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Lindsay, R. C. L., Mansour, J. K., Beaudry, J. L., Leach, A. -M., & Bertrand, M. I. (2009).
Beyond sequential presentation: Misconceptions and misrepresentations of sequential lineups.
Legal & Criminological Psychology, 14, 31-34. doi: 10.1348/135532508X382104
which has been published in final form at

<http://onlinelibrary.wiley.com/doi/10.1348/135532508X382104/abstract>. This article may be

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Beyond sequential presentation:

Misconceptions and misrepresentations of sequential lineups.

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Malpass, Tredoux, and McQuiston-Surrett (2009), hereinafter “MTM”, provide comments on the sequential lineup, research comparing sequential and simultaneous lineups, and the policy implications of this literature. We will comment on points of agreement and disagreement. First, we agree with the following:

1. Peer review, publication of results, and diversity of methods, procedures, and subject populations significantly contribute to the value of research as a basis both for psychological understanding and for recommended policy.
2. Absence of error, omission, and confounds make interpretation and application easier.

These conclusions are not revolutionary but seem to occupy a great deal of MTM’s thinking. We disagree with many things that MTM have to say but have room here only to address a few.

1. The sequential lineup is misrepresented throughout MTM’s paper. There is no such thing as a sequential lineup without the so-called “ancillary” aspects of the procedure. The sequential lineup was originally described as and continues to be a combination of several techniques that each contribute to making the use of a relative judgment strategy more difficult. Similarly misguided arguments have been raised about the cognitive interview (attempting to dismember it and then arguing that some lesser version remains the cognitive interview; Fisher & Schreiber, 2007).
2. The entire discussion of strong and weak forms of the sequential lineup procedure is dubious at best. The original study was not driven by the notion that sequential presentation *per se* was sufficient though we certainly believe it is a necessary component of the procedure. The logic was that relative judgments lead to a higher false positive rate and that witness’s reliance on relative judgments could be reduced through sequential presentation in combination with several other aspects of the procedure. We could have

called the procedure the “sequential - only let them see them once - must decide before proceeding – can’t know how many people they will see” lineup but that seemed too wordy. The notion that the importance of the various components could be isolated and tested is true and some work has been done on that issue. Further research would be useful.

3. In their discussion of the work by Lindsay, Lea, and Fulford (1991) MTM state that “they did not manipulate any aspect of the simultaneous lineup, thus missing an opportunity to disentangle the effects of sequential presentation and the ancillary manipulations” ...and thus.... “we do not know whether it is the sequential presentation itself that produces the apparent identification advantage, or whether the same advantage would be achieved if we harnessed the ancillary manipulations to the traditional, simultaneous lineup.” We assume that MTM intend to conduct a study that capitalizes on the opportunity we missed. We look forward to reading their method section in which they explain how they presented a simultaneous lineup that a) ensures that people do not return to looking at a face again after the first time they scan it, b) guarantees that people do not make a decision but later reconsider and change their decision, and c) presents the lineup without letting people know the number of lineup members. The sequential lineup procedure can be tested with and without each of the so-called “ancillary” procedures but it is difficult to imagine how these components can be combined with simultaneous lineups. The moment a new procedure is devised integrating any of these methods with a simultaneous lineup (e.g., by leading people to believe that more photos will be presented after the first array), there will be three procedures to discuss because the modified simultaneous lineup

will no longer be a reasonable representation of the method typically used by most police now.

4. Although the Lindsay lab counterbalanced less often than other labs, we also employed live staged crimes rather than video or slide presentations and never used a terminal affirmative. These variables (and others) are almost completely confounded with the lab, and as such, a meta-analysis cannot disentangle their influence. Further research is the only way to determine their independent influence (if any) on eyewitness accuracy.
5. MTM seem unable to decide if meta-analyses are good or bad. They draw conclusions based on their meta-analysis but then condemn the technique as not suitable: “Meta-analysis proposes a different way of resolving methodological questions, which is to keep an ‘open mind’ about whether the methods used in a study compromise its findings, rather than applying Campbell’s razor.” Actually, this is untrue (Rosenthal, 1991). There has been considerable debate in the meta-analysis area about the issue referred to in computing as GIGO or garbage in-garbage out. Another error in their discussion of meta-analysis is their characterization of Deffenbacher et al’s (2004) position. Deffenbacher et al. declined to use unpublished studies in their meta-analysis so that the results would be less likely to be challenged in court, not because they deemed unpublished studies unfit for inclusion in meta-analyses generally. Again, this issue has been discussed at length in the meta-analysis area as the issue of “file-drawer studies” and new techniques for dealing with the issue continue to be developed (e.g., Rothstein, 2008). Science favors the inclusion of unpublished studies with publication entered as a moderator in the analysis, exactly as Steblay et al. (2001) did.

6. MTM are aware that the counterbalancing hypothesis is not supported by the data (Stebly, 2007). They are aware that the difference between the Lindsay lab and others confounds counterbalancing with live presentation (live presentation was a variable listed in the Steblay 2001 meta-analysis). They argue that “Knowing, as any experimenter does, ... that the same experimental materials (e.g. face image stimuli, distracter tasks, simulated crime movies) tend to be used many times in any particular laboratory...” was their primary justification for initially performing the analysis using counterbalancing as a moderator. The confounding variable that they know about but avoid, live presentation, suggests that the Lindsay lab should be less rather than more prone to error based on this logic. Lindsay et al (1991b) report five sequential lineup experiments (36% of all studies from our lab) conducted with no overlap of confederates, lineups, or experimenters.
7. Finally, both Lindsay et al. (2009) and MTM agree that studies employing designated innocent suspects selected based on their similarity to targets overestimate false identification rates and recommend a calculation based on estimated false identification rates (false positive rate divided by nominal lineup size). MTM conclude that “Sequential lineups then lead to 4.67% false identifications whereas simultaneous lineups lead to 8.5% false identifications.” The following are reasonable estimates: There are approximately 75,000 lineup procedures conducted in the United States annually. The suspect is innocent in many of these (we believe at least 20% and possibly as many as 50%). A single identification unsupported by other evidence will produce about a 70% probability of conviction (Devlin, 1976) and the courts are unlikely to be able to discriminate accurate from inaccurate identifications based on testimony (Wells, Lindsay, & Ferguson, 1979). The end result is that using simultaneous lineups likely will lead to

the wrongful conviction of from 570 to 1425 *more* innocent people annually than would occur using sequential lineups. It is one thing to say that no system is perfect and some innocent people will always be convicted. It is quite another to know that the means to reduce wrongful convictions is available and not use it.

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