

***What's The Matter with Illinois? How Illinois Squandered an Opportunity to Conduct an Important Study on Eyewitness Identification Reform Procedures\****

In April 2002, the Illinois Governor's Commission on Capital Punishment issued a report that sought to identify why Illinois had experienced so many wrongful convictions in capital cases and to propose changes to rehabilitate Illinois's criminal justice system. One focus was faulty eyewitness testimony. As the Commission noted, "the fallibility of eyewitness testimony has become increasingly well-documented in both academic literature and courts of law."<sup>1</sup> This conclusion was both uncontroversial and understated: Virtually all studies rank mistaken eyewitness identification as the leading cause of wrongful convictions, by one estimate accounting for 88% of the erroneous rape convictions and 50% of the wrongful murder convictions.<sup>2</sup> The Department of Justice itself analyzed twenty-eight DNA exonerations and concluded that inaccurate eyewitness testimony was "the most compelling evidence" in the majority of those cases.<sup>3</sup>

Faced with these facts and the many wrongful convictions already documented in Illinois, the Governor's Commission decided that *status quo* police identification procedures provided inadequate protections against mistaken identifications. The Commission accordingly sought to improve police identification practices, focusing much of its effort on the use of double-blind sequential identification procedures.<sup>4</sup> In a "double-blind" identification procedure, the police

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\* Thanks to Dawn Davison, Sean Clark, Giovanna Shay and Richard Schmechel for their input on this article.

<sup>1</sup> REPORT OF THE GOVERNOR'S COMMISSION ON CAPITAL PUNISHMENT 31 (2002), [http://www.idoc.state.il.us/ccp/ccp/reports/commission\\_report/complete\\_report.pdf](http://www.idoc.state.il.us/ccp/ccp/reports/commission_report/complete_report.pdf) (last visited June 5, 2006) [hereinafter COMMISSION REPORT].

<sup>2</sup> Samuel R. Gross et al., *Exonerations in the United States 1989 through 2003*, 95 J. CRIM. L. & CRIMINOLOGY 523, 544 (2005); see also Daniel S. Medwed, *Anatomy of a Wrongful Conviction: Theoretical Implications and Practical Solutions*, 51 VILL. L. REV. 337 (2006).

<sup>3</sup> EDWARD CONNORS ET AL., U.S. DEP'T OF JUSTICE, CONVICTED BY JURIES, EXONERATED BY SCIENCE: CASE STUDIES IN THE USE OF DNA EVIDENCE TO ESTABLISH INNOCENCE AFTER TRIAL 24 (1996), <http://www.ncjrs.gov/pdffiles/dnaevid.pdf> (last visited June 5, 2006).

<sup>4</sup> The Commission also recommended adoption of standard instructions to witnesses before any identification procedure, use of lineup "fillers" who did not stand out, contemporaneous recording of witness

administrator of the lineup does not know who the suspect is, and therefore *cannot* affect the results of a lineup or photo-spread procedure by consciously or unconsciously signaling the suspect's identity to the witness.<sup>5</sup> The Commission unanimously agreed that double-blind procedures were an important tool for preventing mistaken identifications and encouraged their use; in fact, some Commission members wanted to mandate them.<sup>6</sup> Indeed, given that double-blind procedures are standard in all scientific experiments—because they prevent conscious or unconscious biases of the administrator from affecting the results—and that all identification procedures are properly considered a type of experiment, it is hard to imagine any ground for dissent.<sup>7</sup>

The Commission also voted to encourage use of sequential lineup procedures.<sup>8</sup> The sequential lineup procedure seeks to improve upon the simultaneous lineup procedure by

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confidence statements, and videotaping identification procedures whenever feasible. COMMISSION REPORT, *supra* note 1, at 31–40.

<sup>5</sup> Gary L. Wells et al., *Eyewitness Identification Procedures: Recommendations for Lineups and Photospreads*, 22 LAW & HUM. BEHAV. 603, 627 (1998), available at <http://www.innocenceproject.org/docs/whitepaper.pdf> (last visited June 7, 2006); Gary L. Wells & Elizabeth A. Olson, *Eyewitness Identification*, 54 ANN. REV. PSYCHOL. 277, 289 (2003).

<sup>6</sup> COMMISSION REPORT, *supra* note 1, at 32–33 (“Recommendation 10: When practicable, police departments should insure that the person who conducts the lineup or photospread should not be aware of which member of the lineup or photo spread is the suspect.”).

<sup>7</sup> As one of the psychologists who assisted in the preparation of the Illinois pilot project has recently written, “Double-blind testing is standard procedure in medical and pharmaceutical research, and is used in some areas of experimental psychology to prevent inadvertent communication of information to research participants about critical aspects of the research. Blind administration is an important step to take in any eyewitness identification procedure . . . .” Dawn McQuiston-Surrett, Roy S. Malpass & Colin G. Tredoux, *Sequential vs. Simultaneous Lineups: A Review of Methods, Data, and Theory*, 13 PSYCHOL. PUB. POL’Y & L. (forthcoming 2006), available at <http://eyewitness.utep.edu/Documents/McQuiston-SurrettMalpassTredoux2006SimultSequentLineupReview.pdf> (last visited June 6, 2006). See also Wells et al., *supra* note 5, at 627; Wells & Olson, *supra* note 5, at 289. Furthermore, logistical objections to implementing double-blind sequential eyewitness identification procedures are unfounded. After his county reformed its eyewitness identification procedures, Deputy District Attorney David Angel of Santa Clara stated, “Some people have said that [these reforms] would reduce valid identifications, or they would be too expensive or too difficult to implement, but these problems have not come forward . . . There is compliance; the training is not difficult; good IDs are made, and presumably they're more accurate.” Bernice Yeung, *Innocence Arrested*, SFWEEKLY.COM, Oct. 29, 2003, [http://www.sfweekly.com/issues/2003-10-29/feature\\_1.html](http://www.sfweekly.com/issues/2003-10-29/feature_1.html).

<sup>8</sup> COMMISSION REPORT, *supra* note 1, at 34–36 (“Recommendation 12: If the administrator of the lineup or photospread does not know who the suspect is, a sequential procedure should be used, so that the eyewitness views

addressing potential dangers that may arise as a result of the “relative judgment” effect. Researchers have found that this effect occurs when a witness views the suspects side-by-side and selects the person who looks “most like” the culprit even if the actual culprit is absent from the lineup. Sequential procedures seek to avoid this danger by having the witness view individuals in photo arrays or live lineups one-at-a-time so that the witness makes an independent judgment about whether each individual is the culprit.<sup>9</sup> The majority on the Commission argued that sequential lineup procedures had been proven more reliable than simultaneous procedures by many studies and further noted that the New Jersey Attorney General had recommended this procedure after substantial experience using it.<sup>10</sup>

The Illinois Legislature considered these recommendations throughout 2003. While all the recommendations generated considerable support and most were universally acclaimed, Chicago police vigorously opposed the recommendations for double-blind, sequential procedures. One police spokesman complained that Chicago was being used as “the proving ground” for academic research.<sup>11</sup> In the face of strong police opposition, the reform package the Illinois Legislature adopted in November 2003 omitted double-blind and sequential procedures for the time being.<sup>12</sup> Instead, the Legislature created a pilot project in three Illinois cities, where the Illinois State Police would use sequential, double-blind procedures, and analysts would then study the results. The Legislature’s expectation was that the Illinois State Police would run the pilot project. The Legislature also created a statewide body to monitor all of the newly-adopted

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only one lineup member or photo at a time and makes a decision (that is the perpetrator or that is not the perpetrator) regarding each person before viewing another lineup member or photo.”)

<sup>9</sup> Gary L. Wells & Eric P. Seelau, *Eyewitness Identification: Psychological Research and Legal Policy on Lineups*, 1 PSYCHOL. PUB. POL’Y & L. 765, 772 (1995); *see also* Wells et al., *supra* note 5 at 639.

<sup>10</sup> COMMISSION REPORT, *supra* note 1, at 34–36.

<sup>11</sup> Frank Main, *One-By-One Police Lineup Considered*, CHI. SUN TIMES, May 6, 2003, at 12.

<sup>12</sup> Capital Punishment Reform Study Committee Act, 2003 Ill. Laws P.A. 93-605 (codified in sections of 20 ILL. COMP. STAT., 30 ILL. COMP. STAT., 50 ILL. COMP. STAT., 720 ILL. COMP. STAT., 725 ILL. COMP. STAT., 730 ILL. COMP. STAT.).

reforms, including the pilot project, and named former United States Attorney Thomas Sullivan as chairman of the committee.

The Illinois Legislature’s pilot project held considerable promise to supplement evidence from many other jurisdictions where police and prosecutors have spearheaded reforms to identification procedures—including New Jersey, Boston, MA, Madison, WI, Santa Clara, CA, just to name a few—by providing additional evidence about how sequential, double-blind procedures worked in the field. To ensure that the study would serve this purpose, the Legislature set clear guidelines, fully defining the type of sequential, double-blind procedure it wanted police to study.<sup>13</sup> The Legislature also provided directions about the basis for comparison—that is, what the sequential, double-blind results would be measured against—by instructing the Illinois State Police to “develop a protocol for the selection and administration of lineups which is practical, designed to elicit information for comparative evaluation purposes, and is consistent with objective scientific research methodology.”<sup>14</sup>

On March 17, 2006, the Program Director for the pilot program submitted its *Report to the Legislature of the State of Illinois: The Illinois Pilot Program on Sequential Double-Blind Identification Procedures* (Illinois Report), detailing the results of the study.<sup>15</sup> Disappointingly, the project had a number of defects, which become apparent from a review of the Illinois Report itself.

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<sup>13</sup> 725 ILL. COMP. STAT. 5/107A-10(c) (2005).

<sup>14</sup> 725 ILL. COMP. STAT. 5/107A-10(e) (2005).

<sup>15</sup> SHERI L. MECKLENBURG, ILLINOIS STATE POLICE, REPORT TO THE LEGISLATURE OF THE STATE OF ILLINOIS: THE ILLINOIS PILOT PROGRAM ON SEQUENTIAL DOUBLE-BLIND IDENTIFICATION PROCEDURES 36 (2006), [http://www.chicagopolice.org/IL\\_Pilot\\_on\\_Eyewitness\\_ID.pdf](http://www.chicagopolice.org/IL_Pilot_on_Eyewitness_ID.pdf) (last visited June 5, 2006) [hereinafter ILLINOIS REPORT].

First, contrary to the Legislature’s expectation, the Illinois State Police did not run the project; instead, “the state police ceded that responsibility to the Chicago Police Department, even though Chicago police officials had expressed opposition to testing sequential lineups.”<sup>16</sup>

Second, the Illinois Report also indicates that the police were trained beforehand on the nature and purpose of the study,<sup>17</sup> and therefore had every reason to anticipate what researchers were looking to find and the likely consequences of any results.<sup>18</sup> While training on a permanent program once a new procedure has been adopted is a good way of achieving police buy-in, and while some training on how to use the new sequential double-blind procedures was undoubtedly necessary, it is impossible to defend the widespread practice of training all the subjects of a pilot project experiment about the nature and purpose of that experiment. Such training cannot be squared with valid scientific methodology because it creates the possibility that the subjects will change their behavior to influence the outcome.<sup>19</sup> Of course, this sort of advance knowledge posed little danger in the sequential double-blind group, where the administrator’s ability to affect the identification process was virtually nonexistent. However, this training posed a substantially greater problem in connection with the simultaneous control group, where the administrator had the ability to affect the results.

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<sup>16</sup> Editorial, *The lineup: What Happened?*, CHI. TRIB. Apr. 30, 2006, at C6. That hostility is clearly reflected in comments by police, which are appended to the Illinois Report itself. Exhibit 19 to the Illinois Report reveals police officer comments ranging from “this is a bad idea, scrap it” to “get rid of it, it is useless. Stop making the idiot who decided to start this look like they had a purpose.” One police officer complained that “the only thing th[e sequential] procedure does is make it more difficult for a witness to make an identification . . . . When is the government and the criminal justice system going to stop permitting a small but vocal group of liberal thinkers, whose only concern is the rights of criminals, to dictate procedures?” ILLINOIS REPORT, *supra* note 15 at Exhibit 19.

<sup>17</sup> ILLINOIS REPORT, *supra* note 15 at 36.

<sup>18</sup> Some of the police comments on the study, included as Exhibit 19 of the Illinois Report, also suggest that police knew the nature and purpose of the study and were influenced by that knowledge. For example, one officer commented “glad to be back to the old way” and another observed that “it’s a very difficult process, which results in no increase in the reliability of identifications.” ILLINOIS REPORT, *supra* note 15 at Exhibit 19.

<sup>19</sup> See McQuiston-Surrett, Malpass & Tredoux, *supra* note 7 (explaining that use of double-blind testing guards against inadvertent communication of critical research information to research participants).

Third, the Illinois Report shows that there was no serious attempt to comply with the Legislative mandate to use “objective scientific methodology” in creating protocols for the simultaneous control group. Thus, while the sequential double-blind procedures being evaluated were clearly scripted and fairly consistently followed, the simultaneous control group had no meaningful protocol at all, much less one that was “designed to elicit information for comparative evaluation purposes” or was “consistent with objective scientific research methodology.”<sup>20</sup> Instead, for the control group, the simultaneous procedures varied widely with respect to lineup size, construction, instruction and presentation. The simultaneous control group procedures were conducted by someone who knew exactly who the suspect was, usually the lead detective in the case.

The Illinois Report thus was not comparing apples to apples; the sequential and simultaneous group did not just use different methods of presenting the suspects to the witness, they had other influential variables including, most importantly, the fact that the sequential procedures had a blind administrator while the simultaneous control group did not.

The failure to conduct sequential and simultaneous procedures under like conditions is probably the biggest flaw in the Illinois report.<sup>21</sup> Of course, most field studies cannot completely

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<sup>20</sup> 725 ILL. STAT. COMP. 5/107A-10(e) (2005).

<sup>21</sup> See, e.g., BRIAN L. CUTLER & STEVEN D. PENROD, MISTAKEN IDENTIFICATION: THE EYEWITNESS, PSYCHOLOGY, AND THE LAW 9 (1995) (“The purpose of most experiments is to isolate some factor, such as viewing conditions or the manner in which a lineup test is conducted, and examine its influence while holding all other factors constant.”); *id.* at 61 (“Psychologists design their studies carefully so that they do not inadvertently contaminate their results with chance factors . . . .”); Janice VanCleave, *Science Fair Handbook*, <http://school.discovery.com/sciencefaircentral/scifairstudio/handbook/scientificmethod.html> (last visited June 6, 2006) (explaining independent variables, dependent variables, controlled variables and that controlled variables should be identical in the experimental group and the control group); Dummies.com, *Designing Experiments Using the Scientific Method*, <http://www.dummies.com/WileyCDA/DummiesArticle/id-1203.html> (last visited June 6, 2006) (“Well, there are things called *variables*. Variables vary: They change, they differ, and they are not the same. A well-designed experiment needs to have an *independent variable* and a *dependent variable*. The independent variable is what the scientist manipulates in the experiment. The dependent variable changes based on how the independent variable is manipulated . . . . Experiments can have only one independent variable.” (emphasis in original)); Reference.com, *Experiment*, <http://www.reference.com/browse/wiki/Experiment> (last visited June 6, 2006) (“To demonstrate a cause and effect hypothesis, an experiment must often show that, for example, a

control for variability: Field studies will inevitably have more deviations from protocol than the laboratory, because it is harder to control real-world conditions. But the simultaneous control group incorporated numerous variables, making it fundamentally flawed from the outset—even before it was exposed to variations that inevitably arose from real-world conditions. There were, in short, so many variables between the simultaneous control group and the sequential double-blind group that the pilot program never could have yielded meaningful results. The study’s design itself prevented any determination of whether differences between the sequential double-blind group and simultaneous control group were due to the different presentation format, or whether they were caused by some other difference such as the non-blind administration. The Illinois Report reflects a design flaw that could only have led to one conclusion: that “more study was required” because the pilot program was not executed in a manner that could have provided any valuable information.

While the failure to conduct sequential and simultaneous procedures under like conditions was “probably” the report’s biggest flaw, there was one other glaring defect in the report’s methodology: *It equated a pick of the police suspect with a correct identification.* To put a fine point on it, this means that the Illinois Report would have counted every single one of the DNA exonerations as “correct” identifications.

Even worse, the benchmark picked by the Illinois Report actually *rewards suggestive police procedures* by skewing the results in favor of any method that encouraged witnesses to make more “suspect” selections regardless of accuracy. Because the main problem with suggestive procedures is that they prompt a witness to identify the suspect regardless of whether

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phenomenon occurs after a certain treatment is given to a subject, and that the phenomenon does *not* occur in the *absence* of the treatment . . . . A *controlled* experiment generally compares the results obtained from an experimental sample against a *control* sample, which is practically identical to the experimental sample except for the one aspect whose effect is being tested.” (emphasis in original).

he was actually the culprit,<sup>22</sup> the benchmark chosen by the Illinois Report makes suggestive procedures—i.e., those designed to produce a pick of the police suspect—seem to be the most efficient.

Of course, as the Illinois Report notes, other field studies have used “suspect” identifications as “a useful measure for accurate identifications.”<sup>23</sup> What the report omits, however, is that those field studies used double-blind procedures—that is, they were conducted by officers who did not know who the suspect was, and thus were incapable of suggesting to the witness, consciously or unconsciously, who the suspect was.<sup>24</sup> When a field study adopts procedures that eliminate “suggestivity” as a meaningful concern—as double-blind procedures do—there is no real danger that using suspect identifications as a rough proxy for accuracy will unfairly skew the results in favor of suggestive procedures. Some of the identified suspects will not be the real culprit, of course, but because suggestivity is not a concern, that sort of eyewitness error should remain constant in all groups under study. But when one group has protocols that create classic suggestivity concerns and the other does not—as occurred in the Illinois study—using a “suspect” identification as the benchmark for accuracy in both groups creates a huge risk that the benchmark inflates the perceived reliability of the *most suggestive* procedures, rather than the most accurate ones. Where suggestivity is a legitimate concern, some

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<sup>22</sup> See, e.g., *Neil v. Biggers*, 409 U.S. 188, 198 (1972) (“Suggestive confrontations are disapproved because they increase the likelihood of misidentification and unnecessarily suggestive ones are condemned for the further reason that the increased chance of misidentification is gratuitous.”); *Foster v. California*, 394 U.S. 440, 443 (1969) (“The suggestive elements in this identification procedure made it all but inevitable that [the witness] would identify petitioner whether or not he was in fact ‘the man.’ In effect, the police repeatedly said to the witness, ‘*This is the man.*’” (emphasis in original)).

<sup>23</sup> ILLINOIS REPORT, *supra* note 15 at iii.

<sup>24</sup> The Report claims that its “suspect identification” benchmark was the same one used in a field study of double-blind sequential procedures in Hennepin County, Minnesota. But that study involved *only* double-blind procedures. NANCY STEBLAY, OBSERVATIONS ON THE ILLINOIS LINEUP DATA (2006), [http://www.psychology.iastate.edu/FACULTY/gwells/Stebly\\_Observations\\_on\\_the\\_Illinois\\_Data.pdf](http://www.psychology.iastate.edu/FACULTY/gwells/Stebly_Observations_on_the_Illinois_Data.pdf). (last visited June 6, 2006).

other measure (either conviction, or at least survival of the case through trial) must be used to determine whether an identification is accurate.

Finally, the Illinois Report shows that the pilot project did not reflect the sort of openness that is vital to any good empirical study.<sup>25</sup> In fact, Chicago Police would not allow the chairman of the committee established to monitor state criminal justice reforms to review the study in progress and did not provide him beforehand with the control group protocols created to test the sequential double-blind procedures.<sup>26</sup>

Given the flawed design of the pilot program, the results were predictable. The highly-scripted, closely-monitored, sequential, double-blind procedures produced results that were in the mainstream of other field studies.<sup>27</sup> The highly variable, non-blind simultaneous procedures, however, produced incongruous results that prove worthless for comparison purposes. Contrary to every other field or laboratory study ever conducted, the Illinois report purports to show that the hodge-podge of variable non-blind simultaneous procedures used in Chicago and Evanston worked absolutely perfectly because they produced **no filler identifications out of 152 lineups.**<sup>28</sup> In other words, in Chicago and Evanston, although some witnesses did not make a

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<sup>25</sup> See, e.g., COMM. ON SCI., ENG'G, AND PUB. POLICY, a joint committee of the NAT'L ACAD. OF SCIENCES, NAT'L ACAD. OF ENG'G & INST. OF MED., *Experimental Techniques and the Treatment of Data*, in ON BEING A SCIENTIST: RESPONSIBLE CONDUCT IN RESEARCH (Nat'l Acad. Press 2d ed. 1995), available at <http://www.nap.edu/readingroom/books/obas/> (last visited June 7, 2006) [hereinafter ON BEING A SCIENTIST] (“[R]esearchers have to be extremely clear, both to themselves and to others, about the methods being used to gather and analyze data. Other scientists will be judging not only the validity of the data but also the validity and accuracy of the methods used to derive those data . . . . If someone is not forthcoming about the procedures used to derive a new result, the validation of that result by others will be hampered.”).

<sup>26</sup> Editorial, *supra* note 16. Although the author of the Illinois Report questioned this characterization of the process, see Sheri H. Mecklenburg, Letter to the Editor, CHI. TRIB. May 12, 2006, at C24 (objecting to the characterization and criticism of the Illinois Report in *The lineup: What Happened?*) and although the Chicago Tribune responded with a clarification, of its original editorial, see Corrections and Clarifications, CHI. TRIB. May 12, 2006 (explaining that the editorial had “overstated the Chicago Police Department’s unwillingness to share complete protocols . . . [and Mecklenburg’s] unwillingness to allow Sullivan to observe the lineups”), that “clarification” also reaffirmed the basic point that the chairman of the reform commission had not seen the basic protocols beforehand and had been denied permission to review the study in progress.

<sup>27</sup> NANCY STEBLAY, OBSERVATIONS ON THE ILLINOIS LINEUP DATA (2006), *supra* note 24, at 2.

<sup>28</sup> In the final report, these stark results were obscured somewhat by those of Joliet, which was the other city included in the pilot program. Approximately 33% of the lineups were conducted in Joliet. Joliet’s results

pick at all, every single eyewitness who did make a pick using the non-blind simultaneous procedure selected the person police believed was the suspect. As one expert has dryly commented, “it is puzzling to see 152 lineups in which no eyewitness chose a filler.”<sup>29</sup> Of course, it is more than “puzzling,” it is a sort of perfection that runs counter to human nature and to the results of other field studies, which have examined more than 3000 *status quo* non-blind lineups, and have reported a typical filler selection rate of around twenty (20) percent.<sup>30</sup>

The control group results also contained at least one other extraordinary anomaly: They purport to suggest that witness identifications *improve substantially as time passes*—that is, data from the non-blind simultaneous group suggests that witness procedures conducted more than thirty days after the incident were more accurate than ones conducted immediately after the incident.<sup>31</sup> This is yet another conclusion from the Illinois Study that is irreconcilable with both human experience and the psychological research on human memory generally,<sup>32</sup> and provides yet another reason the results cannot be trusted.

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differed wildly from both Chicago’s and Evanston’s. All of the control group filler picks took place in Joliet and, in fact, Joliet’s results indicated that the blind-sequential worked better than the control group used there. It is unclear why Joliet’s results differed so substantially, although Joliet did use different procedures than the other two cities. See ILLINOIS REPORT, *supra* note 15 at Exhibit 17; STEBLAY, *supra* note 24 at 5. See also, David Feige, Op-Ed., *Witnessing Guilt, Ignoring Innocence?*, N.Y. TIMES, June 6, 2006, at A21 (“And then there is the fact that the supposed superiority of simultaneous lineups was detectable in the study’s cases in Chicago but not in the nearby Joliet police district. Could it be that big city officers were pushing witnesses toward identifications in circumstances in which their smaller-town brethren are more circumspect?”).

<sup>29</sup> STEBLAY, *supra* note 24 at 3.

<sup>30</sup> Tim Valentine et al., *Characteristics of Eyewitness Identification that Predict the Outcome of Real Lineups*, 20 APPLIED COGNITIVE PSYCHOL. 969 (2003); A. Slater, *Identification Parades: A Scientific Evaluation* (Police Research Award Scheme, Police Research Group, Home Office, 1994) (reported in Tim Valentine & Pamela Heaton, *An Evaluation of the Fairness of Police Line-Ups and Video Identifications*, 13 APPLIED COGNITIVE PSYCHOL. S59 (1999)); D.B. Wright & A.T. McDaid, *Comparing System and Estimator Variables Using Data From Real Lineups*, 10 APPLIED COGNITIVE PSYCHOL. 75 (1996); B.W. Behrman & S.L. Davey, *Eyewitness Identification in Actual Criminal Cases: An Archival Analysis*, 25 LAW & HUM. BEHAV. 475 (2001).

<sup>31</sup> Simultaneous control group procedures produced a suspect identification rate of 56.7% for procedures conducted within 48 hours after the incident. The suspect identification rate produced by the simultaneous control group procedures improved to 67.1% for procedures conducted more than 31 days after the incident. ILLINOIS REPORT, *supra* note 15 at Exhibit 17, tbl.11.

<sup>32</sup> See, e.g., CUTLER & PENROD, *supra* note 21 at 105 (“Common sense tells us that memory declines over time.”); Sigfried L. Sporer, *Psychological Aspects of Person Descriptions*, in PSYCHOLOGICAL ISSUES IN

It appears, in fact, that even the authors of the Illinois Study do not take its results seriously. If the authors of the Illinois Report really believed in its methodology, they should have concluded that Illinois's non-blind simultaneous procedures were working perfectly, and recommended that the Legislature move on to reform other areas of the criminal justice system. But even the Report's authors could not support such a rosy view of the *status quo*, and it is telling that they did not even try. Instead, the authors identified ten additional areas where further study and research were needed.<sup>33</sup>

Decades of peer-reviewed research consistently supports the effectiveness of the double-blind sequential procedure in decreasing false identifications. Other field studies, like the one in Hennepin County, Minnesota, have confirmed these results. However, the defense community welcomes additional serious research on reform procedures, both in the field and in the laboratory. It is in everyone's interest to know which procedures work best and how to implement reforms in the field in the fairest, most efficient way possible. No one in the defense community has ever insisted on implementation of reforms that are not supported by serious, credible research.

But studies like this—pervaded by design flaws, conducted in secret by vigorous opponents of reform, and accompanied by a highly-politicized press strategy<sup>34</sup>—do not enhance our collective understanding at all and serve only as misleading diversions from the important questions posed by the demonstrated failures of current identification procedures. The Illinois Legislature gave the criminal justice community a marvelous opportunity to conduct a serious,

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EYEWITNESS IDENTIFICATION 53, 73–75 (Siegfried Ludwig Sporer, et al. eds., 1996) (discussing the effects of delay on retention and retrieval).

<sup>33</sup> ILLINOIS REPORT, *supra* note 15 at 61–65.

<sup>34</sup> See, e.g., Kate Zernike, *Questions Raised Over New Trend in Police Lineups*, N.Y. Times, Apr. 19, 2006, at A1. See also, ON BEING A SCIENTIST, *supra* note 25, at *Publication and Openness* (emphasizing the importance of submitting “important and controversial results” to peer review before releasing them to the public through the general media).

large-scale study of the efficacy of eyewitness reform procedures, one that could have been vital to correcting flaws in our current system. Instead of accepting the opportunity, the Illinois Report ignored the guidance of the Legislature and conducted a valueless exercise at substantial taxpayer expense.

Sadly, this means we will have to look elsewhere for serious studies about how new eyewitness procedural reforms can be implemented in the field. And while we wait for more corroboration of just how flawed current identification procedures are, we give many jurisdictions an excuse to prolong the unacceptable *status quo*, allowing more innocent people to become the victims of mistaken identification testimony in Illinois and around the country. Perhaps that is what the Illinois Report was designed to achieve. It is our job as defenders to make sure it does not succeed.

### *Five Important Facts About The Illinois Report*

- *The Illinois Report contradicted the directions of the Illinois Legislature by failing to “adopt a protocol for the selection and administration of lineups which is practical, designed to elicit information for comparative evaluation purposes, and is consistent with objective scientific research methodology.”*
- *The Illinois Report was conducted under potentially biased circumstances: The pilot program was conducted by the same Chicago Police Department that had vigorously opposed the reforms, the police department did not provide the chair of the Legislative Commission in charge of overseeing all of the reforms access to the study’s protocols, and the police who participated in the pilot program were trained on the nature and purpose of the pilot project despite the fact that doing so runs contrary to scientific methodology;*
- *Instead of creating a control group that would have allowed for meaningful comparison of sequential lineup procedures with simultaneous procedures, the Illinois Report used a hodge-podge of existing practices in three police departments as its control group, so that the study never could have produced any useful information about the difference between sequential and simultaneous lineups;*
- *The Illinois Report used a methodology that would have counted every DNA exoneration case as a “correct” identification, and rewarded the use of suggestive police procedures by counting every “suspect” identification as an accurate identification of the true culprit. Such a methodology cannot produce useful results when applied to police procedures, like the non-blind ones used in the simultaneous procedure control group, where the administrator knows who the suspect is and thus has the ability to suggest that information (consciously or unconsciously) to the witness; and*
- *Taken at face value, the Illinois Report purports to show that the old system of identification procedures worked perfectly. The results claim that, out of 152 lineups in Chicago and Evanston, every witness who actually selected someone from the lineup or photo-spread did so correctly. The Report also purports to show that witness identifications get better as time passes after the incident. These results conflict with common sense, with the DNA exonerations throughout the country, and with independent field studies that involved over 3000 similar lineup procedures, and which showed an error rate of 20%. Viewed in context, the Illinois Report’s 0% error rate for the control groups of two of the three jurisdictions, and its conclusion that memory improves over time, cannot be accepted as credible.*