring the Legislature's wish that "scientific methodologies" govern the study hopelessly entangled the operational issues of what is feasible on the street or in the precinct with the reliability research issue of whether the new procedures are worth doing in the first place.

For operational purposes it was natural for the Report to use "suspect hits" as a proxy for "correct identifications." A radical decline in "suspect hits" in

(**Doyle**, *continued on page 15*)

SYMPOSIUM ISSUE

(**Doyle**, *continued from page 14*)

double-blind sequential procedures would indicate at least in a rough and ready way a very awkward disconnect between that particular identification technique and police field operations. But it is important to confine the "suspect hit" criteria to its operational significance. Obviously, if we were satisfied with every procedure that yields a 100 percent rate of "suspect hits" we would never have undertaken the enterprise in the first place; we would have simply agreed to regard the dozens of DNA exonerations (every one of them based on a "suspect hit" which seemed "corroborated") and the dozens of active criminals who escaped justice in those cases as an inevitable cost of doing business. "Suspect hits" can tell us important things about operations, but—even in the Hennepin County field study, where the results were radically different from those recited in two of the three Mecklenburg Report jurisdictions —they tell us very little about the reliability questions at the heart of the issue of procedural superiority.

Lab methods have their own limitations, and there is a danger of unintentionally imposing those limitations if we undertake "lab-like" studies in the field.

The specific limitation that concerns us here is not the worry that in the real world crime situation human memory operates in a qualitatively different way. There is no evidence for that fear. In studies pre-dating the Report, the rate of "filler ID's" in the lab and in the real world seemed to match up fairly closely. In the occasional hyper-realistic laboratory study, such as Dr. Charles Morgan's controlled study of special forces troops who were asked to identify their interrogators after a high stress interrogation (more than half identified a "filler" in conventional simultaneous arrays) the results, again, are consistent with both the more conventional lab setting experiments and with the scattered field results from the United States and the United Kingdom.[6] But even while we acknowledge that the lab studies and the field studies are examining the same processes of human memory, we have to remember that they do so in different contexts.

The Mecklenburg Report's most intriguing results are its account of a "zero" rate of filler identifications in two of three jurisdictions. These results are unique among existing studies in the lab and in the field. How did this happen? What does it mean? Unfortunately, the design of the study and the Mecklenburg Report's recounting of it leave open a quite simple and obvious explanation: the failure to account for a fundamental difference between lab life and street life.

When the lab scientists study the efficacy of an identification technique, the single "simultaneous" or "sequential" test they scrutinize is almost always the witness's *first* attempt at the identification of a stranger-perpetrator. This places the focus on the most influential (and therefore dangerous) encounter, but it does not automatically duplicate typical real-life practice; in real life a witness's live line-up performance is only one episode in the witness's career in the criminal justice system. In real life—and in the experience of the witnesses depicted in the Mecklenburg Report—a "live" lineup experience can be (in most places, usually is) preceded by a show-up, a "drive-by", or by a photo-array. To treat the rate of suspect identifications attained in *first* attempts in laboratories and *third* attempts in the field (by witnesses who were, in effect, pre-tested on a show-up and a photo-array) as equivalent doesn't just compare apples to oranges; it compares apples to automobiles. It isn't particularly surprising if third attempts by pre-tested witnesses (i.e., after two successful attempts and the dismissal of all of the unsuccessful witnesses) to identify a suspect in the field lead to fewer "filler ID's."

We don't know from the Mecklenburg Report that this happened, but unfortunately we can't know that it *didn't* happen, because the witnesses' history in the investigation is not recorded or reported. The Mecklenburg Report treats the field results as if they were the lab results, but the study under examination did not follow the scientific tradition of recording experimental data, and so it failed to capture data that the lab would have noted as a matter of routine experimental design. How many of these eyewitnesses identified fillers in initial field procedures? How many of these were filtered out of the process before the subsequent, reported lineups? How many witnesses in the subsequent lineups were performing a *confirming* recognition task following a successful suspect identification in a show-up or an array? Either of these features is at least as likely to have affected the suspect/filler identification rates as might wholesale

(**Doyle**, *continued on page 16*)

(**Doyle**, *continued from page 15*)

police "tipping" of witnesses by police. (Although the Mecklenburg Report persistently invokes the straw man of sinister allegations of police misconduct, in fact no one claims either that intentional police misconduct is the problem in the DNA eyewitness exoneration cases, or that if it *were* the problem, procedural reforms would be a silver-bullet solution to all intentional "framing" of suspects.) If people are cheating, they will continue to cheat, whatever procedures are adopted. But, as things now stand we are provided with no authoritative refutation of dark speculations about of police "tipping" because a wide range of data points—for example, the number of "low confidence" filler identifications and the number of failures to identify—were not captured in the study's design.

Operations and reliability are muddled in a different way when the "double-blind" technique is at issue. If "double-blind" procedures add something to accuracy, then implementing double-blind procedures—as jurisdictions in Wisconsin, Massachusetts, New Jersey and elsewhere have done—becomes a question of police ingenuity, commitment and leadership in surmounting operational challenges. But, if we ask the police their opinion of the "double-blind" approach before they are persuaded that it can contribute to accuracy, the police can't be blamed for accepting inconvenience and unfamiliarity as sufficient answer. Besides, when the police are not invited to participate in the design of the specific local double-blind sequential technique but are simply presented with a "take it or leave it" version in informal oral training an opportunity to confuse the performance is created and an opportunity to exploit police expertise is lost.

The Mecklenburg Report represents a taxing effort, its author and the participating officers and departments deserve our thanks, but the Report does not succeed in combining the perspectives of the street investigator, the laboratory scientist and the courtroom litigator into a working synthesis.

The Mecklenburg Report speaks for many when it suggests that we expand our inquiries and address further questions. But, real improvement in justice system processes based on science will only occur if the cops, prosecutors and defenders take responsibility for framing the right questions informed from the beginning by scientific advice. The system's practitioners not only have to take responsibility for integrating science into practice; they have to take responsibility for doing it *together*.

This will cause some discomfort. The Mecklenburg Report documents a field study that followed the more normal course of reform efforts within the system: One actor or another is charged with (or pro-actively assumes) responsibility for mobilizing one scientific advance or another, chooses its own scientists, closely holds the information developed and makes (or foregoes) reforms. This is not the only way.

Behind their adversarial routines, all criminal justice practitioners share a common enemy—the innocent defendant. No one wants the innocent in the system. The police do not want to waste their time on the innocent while the guilty go free to prey on new victims; the prosecutors realize that highly publicized exonerations in the cases they should have lost will later cost them the cases they should win. Maybe young defense lawyers go to law school with dreams of defending the innocent, but experienced defense lawyers see defending an innocent—particularly in an eyewitness case—as a nightmare. Double-blind, sequential lineup procedures—*if* they work to keep the innocent out of the system—are to everyone's advantage, and they should get a genuine scientific test for that reason alone.

It is also worth remembering that the question of eyewitness identification reform is not an all-or-nothing matter. Sophisticated police departments might, after testing, decide that some crime situations (for example, where there is a substantial amount of corroborating information) call for traditional methods of identification, while other, shakier, cases call for the more cautious, conservative double-blind sequential approach. "Double-blind" administration on its own (even if "simultaneous") also serves important law

(**Doyle**, *continued on page 31*)

SYMPOSIUM ISSUE

Getting to Truth Before It Falls into the Hands of the Lawyers: Pursuing Accuracy in Criminal Cases

James B. Zagel[1]

Societies have always wrestled with the overall question of the reliability of witnesses and, even now, when the legal rules are mostly settled, we still worry about perjury, mistakes, delusions and the integrity of memory. At issue today is the relatively small subset of the witness problem-just that one moment when the witness points to one person and says that is the person whose conduct I have described. For most witnesses to crime, the phrase "That's the man" is shortest part of the story they offer. Concern about its accuracy has been with us for centuries.

If solutions were easily found, this would not be an age old concern. Be wary of those who, with great confidence, offer the miracle cure to a problem we all recognize. The results of the Illinois double-blind eyewitness pilot program offer a vivid example of why what some think obvious is often not so. There is another point here; failure teaches as much or more than does success and we ought not to turn our back on any enterprise that seeks to make our investigations and adjudications better. The great value of tests, like the one we discuss herein, is they keep us from a terrible kind of optimism that, once disappointed, can lead us to abandoning the search for something better.

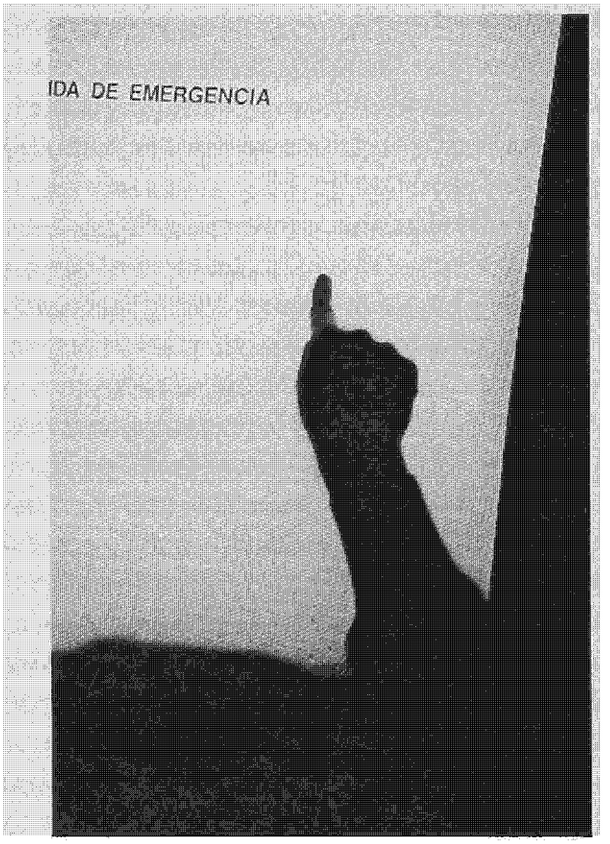
What Is It We Are Trying To Repair and Why?

Our world of arrest, prosecution and defense has changed. The idea that truth arises out of trial in an adversary system is still with us but mostly in theory not practice. Plea bargains are the dominant mode. My colleague, Judge Lynch in New York, has accurately described the process this way:

"[T]he prosecutor ... is the central adjudicator of facts ... arbiter of ... legal issues and of the appropriate sentence to be imposed. Potential defenses are presented by the defendant ... to a prosecutor, who assesses their factual accuracy and then decides the charge of which the defendant should be adjudged guilty..." [2]

This is a far cry from what the Supreme Court envisioned when it began to emphasize 75 years ago that defense counsel at trial was essential to getting at the truth.[3] And the place, they thought, where truth was to be found was trial where it was judge and jury, after hearing prosecution and defense, not prosecutor, who decided the outcome. This reliance on defense counsel to help us get to the truth was a key element in the first cases in which the Supreme Court sought to bring constitutional regulation to eyewitness identification.[4]

But defenders are not duty bound to see that the truth comes out. If the client is guilty they are obliged



It is critical to establish an identification before the eyewitness is subject to outside suggestion and a false identification is made. A false identification often has irreversable consequences.

to use all legally permissible means to see that the truth does not come out. This became particularly clear when we thought about what a lawyer should do at a lineup. Suppose the client tells his lawyer "Yeah, I stole the stuff but I'm sure no one saw me inside." Then the lawyer sees his client in a proposed line-up of seven, six of whom are Hispanic, and his client is the only blond white man in the group. Does

(**Zagel,** *continued on page 18*)

(**Zagel**, *continued from page 17*)

counsel want a fairer or more accurate lineup, one more likely to elicit the truth? If he gets one and the identification is made, he has deprived his client of a good argument at trial. What if the police ask the lawyer for suggestions to improve the lineup and agree that they will follow these suggestions? Does the lawyer improve the process to the detriment of his client? The dilemma here is stark because, unlike interrogation, the lawyer can not simply advise his client not to participate in the lineup. The identification procedure is going to happen. The lawyer is not authorized to decide simply that it is right to have a fair identification parade; the lawyer is only authorized to seek the kind of parade that is good for the client.

Many eventually accepted this state of affairs where, in a trial, getting to the truth was not the single overriding value.[5] I think they did so for two reasons. First, the thought was that, in nearly all cases, the truth came out anyway. Second, there were important social values found in procedural fairness and in giving the defendant a meaningful role in his or her defense. The price of an occasional criminal going free was thought to be worth paying to achieve these good things. This tradeoff has always been controversial. It might not survive a public referendum.

The tradeoff also rests on premises that professionals find hard to accept. It is not easy to find scholars (though not so hard to find judges) who actually believed that trial was really a good way to get at the truth in hard cases; the scholarly defense of the system was based upon its service to other democratic values. And even where the adversarial system could work, it was dependent on having a skilled, adequately funded advocate on both sides of the case. This last condition was often unmet.

In the decades that followed the criminal procedure upheavals of the 1960s, there was a lukewarm to cool acceptance of the way criminal cases were handled: lukewarm to cool because we were in the midst of a rise in crime that lasted for decades and, only relatively recently, subsided; accepted because there was much in popular media, shows like *NYPD Blue* and *Law and Order*, that portrayed a system that got the right result. The right result is the common result, but it is not because of our system of trials. It is because, in most cases, there is no serious question of guilt. The evidence is usually more than good enough and, if it isn't, the prosecutors frequently won't take a chance on the case. Perfection is unattainable, but getting it right in the largest percentage of cases is not good enough even if that percentage is in the high nineties. The consequences of error are too grave.

But still we toddled along with what we had. The volume of criminal cases, which was associated with the rise in crime and the relatively poorly funded defense services, led to fewer trials and what Judge Lynch called an administrative system of criminal justice. We would be that way today but for recent events.

The public, as opposed to the defenders of the world, was generally worried only about the guilty going free. New science and today's news made them worry, at least a little, about the innocent being found guilty.

For this reason the importance of finding the truth about guilt is valued more highly today than it has been in many years. We might be in the midst of a tectonic shift in perspective about crime, investigation and the accused.

Some of this might seem strange to say to the public. Haven't we always thought that the end of the criminal justice system was to find out the truth? Most people did, but no one who labors in this field believes that truth is always revealed or acted upon. There are unjustified convictions and unjustified acquittals. Even under the better practices that we will someday have, we will never reach perfection because the truth is elusive, often beyond the ability of humans to discover. But we won't stop prosecuting. Crime has a devastating effect on its victims and a large effect on the society in which we live. We have never decided to leave the guilty or the innocent to the judgment of heaven. We ought then to do the best we can to lock up all the guilty and free all the innocent, knowing that we will sometimes fail.

(**Zagel**, *continued on page 19*)

SYMPOSIUM ISSUE

(**Zagel**, *continued from page 18*)

What Has Led Us to Try Now?

The short answer to this question is that there is no sudden change, just a gaudy tipping point. The shift to concern for truth has been building for a while.

Start with *Miranda v. Arizona*[6] and the de-emphasis on admissions of guilt. *Miranda* can now be read as an implied endorsement of the reliability of eyewitnesses and, perhaps, of the very forensic evidence that is now under attack, say, bite marks, and even of reliable evidence excludible under the Fourth Amendment. The Court understood it was holding that even voluntary confessions, whose truth value was unquestioned, were to be excluded from evidence. This de-emphasis of truth and re-emphasis on procedural protections did not last very long. The Supreme Court limited the scope of *Miranda*.[7] The Court (in an opinion written by Justice Thurgood Marshall) also decided to permit the police to use deception to induce confessions[8] and narrowed the scope of the exclusionary rules based on the Fourth Amendment.[9]

The idea of science in law enforcement is relatively new. Identification by fingerprinting, for example, was not broadly accepted in the United States until the 1920s.

Despite this renewed endorsement of the value of confessions, there is no doubt that the Supreme Court, and every sane person for that matter, would prefer that guilt be determined by incontestable evidence like that found three times every week in New York, Miami and Las Vegas by an infallible corps of Crime Scene Investigators. It is science that created that gaudy tipping point.

The entry of science into the courtroom started at a very slow pace. Most of the earliest expert testimony from doctors and alienists was admitted into evidence because the law permitted *anybody* to offer opinion evidence.

Fingerprints are a good example of the early evidence of experts. Fingerprints were used for identification in India in the 1850s.[10] Written work appeared in 1881 and Galton's book was published in England in 1892.[11] By 1910, fingerprinting itself was in fairly wide police use.[12] Despite this, it was not until 1911 that a reviewing court approved its use, but broad acceptance did not come until the 1920s, and it was not until the 1940s that courts said the prosecution would no longer have to prove that no two fingerprints are alike.[13] The course of admission of other forms of identification evidence was similar. It took time to get the courts to approve comparative micrography, microanalysis, questioned documents. Most of the first scientific evidence dealt with traces and marks which the jurors themselves could perceive-friction ridges, striations on bullets and so forth. As the twentieth century went on, and science itself began to deal with things not directly observable, the law began to take in serology, general chemistry and neutron activation analysis. In all these cases, though, one reason the courts moved slowly was the resistance of defense counsel to the admission of such evidence because it rarely served any purpose other than to incriminate their clients.

DNA evidence was accepted with amazing speed precisely because it could exonerate as well as incriminate. There were very few to fight tooth and nail against its admissibility because the prosecutor or defender who objected vigorously to DNA evidence knew that, in the next case, they might be offering that same evidence. DNA, too, came to the courts at a time when standards of what constituted reliable and valid science had become clearer. DNA analysis had the advantage of service as a tool in many sciences, not merely criminalistics. The broader use of DNA analysis meant the discipline had been critically reviewed by many more scientists than, say, fingerprints.

It is true that the advent of closer judicial scrutiny of expert witnesses in recent years[14] has called into question much of the science that is offered in the courtroom, but the outcome of disputes about questioned document examination, serology, fiber analysis,

(**Zagel**, *continued on page 20*)

(**Zagel**, *continued from page 19*)

(maybe, even fingerprints) are not for today's discussion. DNA survived its first tests and it proved that there were a small, but nonetheless very meaningful, number of wrongful convictions. DNA science teaches there is, in a certain class of cases, a class of evidence that, if properly handled, is conclusive. Of course, properly handled is not inevitable, but it put forth an implicit message. We can get to the truth better than we had gotten to it before. And this is why pilot programs are undertaken: not because eyewitnesses can become the legal equivalent of recombinant strands of DNA, but because DNA showed that we can do better. Obviously only if we try to do better.

There will be attention paid to the causes of wrongful convictions, now the predominant element is bad eyewitness identifications. So too, we will look at error rates in particular kinds of cases.[15] These studies might help us in our scrutiny of past errors.

Professional investigators of crime have known for hundreds of years that evidence can lead to the wrong person, that some eyewitness identifications are worthless, that some confessions are worthless and that some forensic analyses are worthless. From the perspective of the police and prosecutors, the solution was either not to charge in those cases or to drop the charges if already brought. The defense often proposed this solution. The problem was handled in house and there was always the final safeguard of the trial.

What DNA told the public is that a trial does not protect adequately against these errors if the prosecutor decides to go forward with the case. DNA put an enormous dent into the idea that the adversary system is the best way to protect against false convictions. The belief that even good faith errors made earlier in the process will be detected and repaired as the case moved through our adversary system has lost some of its hold on our society. While the adversary system might serve many social values apart from its detecting the truth of accusations, all these values collectively seem no longer to outweigh the risk of that the judgments it produces may be untrustworthy. It is not that these values are to be disregarded, it is the degree to which they are fostered that is questioned. When a crime victim sees the perpetrator unjustly acquitted, it is small consolation to tell them that it is better that ten guilty men go free than one innocent man be convicted, but the society as a whole accepts, or at least understands, this policy. Now the message is, not only do we let those ten guilty go free by the way, we also send quite a few innocent men to prison too. That message is not well received.

So the turn now is to making things right before the lawyers in the adversary system get their hands on it.

What Can We Expect from Science?

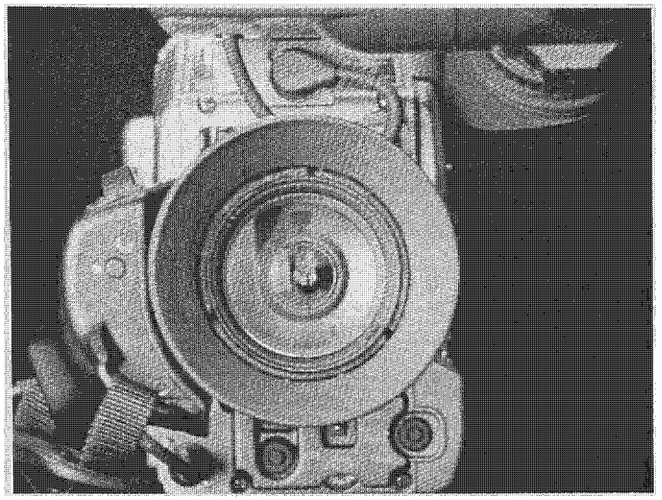
Science proceeds by evaluating ideas, theories, guesses, conjectures, hopes and dreams. It does this by experiments of all sorts and observations.

The idea that there is science in law enforcement is not so old. Early criminology was a form of moral philosophy as in Cesare Beccaria.[16] One of its first scientists was an anthropologist Cesare Lombroso,[17] who died less than one hundred years ago. Many of his theses seem laughable today but he used the inductive method of science as well as he could. Our understanding of criminology is still very much in flux. We seem to know that more police officers and more people in prisons are good at reducing crime rates. Some of the standard explanations about strong economies, too many people under thirty, order maintenance policing, strong gun laws, capital punishment, changing drug habits and markets all remain unproved.[18] But we are not talking about criminology here, it is criminalistics or police science which largely concerns itself with helping to find out, by examination of physical objects, who did what, when and how.

In recent times, I have noted, some accepted police science has come under question. This is not unique to police science. The scientific enterprise is filled with failure and mistakes. There is a well-known maxim offered to some first-year medical students which runs this way: "half of what we teach you will be wrong, we just don't know which half."

Mistakes in science are not limited to earlier centuries like the phlogistan theory of fire. The theory is a laughing stock today but it was clever in its time. The inventor believed there was a combustible substance-phlogistan-consumed by combustion which required air. After the phlogistan was gone the residue weighed less than the original product as is demonstrated by the case of ashes which weigh less than the burnt log.

(**Zagel**, *continued on page 32*)



Recent experiments have studied and compared eyewitness identification procedures including the use of moving video versus photographs, culprit-absent lineups versus culprit-present lineups, and foil selection by culprit description versus selection by suspect resemblance.

How can psychological science enhance the effectiveness of identification procedures? An international comparison.

Tim Valentine, Stephen Darling and Amina Memon[1]

The sequential double-blind method protects the guilty, moving video images protect the innocent (a little), but foil selection strategy makes no difference.

The reliability of eyewitness identification has attracted concern from the legal profession in England for at least 100 years. In 1904 a committee of enquiry was established to investigate the trials of Adolf Beck. Incredibly, on two separate occasions Adolf Beck was wrongly convicted on the basis of mistaken eyewitness identification. In both trials, multiple eyewitnesses identified Beck as a confidence trickster who stole jewellery from them. The crimes were subsequently found to have been committed by William Wyatt. The 1904 Committee of enquiry led directly to the establishment of a Court of Appeal.[2]

Concern about further wrongful convictions based on mistaken identification led to a government enquiry into the reliability of eyewitness identification evidence, chaired by Lord Devlin, which reported in 1976.[3] The Devlin report led directly to a landmark judgement in the English Court of Appeal, which established a requirement that in cases of disputed identification the trial judge must caution the jury about the dangers of eyewitness identification evidence. The judge should point out that confident eyewitnesses may be mistaken and instruct the jury to consider carefully the circumstances of the identification.[4]

From this historical perspective, it is unsurprising to learn that mistaken eyewitness identification is also a major problem for the United States courts. Nevertheless, the extent of the problem has proved to be greater than many may have anticipated. The work of the U.S. Innocence Project, which to date has led to 183 prisoners being exonerated by new DNA evidence, found that mistaken eyewitness identification was a factor contributing to three-quarters of the original wrongful convictions.[5]

Recent developments to eyewitness identification procedures

Eyewitness identification procedures used in the United States and the United Kingdom have some important differences. In the United States, live lineups and identification from arrays of photographs are both frequently used to collect formal eyewitness identification. Traditionally, in the United Kingdom all formal eyewitness identification evidence has been obtained from live lineups. Identification from arrays of photographs has never been permitted as a formal means of identification. Over the last few years video has replaced almost all live lineups. This innovation has been made possible by development of sophisticated computer systems used to compile video lineups from a standardised database of moving video clips.

Recently identification procedures in the United States have been the subject of consultation with eyewitness researchers. Identification from arrays of photographs is still widely used, but the U.S. National Institute of Justice set up a Technical Working Party for Eyewitness Evidence to review procedure and produced a guide to best practice.

(**Valentine**, *continued on page 22*)

(**Valentine**, *continued from page* 21)

Recent developments to identification procedures in the USA

The U. S. National Institute of Justice document *Eyewitness identification: A guide for law enforcement* ("the Guide")[6] includes the following guidance:

- The foils in a lineup should be selected to generally match the witness' description of the culprit.
- There should be a minimum of five foils.
- The witness should be advised that the culprit may or may not be in the lineup.
- The witness should state in their own words how confident they are of any identification.

Two methods of lineup presentation are endorsed by the Guide: 1) a *simultaneous* lineup, in which the witness is permitted to inspect all of the photographs or lineup members before making an identification and 2) a *sequential* lineup, in which the witness sees one photograph or person at a time and makes a decision prior to viewing any other photograph or person.[7] The guide does not express any preference for one method over the other. The procedures mentioned here do not form an exhaustive list of the provisions in the Guide. It should be noted that the guidance is a recommendation of best practice and has no direct legal force.

In an earlier 'white paper,' written under the auspices of the American Psychology - Law Society ("AP-LS"),[8] psychologists had advocated that the person who administers a lineup should not know which person in the lineup is the police suspect. That is to say that the administrator should be 'blind' to the identity of the suspect. This procedure is known as 'double-blind' as neither the administrator nor the witness has prior knowledge of who the suspect is in the lineup. This measure was strongly advocated by researchers because it removes all possibility of the witness being influenced by the lineup administrator. Such influence can be very subtle and may occur without any intention or awareness of either the administrator or the witness. The double-blind procedure is well established as an important aspect of scientific enquiry. For example, neither the patient nor the clinical staff should know which patients received a placebo in a drug trial. A recommendation of the double-blind method is conspicuously absent from recommended best practice in the Guide on eyewitness identification.

Research based on identification from photograph arrays suggests that mistaken identification can be reduced by sequential presentation of the photographs as outlined in the Guide.[9] However, the Guide did not include the important stipulation of a 'sequential double-blind method.' Under sequential presentation instructions the witness should make a decision after viewing each photograph as to whether he or she is the culprit. If the witness rejects the photograph they are shown the next photograph. The procedure stops when the witness makes an identification. The method endorsed by researchers crucially stipulates that the witness should not know how many photographs are in the lineup, the witness is given unbiased instructions (e.g., that the person they saw may or may not be in the lineup) and, importantly, that the administrator is blind to the identity of the suspect.[10]

Video identification has a number of important benefits [including]. . . dramatically reduc[ing] the delay before an identification can be organized, . . . usually produc[ing] a video lineup within two hours of request, . . . [has] a large database of video clips from which to select foils, . . . and [employs] a laptop which can be taken to a witness who is unable to attend the police station.

Sequential presentation is believed to reduce mistaken identification by reducing the opportunity for the witness to make a relative judgement. In the traditional simultaneous presentation, a witness who believes that the culprit is in the lineup may identify the person who most looks like the person they saw, having had the opportunity to view all the photographs in an array. Sequential presentation aims to prevent relative judgements by forcing the witness to make

(**Valentine**, *continued on page* 23)

(**Valentine**, *continued from page* 22)

independent judgements to each lineup member. Sequential presentation has been adopted in some jurisdictions in the United States. However, in some cases the strict procedure advocated by researchers has not been followed in all of its aspects. It is worth noting that researchers did not include sequential presentation amongst the recommendations of the AP-LS white paper.[11]

Recent developments to identification procedures in England & Wales

The Police and Criminal Evidence Act of 1984 ("PACE") which applies in England and Wales (but not in Scotland or Northern Ireland), includes a code of practice for identification by eyewitnesses ("code D"). The code can be revised without the need for new primary legislation. In recent years the code has been revised on an annual basis. The current code of practice (2005)[12] includes the following provisions:

- A lineup that includes one suspect must consist of at least eight foils.
- The foils must resemble the suspect in age, general appearance and position in life.
- The suspect has the right for their legal representative to be present during the identification procedure.
- The person who administers the lineup cannot be involved in the investigation of the case (but note that the administrator does know who the suspect is).
- Witnesses must be advised that the person they saw may or may not be present.
- Witnesses must be advised that if they cannot make a positive identification they should say so.
- Witnesses must view each member of the lineup twice before making any identification.
- Video identification should be used unless there is a reason why a live identification is more appropriate.

Although the code of practice does not have statutory force, trial judges have the discretion to exclude or allow eyewitness identification evidence. Therefore police forces have systems in place to demonstrate compliance with the code.

Two different IT systems are in widespread use in British police forces to provide video identification. VIPER™ (Video Identification Procedure Electronic Recording) and PROMAT™ (Profile Matching).[13] The systems produce similar formats of video lineup, but each has its own database of images. Lineups consist of 15 second clips of each person shown one after another. The sequence starts with a head and shoulders shot of the person looking directly at the camera, who slowly turns their head to present a full right profile to the camera. The person then slowly rotates their head to present a full left profile to the camera. Finally the person returns to looking directly into the camera in a full-face pose.

Research on video identification

Research has demonstrated that VIPER video lineups from real criminal cases were fairer to the suspects than conventional 'live' lineups,[14] and that VIPER video lineups were equally fair to white European and African–Caribbean suspects.[15] In these studies, participants (known as 'mock witnesses') were shown a set of videos of VIPER lineups or a set of photographs of live lineups held as part of the investigation of the case. For each lineup they were given the first description of the offender made by the original witness. The mock witnesses were required to choose, on the basis of the witness' description, the lineup member who they think is most likely to be the police suspect. Therefore, a 'mock witness' simulates a witness who (a) has no memory of the culprit at the time of the identification procedure; (b) can remember the description they previously gave to the police and (c) nevertheless, makes an identification from the lineup. If the lineup is perfectly fair, and all members fit the description, the mock witness would have no basis on which to make their selection and would merely have to guess who is the suspect. Therefore, if a large number of the mock witnesses are asked to make a selection they would select the suspect on 11 percent of occasions (1 in 9) from each lineup, because the lineups all contained a suspect and eight foils.

(**Valentine**, *continued on page* 24)