Law Hum Behav
DOI 10.1007/s10979-007-9104-x

ORIGINAL ARTICLE

Moving Forward: Response to "Studying Eyewitness Investigations in the Field"

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Abstract Field studies of eyewitness identification are richly confounded. Determining which confounds undermine interpretation is important. The blind administration confound in the Illinois study is said to undermine it's value for understanding the relative utility of simultaneous and sequential lineups. Most criticisms of the Illinois study focus on filler identifications, and related inferences about the importance of the blind confound. We find no convincing evidence supporting this line of attack and wonder at filler identifications as the major line of criticism. More debilitating problems impede using the Illinois study to address the simultaneous versus sequential lineup controversy: inability to estimate guilt independent of identification evidence, lack of protocol compliance monitoring, and assessment of lineups quality. Moving forward requires removing these limitations.

Keywords Eyewitness policy · Eyewitness field studies · Lineup reformation

Schacter et al. (2007, this issue) comment on the controversy surrounding implementation of sequential lineup presentation as a replacement for simultaneous lineup presentation, and the findings and implications of the Illinois Pilot Program on Simultaneous versus Sequential Lineups (hereafter, "the Illinois study"; Mecklenburg 2006). Their major focus is on interpreting the impact of the confound resulting from sequential lineups (SEQ) being administered with the administrator blind to the identity of the suspect while simultaneous lineups (SIM) were not. We discuss the interpretation of this confound and the larger interpretation questions of the Illinois study, but first we feel compelled to state our position on the SEQ controversy.

Schacter et al. (2007, this issue) treat the controversy as a unidimensional question of favoring or not favoring some form of SEQ lineup. This does not adequately describe our position. Our primary concern is that psychological scientists will have to take back the recommendation that SEQ lineups will solve—or even go a long way to diminish—the problems of false identification and therefore wrongful conviction. Questions about the adequacy of a research domain for supporting sweeping policy changes have not been resolved, and criteria for making such judgments about domain adequacy remain largely undeveloped (but see Malpass, MacLin, Zimmerman, Tredoux, & McQuiston, 2003, July). Our focus on the strength of both the theoretical and empirical base (McQuiston et al. 2006) are well known, as is our analysis of the utility of SIM and SEQ as policy alternatives and the conditions under which this might change (Malpass 2006a, b). While SEQ advocates favor a particular family of lineup procedures, we favor a broader search for ways to confront identification errors—both failures to identify offenders and failures to reject identification of innocent suspects—that are theoretically well understood and empirically stable.

Of particular relevance for the present exchange, it is also desirable to understand the implications of the confounds between SIM and SEQ in the vast majority of comparison studies in the laboratory. Important techniques that are part of the SEQ "package" have not been studied in combination with SIM to determine if they contribute to lowering false identifications, or if they are actually the active ingredients in the effects of SEQ instead of

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sequential presentation. Given that there is so much left unresolved we believe it premature to advocate policy change, especially since the policy communities are so dispersed and since psychological science will both take a black eye and have difficulty implementing alternative policies if current advocacy is found to be incorrect, oversold or both.

Our opposition, therefore, is not focused on SEQ per se, but rather on issues concerning the conditions under which a policy should be advocated with the claim that the underlying science is adequate. That the concern for the policy adequacy of a scientific research domain should take the SIM/SEQ lineup question as its focal point is unfortunate in some ways, because it gives rise to the perception that these are SIM versus SEQ advocacy oppositions. Instead, we believe that both the theoretical and empirical bases for the policy adequacy of SEQ are inadequate, and that there currently is not a single and sovereign solution to the lineup administration problem.

This discussion takes place in the context of a multi-jurisdictional advocacy focused on replacement of traditional SIM presentation (Wogalter et al. 2004) with some form of SEQ presentation. The Illinois study has been interpreted by some as weighing generally against the adoption of SEQ in practice across the nation. While we think there are good reasons not to adopt SEQ wholesale (Malpass 2006a, b; McQuiston et al. 2006), we also think that the Illinois study does not speak clearly on the general superiority of either SIM or SEQ lineups. In contrast to argument based on empirical evidence (Ellsworth 1999), commentary advancing speculative interpretations of the results, the design, and the intentions of the participants in the Illinois study process has appeared on the internet and throughout a range of media outlets. Lawsuits have been threatened and justice advocacy organizations have taken sides.

Schacter et al. (2007, this issue) join a group of at least 16 separate criticisms of the Illinois study published either online or in print (Diamond in press; Doyle et al. 2006; Feige 2006; Innocence Project 2006; Lieberman 2006; Malpass 2006a; O'Toole 2006; O'Toole and Shay 2006; Paulson and Llana 2006; Sherman 2006; State of Wisconsin 2006; Steblay 2006; Sweeney 2006; Wells 2006a, b; Zernicke 2006). Of these 16, at least 14 have specifically cited the confound of blind/SEQ and non-blind/SIM as the major criticism of the Pilot Program. Few have provided useful insights into how future evaluations could better address that and other important questions about the place of field studies in policy development and useful study design improvements.

There is a lot riding on the perception of SEQ superiority, and no one should be surprised that the effort to solidify that perception has been vigorous following what we think is a needless interpretation that the Illinois study was an important threat to the accumulated evidence on SEQ.

As an aside, it is not true that *all* current proposals for change have "at their cores the novel, double-blind sequential technique" (Schacter et al. 2007, this issue). Some have separated reform of admonitions and lineup quality from a sequential lineup proposal (e.g., The Justice Project 2007). Reforms of admonitions and lineup quality improvement are uncontroversial and have long and consistent scientific histories.

Schacter et al. (2007, this issue) note that the Illinois study can be regarded as a particularistic evaluation of the relative merits of a set of existing procedures (SIM, non-blind) compared with a new set of procedures (SEQ, blind). This approach, pitting one package of procedural elements against another, is a traditional form of evaluation research commonly used in medical as well as criminal justice communities (Sherman 2006; see Ebbeson (2006) for a more detailed discussion of the importance of this type of evaluation in the Illinois study). Of course, comparisons of such complex "packages" as SIM and SEQ do not permit causal interpretation of the source of effects. One cannot know whether observed effects can be attributed to either the SIM/SEQ difference, the blind/non-blind difference or to the many other differences.

A particularistic evaluation approach may not be useful for causal analysis or an academic debate. Rather it may be concerned only with the relative performance of two (in this case) procedural options. A particularistic approach limits the utility of the study for broader issues. For the narrow, particularistic purpose the blind administration confound present in the Illinois study is not particularly important.

There is a larger context to which the Illinois study has been elevated, and it is in this context that Schacter et al. (2007, this issue) suggest that the blind confound is both important and disabling. After carefully examining the arguments and the available research, we find little evidence that the blind confound is important even for an academic interpretation of the Illinois study.

Throughout the literature on eyewitness identification, the major outcome variables have always been correct identification of guilty suspects and false identification of innocent suspects. For the Illinois study, and the majority of all field studies completed to date, it is not possible to move from suspect identifications to identification accuracy because supporting case information was not available. The only "ground truth" data available is filler identifications, which are (a) far lower in value than data on false and accurate identifications, and (b) uninterpretable in the face of recording lapses and both witness and investigator decision criterion issues, on which little or no

information is available. Schacter et al. (2007, this issue) state that the Illinois study has been controversial as it contradicts both the laboratory studies and "sparse existing field data" that have been collected to date. This is not entirely true. While the Illinois study does contradict the laboratory data in regards to the filler identification rate—suspect identification and choosing rates are consistent with what would be expected from laboratory studies (see Malpass 2006a for details). Further, while the results of the Illinois study differ from many field studies, there is considerable variation among the field studies that may explain these differences, a point which we will address later. Yet it is primarily on the grounds of the differences in filler identifications that the Illinois study has been criticized and on which "detractors" have based their arguments for the disabling importance of the blind confound.

The arguments arise from the "suspiciously" low filler identification rates in the non-blind SIM lineups. Schacter et al. (2007, this issue) claim that "Wells cites enough evidence that [the filler identifications] may be so low [in the simultaneous condition] to justify concern that administrator bias is operating, either consciously or unconsciously" (p. 5). While Wells' comments may be enough to justify concern over the influence of administrator bias, they do not justify the conclusion that administrator bias in fact influenced the results of the Illinois study. Wells (2006a) cites two experimental studies (Haw and Fisher 2004; Phillips et al. 1999) showing "that non-blind lineup administrators have precisely such effects" ("steering eyewitnesses away from the fillers and steering them toward the suspected person", p. 1). While these studies report that administrator expectancies can influence eyewitness choices, their results do not mirror the Illinois study. Neither study found that administrator expectancies influenced filler identifications. Haw and Fisher (2004) found that "high contact" between a non-blind administrator and the eyewitness during the identification procedure increased false identifications of a designated innocent suspect in SIM lineups, but found no differences in filler identification rates. Phillips et al. (1999) found that administrator expectancies were biasing in SEQ but found no evidence for administrator bias in SIM. Thus the evidence cited by Wells (2006a) is unrelated to the filler identification findings of the Illinois study. There are two other studies in the small literature on blind versus non-blind effects (Haw et al. 2003; Russano et al. 2006), that do not find a bias effect (Malpass 2006a).

We do not argue that non-blind administration does not allow for potential biasing of the eyewitness' decision—clearly mechanisms and opportunities exist for non-blind investigators to implement such biases. But rather than speculate about possible effects, substituting data for argumentation is a better approach (Ellsworth 1999). Ebbeson and Finklea (2006) examined possible effects that would be observed were such biases present in the Illinois data including the potential influence on cross-race identifications, "stranger" identifications, and witness confidence. Their analyses provided little support for the bias hypothesis.

Taking these findings together with the failure of studies cited by Wells (2006a) to explain the Illinois study findings, there is little evidence that the filler identification rate in simultaneous lineups is a product of administrator bias.

It is interesting to note that most laboratory studies comparing SIM and SEQ do not study blind administration and do not report its implementation (McQuiston et al. 2006). The potential for differential effects of non-blind administration on SIM and SEQ is present in most studies that make up the body of literature on which the claim of SEQ superiority is based.

The filler identification rates in the non-blind SIM lineups may have other alternative explanations. Some commentators have compared them to those found in other field studies (Steblay 2006; Wells 2006a). While the Illinois study filler identification rate is significantly lower than that found in other field studies, other attributes of prior field studies may explain this discrepancy (Ross and Malpass 2007). Lower filler identification rates are associated with lineups having fewer members and shorter delays before an identification procedure. Illinois study lineups were smaller in size than most of the other field studies (5- or 6-members, compared with 8- or 9-members) and occurred after a shorter average delay. While we cannot claim these differences are the cause of the low filler identification rate, they do provide an alternative to the "administrator bias" explanation.

Another explanation may come from the absence of protocol compliance monitoring. Wells (2006a) acknowledges that in previous field evaluations police reports made no distinction between filler identifications and lineup rejections (e.g., Behrman and Davey 2001; Pike et al. 2002; Tollestrup et al. 1994). While the Illinois study protocol stipulated that the investigators were to indicate filler identifications as such, and not to consider them non-identifications, compliance levels are unknown. Other evidence suggests that the Illinois study protocol often was not followed. The protocol stipulated that investigators were to gather and report confidence statements for all sequential lineup identifications. Only 62% were accompanied by confidence statements. This problem is common to most field studies on eyewitness procedures conducted to date. Klobuchar et al. (2006) report under-reporting of confidence statements in the Hennepin County evaluation of sequential lineups. Even though stipulated in the protocol, confidence statements were recorded in only 15% of the lineup reports. With protocol compliance monitoring

not included in the study, low reported filler identification rates may be a function of poor reporting and not indicative of the actual filler identification rate.

Regardless of the actual reasons for the low filler identification rate, it is not clear why the results of the Illinois study would be accorded significance for interpretation of the relative merits of SIM and SEQ in the academic debate, because there are so many good reasons why it does not/should not contribute at that level. It would be really unfortunate if adoption of SEQ procedures in law enforcement jurisdictions were to become a sign of success independent of the academic debate. If appropriate interpretive information is not present, the actual value of any lineup procedure cannot be known, regardless of perception and advocacy. In our view the policy formation and implementation ought to come after the academic question is settled by good and relevant research. And for this and many other reasons (Malpass 2006a, b; McQuiston et al. 2006) we would not consider the Illinois study highly informative for the more general question of the conditions under which either SIM or SEQ is a preferable lineup administration procedure.

There are many important limitations inherent in field settings that serve to prevent these evaluations from addressing the academic questions. Most limitations of the Illinois Study are part of every field study that has been conducted on eyewitness procedures.

(1) Knowledge of the guilt or innocence of each individual suspect, based on case files and independent of identification evidence, is required to determine identification error rates. Instead, most field evaluations rely upon assumptions and estimates of ground truth. Many, like the Illinois and Hennepin County studies, utilize suspect identifications and filler identifications as proxies for identification accuracy in the interpretation of their findings. The use of suspect identifications as a proxy for accuracy is clearly problematic as we know innocent individuals are sometimes suspects in criminal cases and the authors of the Illinois and Hennepin County reports acknowledge this limitation.

Filler identifications are often utilized as a proxy for accuracy since any identification of a filler is a known error. Perhaps this is the reason why so much interest is being placed on the filler identification rates in the Illinois study since the utility of suspect identifications is not clear. Wells (2006b) discusses the limitations of using filler identifications as a proxy for accuracy in any field study, including the Illinois study. Among other conditions, he argues that only when blind administration of a lineup is employed can filler identifications serve as an indicator of accuracy.

Even under the best circumstances, the use of suspect and filler identifications as proxies of accuracy does not produce an environment where any field evaluation can answer the scientific question. And as we noted above, filler identifications are sensitive to recording lapses and witness and investigator judgments whose influence cannot be reliably estimated. Attempting to reach some meaningful interpretation through filler identifications is grasping at straws. The general, academic question about the superiority of any lineup administration procedure cannot be answered without knowledge about the two kinds of identification error and this requires independent information about suspect guilt.

(2) Compliance with study protocols—analogous to manipulation checks in laboratory studies—is important to interpreting the results of any field study. Without knowing that the protocol is implemented as directed, we cannot know whether the differences are attributable to the protocol or some deviation from it.

Appropriate reporting is one method for obtaining protocol compliance information. Conjectures can be formed about the degree to which certain procedures are being used (i.e., standard admonitions), but as noted above, reporting is not reliable. Poor reporting standards in field studies echoes reporting standards in laboratory studies, which in both settings undermines their utility (McQuiston et al. 2006).

(3) Assessment of lineup quality is as important in field studies as it is in the laboratory. It is well known that lineup quality is related to identification rates (Lindsay and Wells 1980; Malpass 1981; Malpass and Devine 1983; Brigham et al. 1999; Lindsay et al. 1999; McQuiston and Malpass 2002; Tredoux 2002). The quality of the lineups cannot be determined without access to the lineups themselves. Access to the actual lineups was not available in the Illinois study.

Treating the Illinois study as an attempt to answer the scientific questions in the SIM versus SEQ controversy is erroneous, inappropriate and fruitless. This and other field studies demonstrate the need for a high degree of access to detailed information about study procedures, case files and the actual lineups themselves. Continued wrangling about the field studies of the past is counterproductive. We agree with Schacter et al. (2007, this issue) that developing *desiderata* for future studies will be more productive, and that is where we turn next.

Sherman (2006) suggests an approach that combines the "testing" of programs in evaluation research with the "development" of programs in the experimental sciences. We agree with Sherman (2006) that the current trend of testing pre-formed program packages in the field is not the most effective way to discover how to enhance eyewitness accuracy in identification procedures. We would be better served by incorporating a broader and more flexible "development and testing" approach that incorporates both

the development skills of the scientist and the assessment skills of the evaluator. This approach combines the joint effort of law enforcement and social scientists to develop and implement field evaluations of eyewitness identification procedures in order to ascertain what procedures are the most effective at ensuring accurate eyewitness identifications. In particular, it requires serial access to program change, renewed implementation, and a base in law enforcement from which to work. Such a collaborative relationship with law enforcement has been elusive, but is the necessary step in advancing the quality of field research. Sherman's approach goes a considerable distance towards Geller's (1997) ideas of research and development activities situated in law enforcement organizations incorporating scientific collaboration.

But a development and testing approach is not enough. Future field evaluations must incorporate procedures to ensure that observed differences can be interpreted. These include (a) ensuring consistency in background variables across groups, thereby removing important confounds and manipulating only the focal variables of interest, (b) utilizing protocol compliance measures to ensure that procedures are implemented as mandated, (c) incorporating training programs for law enforcement implementing the procedures *prior* to study, (d) allowing for lineup quality assessment, (e) allowing researchers access to case files for estimates of probable guilt independent of eyewitness identification, and (f) improving reporting standards in the actual case reports and the final written manuscripts. These procedures will work to improve the likelihood that future field evaluations will be able to address both the particularistic and academic questions.

A new round of field studies currently under development includes many of these recommendations. With the collaboration of the American Judicature Society, the Innocence Project, the National Institute of Justice, the Urban Institute, and a number of eyewitness researchers, these studies are built around computer-based identification protocols utilizing randomized selection of SIM or SEQ and the lineup position of the suspect. These studies will be implemented with the collaboration of police departments of various sizes in a geographically and demographically diverse set of communities. Access by the research team to case files and lineups is a basic component of the collaboration agreements, so that the extremely important matters of lineup quality and accuracy, rather than mere suspect identification, can be addressed. The projects incorporate training programs for law enforcement, utilize protocol compliance monitoring and recording of identification sessions, and improve reporting standards.

The field studies currently under development are the first steps towards the development of a collaborative approach for assessing the effectiveness of eyewitness identification procedures, and signal a new level of sophistication in the study of eyewitness identification in the field. Focusing on the limitations of previous field evaluations can provide directions for change, but there is no future in continued fixation on the past. Let's move forward.

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