

The Illinois Field Study: A Significant Contribution to Understanding Real World Eyewitness Identification Issues

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The Illinois Pilot Program on Eyewitness Identification

In 2006, Illinois law enforcement released the Report to the Illinois Legislature: Illinois Pilot Program on Sequential, Double-Blind Lineup Procedures (“Illinois Pilot Report”), setting forth the data from a year-long, multi-jurisdictional

field study examining the efficacy of the sequential, double-blind lineup method. The Illinois data showed that the sequential, double-blind lineups had a known error rate, measured by filler identifications, of 9.2%, whereas the traditional (i.e., simultaneous, non-blind) lineups had a known error rate, also measured by filler identifications, of 2.7%.

The Illinois Pilot Report represents a significant advancement in the study of eyewitness identification. Prior to the Illinois Pilot Report, the advocates of the sequential, double-blind lineups relied upon laboratory studies to support calls for policy changes in eyewitness identification. Some practitioners viewed the laboratory studies with skepticism because, among other things, the laboratory studies do not capture the real-world factors vital to assessing the reliability of an eyewitness identification. The laboratory experiments generally attempt to imitate only brief, non-violent stranger crimes, whereas many real crimes involve more complicated factors, such as the psychological experience of a real crime, authentic physical viewing conditions (as opposed to observing the viewing conditions on a video), the impact of participating in a police identification procedure, the effect of a witness’s prior familiarity with the offender, the variety of motivations of witnesses and victims, and the effect of understanding the multitude of consequences of making or not making an identification in a criminal case. The laboratory, unlike law enforcement, also does not “weed out” unreliable witnesses, but allows all research participants to be included in the testing regardless of whether the participant actually paid attention to the stimulus or believed that he could identify the culprit. The laboratory studies also are devoid of the influence of investigative techniques, which result in a significantly higher rate of target present lineups in real life than the 50/50 target

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absent/target present ratio often used in the laboratory. These investigative techniques also lead to either corroborating evidence, which verifies the accuracy of an identification, or exculpatory evidence, which corrects an inaccurate identification recorded as false in laboratory studies.

A handful of jurisdictions have adopted the sequential, double-blind method of lineups based upon these laboratory studies, but none had collected field data to evaluate the error rates of the sequential, double-blind lineup procedures. The Illinois Pilot Program on Sequential, Double-Blind Lineup Procedures (“Illinois Pilot Program”) represents the first field study in the nation to collect data on sequential, double-blind lineups. It is the first field study to collect concurrent data on traditional lineups from the same sources. It is also the first field study to offer a comparison between the two methods. Most importantly, the Illinois Pilot Program was the first, and still only, field study to submit its data to two separate experts for analysis, both of whom independently reached the same conclusions.¹

The results of the Illinois Pilot Program have caused vociferous debate, prompting concerns by researchers and practitioners alike over the viability and acceptance of future field studies. Critics of the Illinois Pilot Program have focused primarily on the design of the study, claiming that the confound of the design—i.e., comparing the two variables of the sequential, double-blind lineup to the two variables of the simultaneous non-blind lineup—has “devastating effects” on the value of the Illinois Pilot Program (Schacter et al. 2007, this issue). These critics argue that the results likely are attributable to improper police influence in the non-blind traditional lineups. This article, written by invitation to respond to the Schacter article, discusses three reasons why the Illinois data is a significant and valuable contribution to the study of eyewitness identification: first, the confound is inherent to evaluating the confounded policy recommendation of changing from the traditional eyewitness identification method to the sequential, double-blind method; second, the Illinois data can readily be analyzed outside the context of the confound simply by eliminating the comparison between the two sets of data; and third, a thoughtful analysis of the data shows that the results are not the product of police influence.

¹ The data analysis for the Illinois Pilot Program was conducted by two independent teams of researchers, headed respectively by Professor Roy Malpass of the University of Texas, El Paso and Professor Ebbe Ebbesen of the University of California at San Diego, both of whom have qualified as experts in courts around the country and have researched and written extensively in the area of eyewitness identification. Despite the misnomer, “The Mecklenburg Report,” the coordinator of the project had no role in the analysis of the data.

The Comparison of Confounded Procedures Is a Valid First Step in Assessing Confounded Policy Recommendations

Beginning in the 1980s, eyewitness researchers undertook a series of laboratory experiments concluding that the sequential, double-blind method of identification was superior to the simultaneous, non-blind identification procedure; these experiments led to the recommendation of a wholesale policy change from the simultaneous, non-blind identification procedure to the sequential, double-blind method of identification. At least one seminal study underlying this policy recommendation relied upon the experimental design of comparing the sequential, double-blind procedure to the simultaneous, non-blind procedure, the very confound later mirrored by the Illinois study (Lindsay and Wells 1985). Studies claiming to demonstrate the “sequential superiority effect” have continued to employ confounds. In Cutler and Penrod (1988), the simultaneous lineups were presented by both photo array and video, while the sequential lineups were presented only by video. In Lindsay, et al. (1991) (Experiment 1), the simultaneous lineups employed different lineup instructions, lineup member dress and foil similarity than the sequential lineups. In Sporer et al. (1993), the sequential presentation of photos randomized the positions, whereas the simultaneous lineups did not. In Morgan et al. (2004), the study assembled simultaneous live lineups consisting of 15 members and simultaneous photo arrays with 16 photos, each 2” × 2.5”, and compared them to sequential photo arrays using 16 photographs, each projected on a 6’ × 8’ screen; further, only the sequential procedures containing a sub-group of lineups cued by the target wearing the same clothing worn during the “crime.” Hence, some of the very laboratory studies which form the basis of the policy recommendation are, in themselves, confound studies.² The

² The research corpus which forms the basis of the policy recommendation to adopt sequential lineups is also unclear, in that it does not employ a consistent sequential method in testing this procedure. For instance, some studies use the “stopping” rule, ending the procedure upon an identification, whereas other studies continue to show the entire set of photos; some studies double-blind the task, while others do not; studies use widely varying numbers of photos in the arrays; some studies hold the target’s position steady while others randomize the target’s position; and some studies allow more than one round through the sequential presentation, whereas others do not. McQuiston-Surrett et al. (2006). It is difficult to assess the claim of “sequential superiority” when the very definition of the sequential lineup being tested differs from one experiment to another. Although some may argue that the emergence of the sequential superiority from varying experiments demonstrates the “robustness” of the results, this argument does not address the issue of what is causing the results, and whether it truly reflects the sequential presentation or some other factor in the mix-match of experiments. Further, given that a number of studies which did not find a sequential superiority effect went unpublished, we do not know why those experiments or their designs

presence of the confound in the Illinois study is no more “devastating” to the value of that study, than the confounds contained in the laboratory studies are to their value in assessing the sequential, double-blind method of identification.

When advocates sought policy changes in Illinois (and in other jurisdictions), they recommended that law enforcement change from the simultaneous, non-blind lineups currently in use to the sequential, double-blind method of lineups. Indeed, the advocates pronounced that the two changes went hand-in-hand: the sequential method of presentation eliminated the alleged use of relative judgment by witnesses, and the blind administrator eliminated the potential for police influence, a potential which is even greater with the sequential presentation. As a result of this dual recommendation, Illinois adopted legislation mandating the testing of these two recommendations together, i.e., to test the efficacy of the double-blind, sequential lineup method.³

Schacter et al. (2007, this issue) now argue that because the Illinois study was not designed to isolate the blind administrator factor by assessing yet a third type of lineup—blind, simultaneous lineups—then “the Illinois study addressed a question...that is not worth addressing...” Yet, this is exactly the “question” posited by researchers around the country. In response to this particular dual recommendation, the Illinois study provided law enforcement with the first field data on the effects of such a wholesale change in identification procedures, a necessary step to evaluating the recommendation. The Illinois Pilot Program contributed valuable information to assessing the confounded recommendation, as well as providing a basis to consider modified recommendations, such as blind simultaneous lineups. Although Schacter et al. (2007, this issue) suggest that the Illinois Pilot Report recommended no changes be made to eyewitness identification procedures, the Illinois Pilot Report recommended ten areas “ripe for future study,” including blind administration and “additional lineup methods.” Indeed, since the Illinois results were released, some researchers have been emphasizing the use of the blind administrator and relegating the sequential lineup presentation to an optional status (Wells 2006).

Footnote 2 continued

resulted in a lack of sequential superiority. These unanswered questions are at the very heart of the call for field studies before embarking on wholesale policy changes.

³ A fallacy perpetuated throughout this debate is that the coordinator of the Illinois Pilot Program designed and directed the program alone. (Schacter et al. 2007, this issue) In fact, the program was designed in consultation with multiple experts and others who had been instrumental in drafting and overseeing the legislative mandate, and was directed in regular consultation with experts and policy makers alike.

The Illinois Pilot Program Can Be Evaluated Independent of the Confound

The confound of the Illinois Pilot Program does not necessarily affect scientific analysis of the data, because the Illinois data on sequential, double-blind lineups can be evaluated independently of the confound, simply by eliminating the comparison between the traditional lineups and the sequential, double-blind lineups. Looking exclusively at the Illinois sequential, double-blind lineups, the issue is whether the 9.2% known error rate associated with those lineups is acceptable, particularly when compared to other eyewitness methods which have been safeguarded against police influence. This mirrors the approach taken, to many accolades, by the Hennepin County study, which collected data on only one type of lineup and then sought other reliable data for purposes of comparison (Klobuchar et al. 2006).⁴

The traditional lineup data collected by the Queens District Attorney’s Office in New York City (reported in the Illinois Pilot Report) presents a reliable source of comparison, because the presence of a prosecutor at each of the Queens lineups safeguarded against improper police influence in those lineups. Certainly, if the prosecutor, whose purpose and duty is to ensure the integrity of the lineups, did not observe any improper cues, then it is reasonable to assume that the witness, who is focused on viewing the lineup, also did not observe such cues. The

⁴ Although the Hennepin County study is lauded by Schacter et al. (2007, this issue) and others as an example of a successful pilot program testing sequential, double-blind lineups, that study did not even involve sequential, double-blind lineups as advocated by the laboratory studies. Despite the claim that each witness was required to make a decision after each photograph (Klobuchar et al. 2006), the prosecutors who designed the study and conducted the training have repeatedly and publicly acknowledged that the Hennepin County study protocol called for an identification only at the end of the presentation of the entire photo array. Remarks of Hennepin County Assistant Prosecuting Attorney, at: Cardozo School of Law Symposium, Reforming Eyewitness Identification: Convicting the Guilty, Protecting the Innocent (New York City, September 2004), Loyola School of Law Conference, New Policies, New Practices: Fresh Perspectives on Eyewitness Identification (Chicago, April 2006), Police Executive Research Forum Conference (Washington, D.C. May 2006). The Hennepin County training material confirms that there was no protocol instructing a witness to make a decision after each photograph. This is contrary to the recommended sequential method of requiring a choice on each photograph individually, designed to eliminate “relative judgment” by the witnesses. The Hennepin County study remains valuable for examining blind lineups where the witness makes a decision after seeing all the choices, but continued reliance on the Hennepin County study as an assessment of “sequential, double-blind lineups” is misplaced. Moreover, data are being collected to ascertain identification rates in Hennepin County simultaneous, non-blind lineups for the period just prior to the pilot program, to provide a “helpful” control group comparison (Klobuchar et al. 2006). This proposed comparison contains the same confound as the Illinois study.

Office of the District Attorney in Queens collected data on live lineups over a 5-year period, which showed a known error rate of filler identifications ranging from .58 to 5.62%, significantly lower than the 9.2% error rate of filler identifications contained in the Illinois sequential, double-blind lineups. As we collect additional field data on different types of lineups, comparison to the known error rate of the Illinois sequential, double-blind lineups will continue to be invaluable.

The Illinois Pilot Results Cannot Be Presumed to Be the Product of Police Influence

Researchers advocate the use of blind administrators in eyewitness identification procedures in the field to prevent police from inadvertently cuing the witnesses to identify the suspect. Some researchers have suggested that the lower rates of filler identifications in the Illinois non-blind simultaneous lineups are the result of such cues (rather than a qualitative difference between the simultaneous and sequential presentations), proving that police influence eyewitness identification procedures on a systematic basis.

The critics argue that police influence is the most likely explanation for the Illinois results, because the Illinois study contradicts years of laboratory research showing the sequential superiority effect. In fact, the Illinois data is not necessarily contrary to the laboratory studies. Not all laboratory studies, particularly the unpublished studies, support the claim of superiority with the sequential method of identification (Stebly et al. 2001). Moreover, to the extent that certain laboratory studies have concluded that the sequential presentation is superior to the simultaneous method, the results of at least some of those studies may not be attributable solely to the method of presentation, as discussed in Sect. “The Illinois Pilot Program on Eyewitness Identification” of this article. Any contradiction between the Illinois field study and laboratory studies also may reflect the fact that laboratory studies cannot reliably capture the real-life factors that affect eyewitness identifications in the field, as discussed in Sect. “The Illinois Pilot Program on Eyewitness Identification” of this article.

Both the laboratory and the field research on blind administrators show that an assumption of police influence over lineup identifications is unwarranted. Researchers advocating the blind administration of all eyewitness identification procedures in the field argue that the use of blind administrators is necessary in the field, as in the research experiments, to ensure objectivity of data collection and interpretation and to prevent administrator influence. Yet, blind administrators are not widely used by researchers in conducting studies of eyewitness identification, including lineup presentation (e.g., Lindsay and Wells

1985; McQuiston-Surrett et al. 2006). Although the lack of blind administrators in the experimental research may not threaten comparative results when both conditions are non-blind, each of the non-blind research studies as a whole could be tainted with administrator bias. This may affect a wide variety of eyewitness laboratory experiments, ranging from sequential superiority to cross-racial bias to weapon focus. If an assumption of non-blind administrator influence is unwarranted in the eyewitness research studies, where the administrators face no consequences for such influence, then certainly such an assumption in the absence of concrete evidence to the contrary is not warranted in the field, where the officer can face serious consequences from influencing an identification process, such as: the loss of an accurate identification, the ramifications of a false identification in which an innocent person could be convicted and the guilty offender goes free, disciplinary action and civil liability.

In addition, the only published laboratory study isolating the effects of blind administration on the sequential and simultaneous lineups found that an administrator’s knowledge of the identity of the suspect increased false identifications *in sequential arrays only*, and even then only when there was an observer present who also knew the identity of the suspect (Phillips et al. 1999). The Phillips et al. study showed no administrator influence in the non-blind simultaneous identification procedures.

Similarly, the only field data to date isolating the effect of a blind administrator, collected by the New York City Police Department (and reported in the Illinois Addendum), shows an absence of systematic police influence in simultaneous non-blind lineups. In comparing simultaneous blind lineups with simultaneous non-blind lineups, NYPD data from 1,052 live lineups held in Brooklyn showed virtually identical identification rates. The use of, or lack of, a blind administrator, did not affect the choice rates made by the witnesses, indicating that there was no systematic police influence over the lineups. There simply is no concrete evidence to support the claim that inadvertent police influence—which requires not just that officers give cues, but also that witnesses correctly assess those cues and then accurately use those cues to choose someone that the witness would not have chosen in the absence of the cues—occurs on a systematic basis in real world identifications, where the motivations and consequences are very different than those in the laboratory experiments.

Finally, the Illinois data itself does not support the assumption that Illinois law enforcement from three separate jurisdictions systematically influenced the non-blind lineups in the Illinois Pilot Program. The Illinois data are consistent with other field data which are not attributable to police influence. The Illinois data are consistent with the Queens data, which we know is not the product of

systematic police influence due to the additional safeguard of a prosecutor's presence at each lineup. The Illinois data also are consistent with the data collected in Hennepin County, when the Hennepin County data are properly adjusted to exclude the influence of "accurate" identifications by those who previously knew the suspect (Stebly 2007).

A scientific analysis of the Illinois data further demonstrates that police influence over the witnesses was not the likely source of the higher suspect and lower filler identification rates in the Illinois traditional lineups (Ebbesen and Finklea 2006). Ebbesen and Finklea (2006) analyzed patterns of identification where the witness had prior familiarity with the suspect compared to stranger crimes, patterns of witness confidence and patterns of cross-racial identification. In those situations where police influence would have made a noticeable difference, no evidence of police influence appeared. Based upon their scientific analysis, Ebbesen and Finklea (2006) concluded that the patterns were "inconsistent with the investigator influence explanation." Malpass, likewise, has noted the absence of any evidence of police influence in the study (Malpass 2006).

The position of Schacter et al. (2007, this issue) regarding the "devastating" impact of the confound, rests on an assumption that the non-blind administrators influenced identification decisions in the Illinois study. There is no independent evidence to support this assumption and, indeed, the evidence suggests that no such influence occurred on any significant or systematic basis (Ebbesen and Flowe 2006; Malpass 2006). Thus, we question the conclusion that the confound is "devastating" to the value of the Illinois study.

The Illinois Study Contains Valuable Data Unrelated to the Confound

The Illinois study provides rich field data on a variety of eyewitness issues unrelated to the confound of comparing blind sequential to non-blind simultaneous procedures. For instance, the Illinois data showed that, contrary to some laboratory experiments, cross-racial identifications across the board did not result in a higher rate of filler identifications than did same-race identifications. Instead, the cross-racial identifications in the field resulted in a higher rate of non-identifications, which are not considered false identifications in the field. Similarly, the Illinois data showed no difference in identifications based upon being a victim or a witness, the use of a weapon or the threat of violence. There has been almost no discussion of the potentially far-reaching implications of this field data. Overlooking the significant value of this field data is

detrimental to the study of real world eyewitness identification and, ultimately, the quest for the truth.

Conclusion

The continued debate over the effect of the "confound" in the Illinois study has obscured, intentionally or not, the significant value of the Illinois data. The confound of the Illinois study has value in addressing the confound question that the researchers have posed, namely, whether policy should replace the non-blind, simultaneous eyewitness identification methods with double-blind, sequential procedures. The Illinois study also offers data which can be analyzed independent of the confound, as well as data which is unrelated to the confound. The value of this data should not be dismissed on a hypothesis of police influence over the non-blind simultaneous lineups: such a hypothesis is at odds with the lack of blind administrators that pervades the laboratory research on eyewitness identification, the limited laboratory research directed at blind administration, the limited field data on blind administrators, and the patterns emerging from the Illinois data itself.

As prosecutors, we have an allegiance only to the truth, not to any particular method of eyewitness identification procedure. We believe that the truth will be found in field studies, which flesh out the issues that laboratory research cannot address and provide an acceptable basis for evaluating eyewitness identification procedures. Therefore, we cannot dismiss the value of a field study such as the Illinois Pilot Program, which has great potential to add to the body of knowledge on eyewitness identification. We welcome further field studies, and look forward to an ongoing dialogue over the evolving principles of eyewitness identification.

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