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

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The Mecklenburg Report 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 ("the Guide") in 1999. We still have a substantial body of laboratory results arguing for the procedures that the Guide identified as good (pre-lineup 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”
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 perversely determined 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.”

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.” 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.

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

James B. Zagel

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.”

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. 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.

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

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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. 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.

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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* 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*. The Court (in an opinion written by Justice Thurgood Marshall) also decided to permit the police to use deception to induce confessions' and narrowed the scope of the exclusionary rules based on the Fourth Amendment.

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. Written work appeared in 1881 and Galton’s book was published in England in 1892. By 1910, fingerprinting itself was in fairly wide police use. 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. 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 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,

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(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. 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. One of its first scientists was an anthropologist Cesare Lombroso, 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. 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 phlogiston 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.

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