Running title: BLANK LINEUPS

Refining the Blank Lineup Procedure: How Should We Instruct Eyewitnesses?

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Conflict of Interest

The authors declare no conflict of interest regarding this research.

Data Availability

Data are available from the authors upon reasonable request.

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Abstract

Presenting a blank lineup—containing only fillers—to witnesses prior to showing a real lineup might be useful for screening out those who pick from the blank lineup as unreliable witnesses. We show that the effectiveness of this procedure varies depending on instructions given to witnesses. Participants (N = 462) viewed a simulated crime and attempted to identify the perpetrator from a lineup approximately one week later. Rejecting a blank lineup was associated with greater identification accuracy and greater diagnosticity of suspect identifications, but only when witnesses were instructed prior to the blank lineup that they would view a series of lineups; the procedure was ineffective for screening when witnesses were advised they would view two lineups or received no instruction. These results highlight the importance of instructions used in the blank lineup procedure, and the need for better understanding of how to interpret choosing patterns in this paradigm.

Keywords: eyewitness identification; blank lineup; confirmation bias; lineup instructions

Eyewitness errors can have devastating consequences in the criminal justice system (e.g., Innocence Project, 2018). To mitigate the impact of eyewitness errors, researchers have attempted to develop procedures that enhance the accuracy of identification decisions, and help legal professionals evaluate the likely accuracy of identification decisions (e.g., Wells et al., 1998). One example is the blank lineup procedure (Wells, 1984). Prior to showing the witness a lineup that contains the police suspect (the *real lineup*), the witness is shown an additional lineup that contains only known-to-be-innocent fillers (the *blank lineup*). This procedure was designed as a method of screening witnesses in terms of the reliability of their identification decisions. Witnesses who choose from the blank lineup (*initial choosers*) can be screened out as poor witnesses who tend to pick from lineups too liberally and likely have poor memories of the culprit. Those who reject the blank lineup (*initial non-choosers*) can be regarded as relatively reliable. (Note that this does not imply that identification evidence from initial non-choosers should be assumed to be accurate; it must be evaluated, and other case evidence taken into account.)

The blank lineup procedure has received little attention from researchers and has not come into common use in police investigations, perhaps in part because at the time of development, there were practical constraints on the ability of investigators to locate suitable fillers for an extra lineup (Lindsay & Wells, 1985). However, there is legal precedent for courts to uphold a suspect's request for a blank lineup procedure to be used (Hibel, 2006; United States v Crouch, 1979; United States v Tyler, 1989), and the increasing availability of large databases of filler photographs and the use of computer-administered photo- or video-lineups (e.g., Cutler, Daugherty, Babu, Hodges, & Van Wallendael, 2009; Memon, Harvard,

Clifford, Gabbert, & Watt, 2011) mean that procedures involving multiple lineups are now much more feasible for use in police investigations (Palmer, Brewer, & Weber, 2012).

We refined the blank lineup procedure by modifying the instructions given to witnesses. Prior research—described below—has shown that various cognitive mechanisms influence witnesses' responses to a blank lineup and a subsequent real lineup. Our premise was that the specific instructions given to witnesses would influence the operation of these different mechanisms; the goal was to develop instructions that would enhance the usefulness of the blank lineup procedure as a screening tool for evaluating the reliability of witnesses.

Cognitive Processes in the Blank Lineup Procedure

Relative versus Absolute Judgment Strategies. In the original rationale for the blank lineup procedure, Wells (1984) distinguished between two types of decision strategies that eyewitnesses could adopt when viewing a lineup. With an *absolute judgment strategy*, identification decisions are based on the degree of match between the lineup members and the witness's memory of the perpetrator. With a *relative judgment strategy*, witnesses compare lineup members and choose the one that most closely resembles their memory of the perpetrator, regardless of the degree of match. Relative judgments increase the likelihood that innocent suspects will be falsely identified because, when the culprit is not in the lineup, witnesses tend to choose the best available lineup member (Wells, 1993). Any given identification decision is unlikely to be wholly relative or absolute, but the proportional contribution of the two processes can vary such that some decisions involve more relative judgment than do others. Although there is debate about the definition of these processes and the extent to which they can explain identification decisions (see below), the relative-absolute

distinction has been highly influential in eyewitness research (e.g., Clark, 2003; Fife, Perry, & Gronlund, 2014; Lindsay & Wells, 1985).

Although Wells (1984) conceptualized different judgment strategies as a distinction between relative and absolute judgments, more recently it has been suggested that judgment strategies might be better understood in terms of criterion placement (Moreland & Clark, 2020; Wixted & Mickes, 2014). As such, eyewitnesses may have a propensity to provide either a positive identification or lineup rejection. Those biased towards choosing a member from the lineup set a more lenient criterion. Alternatively, with cautious responding, stronger evidence is needed before the witness provides a positive identification. In the present context, the use of a blank lineup might be construed as a way of measuring bias to choose. Moreover, given that the decision criterion adopted by eyewitness can be influenced through experimental manipulations (Wixted & Mickes, 2014), the instructions accompanying the blank lineup procedure might constitute a manipulation of criterion.

Responses to a blank lineup might also be informative about discriminability (Moreland & Clark, 2020); that is, the ability of witnesses to distinguish the target from other lineup members. Wells (1984) argued that the tendency to choose from or reject a lineup was also related to the strength of a witness's memory for the culprit, such that rejecting the blank lineup is an indicator of those with stronger memories of the culprit, who are better able to discriminate guilty suspects from innocent suspects and fillers. Conversely, choosing from the blank lineup reflects poorer discriminability, and indicates that a witness is likely to perform worse on a subsequent real lineup task compared to witnesses who rejected a blank lineup. Consistent with these ideas, Wells found that, compared to initial non-choosers, initial

choosers—on average—made more false identifications from target-absent lineups and fewer correct identifications from target-present lineups (see also Palmer et al., 2012; but see Moreland & Clark, 2020, for evidence that relative judgments can produce greater discriminability than absolute).

In addition to providing a means of evaluating witnesses, it could be argued that the blank lineup procedure might improve identification performance by better preparing eyewitnesses for a subsequent judgment on a second lineup. For example, viewing the blank lineup might help participants learn the characteristics that are diagnostic of suspect guilt, or to adjust their decision criterion as a result of gaining an idea of the difficulty of the identification task. However, such expectations must be tempered by evidence suggesting that identification performance tends to suffer when witnesses perform multiple identification attempts for the same suspect, even if the suspect does not appear repeatedly (as is the case with a blank lineup; e.g., Palmer, Brewer, & Weber, 2010; Smalarz, Kornell, Vaughn, & Palmer, 2019).

Confirmation Bias and Commitment Effects. Identification responses in the blank lineup procedure are also influenced by confirmation bias and commitment effects (Palmer et al., 2012). Confirmation bias refers to the tendency to seek and process evidence in ways that support pre-existing beliefs or hypotheses (e.g., Klayman & Ha, 1987; Nickerson, 1998; Wason, 1960). Commitment effects can be considered a special case of confirmation bias, whereby commitment to an initial decision affects a subsequent related decision. For example, if a witness incorrectly recognizes an innocent person in an initial identification attempt (e.g., a lineup or a showup), the witness will be more likely to identify that same

innocent person in a subsequent identification attempt, even if the real culprit is present (e.g., Deffenbacher, Bornstein, & Penrod, 2006; Dysart, Lindsay, Hammond, & Dupuis, 2001; Gorenstein & Ellsworth, 1980; Memon, Hope, Bartlett, & Bull, 2002).

One way that commitment and confirmation bias effects manifest in the blank lineup procedure is that witnesses who choose from an initial lineup are predisposed to reject a subsequent lineup that does not contain the person they chose. Palmer et al. (2012) found that—compared to initial non-choosers and witnesses who did not view an initial lineup—initial choosers were more likely to reject a second lineup that did not contain the culprit, and that incorrect rejections of a second lineup were faster when made by initial choosers than by other witnesses. These results suggest that, when faced with a second lineup, witnesses who picked from a previous lineup tend to process evidence in a way that confirms their earlier decision. This process could be underpinned by commitment effects (e.g., inferring that the culprit cannot be in the second lineup, having already appeared in the first) and/or confirmation bias (e.g., selectively attending to features of lineup members that do not match their memory of the culprit, and interpreting ambiguous evidence as evidence of a mismatch).

These results are important because they imply that the blank lineup procedure has limited efficacy for screening unreliable witnesses: Instead of predicting the likely accuracy of witnesses' decisions regarding a subsequent real lineup, responses to a blank lineup might simply predict tendency to choose from the real lineup. Given the discrepancy in results between Palmer et al. (2012) and Wells (1984), it is worth considering characteristics of the blank lineup procedure that might limit confirmation and commitment effects and maximize the utility of the procedure for screening witnesses on likely memory quality.

Refining Instructions for the Blank Lineup Procedure

The way that confirmation bias and commitment effects operate in the blank lineup procedure likely varies depending on two aspects of the witness's beliefs about the procedure. The first is whether the witness thinks a person from the first lineup can appear in the second lineup. If so, a witness who picks someone from the first lineup might try to recognize and pick the same person from the second lineup (as in other studies showing commitment effects across multiple identification opportunities). It is important to note that this would make the memory task more complex, because the witness would be comparing members of the second lineup not only to their memory of the culprit, but also to their memory of the members of the first lineup (Palmer et al., 2010).

Second, commitment and confirmation bias effects also may vary depending on witnesses' beliefs about the number of lineups to be viewed. If a witness thinks that there will be two lineups, choosing from the first implies that the person they are looking for will not be in the second (unless the same person they chose appears in the second lineup). Thus, witnesses who choose from a blank lineup should be less likely to choose from a subsequent lineup. Conversely, rejecting an initial lineup may increase choosing from a second lineup. Rejecting an initial lineup does not imply per se that the culprit must be in a second lineup, because the culprit may be in neither lineup (e.g., if the police have arrested an innocent suspect). However, if the witness assumes that the police have arrested the correct person and decides that the culprit is not in the first lineup, they could infer that the culprit must appear in the second lineup.

These beliefs—and, hence, the operation of confirmation bias and commitment

effects—can be malleable using instructions given to witnesses about the blank lineup procedure. We consider several options. The simplest is to avoid telling witnesses in advance that they will be viewing more than one lineup, and only mention a second lineup after the witness has responded to the blank lineup. However, a clear practical drawback of this approach is that the witness might feel they have been tricked when told about the second lineup, undermining trust in the lineup administrator and potentially altering the way the witness responds to the second lineup.

Another approach is to tell the witness in advance that they will be viewing two lineups. This would reduce the potential for the witness feeling deceived. If combined with information that no-one from the first lineup will appear in the second, this would also reduce the chances that the witness might attempt to recognize members of the first lineup when viewing the second. However, these instructions might exacerbate any commitment and confirmation bias effects. For example, if a witness knows that they are trying to identify one culprit from two lineups containing different people, it could make more salient the notion that the culprit can only appear in one of the lineups and, hence, if the witness chooses from the first lineup they must reject the second.

A third option is to inform witnesses that they will be asked to view a series of lineups, without specifying how many. This approach is similar to telling witnesses before they view a showup or lineup that there may be additional opportunities to identify a suspect (Smith, Wells, Lindsay, & Myerson, 2018), and to "backloading" a sequential lineup to create the impression that there are more lineup members than actually exist (e.g., Lindsay & Wells, 1985). A key principle behind these instructions is that the decision criterion for making a

positive identification varies depending on how many chances for identification remain. The fewer the chances the witness thinks remain to identify the culprit, the more lenient the decision criterion becomes. Concealing from the witness the number of chances remaining undermines the setting of a lenient decision criterion, and typically increases the accuracy of identification decisions (Horry, Palmer, & Brewer, 2012; Smith et al., 2018). In the same vein, blank lineup instructions telling witnesses that they will view a series of lineups should limit confirmation bias and commitment effects by undermining any tendency to infer that the suspect must appear in either the first or second lineup.

Variation in instructions may have contributed to the differences in results between Wells (1984) and Palmer et al. (2012). In Wells, participants were not told in advance they would view more than one lineup and were told prior to the second lineup "now we would like for you to look at a second lineup. Again, the person who took the computer game might not be present." (p. 97). There was no indication that members of the first lineup would not appear in the second. Participants in Palmer et al. were told prior to the first lineup that they would view two lineups but were not told that the lineups would be for the same culprit (participants had seen a simulated crime with multiple culprits). Prior to the second lineup, participants were asked to again look for the same person and told that no-one who had appeared in the earlier lineup would appear in the second lineup. The knowledge that there would be two lineups may have contributed to commitment and confirmation bias effects in Palmer et al.'s data. However, this cannot be assumed, because other methodological differences may have been important. For example, Wells' use of a biased blank lineup (with only one lineup member being a plausible match for the culprit) and unbiased second lineup

(where all lineup members were plausible matches for the culprit) might have prompted some participants to assume that the first lineup had been a trial run (Palmer et al., 2012). Regardless, the data from these experiments are consistent with the notion that instructions can shape the pattern of responses in the blank lineup paradigm.

The Current Experiment

All participants viewed a video of a mock crime and attempted to identify the perpetrator from a lineup approximately one week later. Compared to a short retention interval, this delay more closely matches conditions in real police investigations. It cannot be assumed that results obtained with a short retention interval will translate to a longer interval. Delaying eyewitness identification decisions can reduce memory accuracy (e.g., Palmer, Brewer, Weber, & Nagesh, 2013; Read, Lindsay, & Nichols, 1998; Sauer, Brewer, Weber, & Zweck, 2010) and prompt a more lenient decision criterion for recognizing stimuli as previously seen (e.g., Singer & Wixted, 2006). This has implications not only for witnesses' ability to identify a culprit but also for how witnesses respond to a blank lineup: After a long retention interval, worse memory and a more lenient decision criterion may result in a greater proportion of picks from a blank lineup.

We compared three versions of blank lineup instructions. Participants in the *two lineups instruction* condition were told in advance that they would be asked to view two lineups. Participants in the *series instruction* condition were told that they would view a series of lineups but were not told how many lineups there would be. Participants in the *no instruction* condition were not told in advance how many lineups there would be. Participants in a single-lineup control condition saw the "real" lineup but not the blank lineup. We

hypothesized that confirmation bias effects would be most evident in the two lineups instruction condition, where responses to the first lineup would most easily give rise to inferences about the likely presence of the culprit in a second lineup. In these conditions, we expected that choosing from a blank lineup would be associated with increased likelihood of rejecting a second, "real" lineup. Such effects were expected to be least evident in the series instruction condition, where concealing the number of remaining identification attempts should undermine inferences about the likely presence of the culprit.

Method

Participants

Data were collected from 462 participants (282 female, 180 male) who were aged between 17-81 years old (M = 31.1, SD = 14.6). Participants were members of the public, recruited from the University of Tasmania (Australia) and the broader Tasmanian community, and participated voluntarily. The sex and age distribution did not differ across experimental conditions (all F and χ^2 values <1).

Materials

We used three sets of stimuli to promote generalizability of our results. Each set of stimuli comprised a video of a mock crime and an accompanying set of lineup photographs. The three videos depicted different scenes: a house burglary, a wallet stolen at a café, and an attempted car theft. The videos ran for 29s, 38s and 15s, respectively, and were non-violent in nature. All involved a single culprit with the café video featuring a female and the remainder featuring male culprits. A chi-square test to examine the distribution of stimuli across

instruction conditions indicated no association between stimulus set and condition, $\chi^2(6) = 3.38$, $p = .760$.

The lineup photographs were colored head-and-shoulders photographs. Each set of photographs comprised one photograph of the culprit and 12 photographs of fillers, selected to match the description of the culprit (Wells, Rydell, & Seelau, 1993). For each set of stimuli, six filler photographs were randomly selected for use in the blank lineup. The remaining filler photographs comprised the target-absent lineup. For the target-present lineup, the photograph of the culprit replaced a randomly-chosen filler. Note that no photographs used in the blank lineup were featured in subsequent lineup presentations. For each lineup, photographs were presented simultaneously in 2 × 3 array. For culprit-present lineups, the suspect appeared in the top-right or bottom-center location (counterbalanced). The position of lineup fillers was counterbalanced across locations.

Procedure

Under the supervision of a student volunteer research assistant, participants viewed one of the three mock-crime videos, randomly assigned. Participants viewed the video either alone or in groups, while the remainder of the experiment was completed individually. All components were administered via a computer. During this initial phase, participants had not yet been assigned to an experimental condition; thus, there was no possibility of experimenter bias. Approximately one-week later participants received an email asking them to complete the remainder of the study online. Participants did this privately, without a researcher present. At this stage, participants were randomly assigned to view a culprit-present or -absent "real" lineup, and to a lineup instruction condition (no instructions, two lineups, series, or the single

lineup control condition). Prior to each lineup, participants were asked to look for the person they saw committing a crime in the video (e.g., the girl who stole a wallet at a café) and given unbiased instructions that the person may or may not be in the lineup (for discussion of the effects of unbiased instructions, see Clark, 2012; Malpass & Devine, 1981; Steblay, 1997).

Participants in the no instruction condition were told prior to the second lineup that no-one from the first lineup would be present in the second. Participants in this condition were not provided with any other information about the number of lineups to be viewed. Participants in the two lineups instruction condition received all the instructions for the no instruction condition and were told (prior to the first lineup) that they would be shown two lineups and none of the faces from the first lineup would be present in the second lineup. Participants in the series instruction condition received similar instructions but were told prior to the first lineup that they would view a series of lineups (rather than two lineups).

Participants were asked to make their identification responses by clicking the numbered circle corresponding to the suspect's lineup position, or to click on the *not present* button if they thought the person was not in the lineup. Participants were then asked to rate their confidence in their decision on an 11-point scale from 0% (*not at all confident*) to 100% (*completely confident*). After completing the first identification decision and confidence judgment, participants were informed they would see another lineup and were asked to look for the same suspect. They were reminded the suspect may or may not be present and to click the suspect's corresponding circle or choose not present. Participants were then asked to rate their confidence in their decision.

Results

The proportions and frequencies for correct identifications, foil identifications, and incorrect and correct rejections for target-present and target-absent lineups are displayed in Table 1. At the outset, we note that the number of participants in some cells was small. Our total sample of 462 participants was split not only between the lineup instruction conditions, but also by whether participants chose from or rejected the blank lineup, with an uneven split between these responses in some conditions. As a result, cell numbers for some analyses were as low as 17, and our results should be interpreted with appropriate caution.

Choosing Rates

The choosing rate in the single-lineup control condition was higher than any of the blank lineup conditions. Across target-present and -absent lineups, participants in the single-lineup condition (.60) were more likely to pick from the second lineup than initial choosers (.39), $\chi^2(1, n = 279) = 11.52$, $p = .001$, $w = 0.203$, odds ratio (OR) = 1.52, and non-choosers (.32), $\chi^2(1, n = 314) = 23.14$, $p < .001$, $w = 0.271$, OR = 1.85. Thus, regardless of the lineup instructions or response to the blank lineup, viewing a blank lineup was associated with reduced choosing from the real lineup.

Contrary to expectations, choosing rates were not contingent on the response to the initial lineup in any of the blank lineup conditions, $\chi^2(1)$ values < 1.7, ns ≥ 108, p values > .195. Thus, there was no evidence that choosing from the first lineup triggered an inference that the culprit could not appear in the second lineup. Given this unexpected result, we conducted additional analyses to examine the extent to which participants were consistent in their responses across both lineups in the blank lineup conditions (i.e., choosing from or rejecting both the blank lineup and real lineup). We predicted that choosing from the blank

lineup might increase rejection of a second lineup due to commitment effects. However, there is evidence of individual differences in response bias, whereby some people are consistently more (or less) conservative in their recognition decisions (Kantner & Lindsay, 2012). Such individual differences might contribute to consistency in responding in the blank lineup paradigm; for example, people who are predisposed to set a conservative criterion might be more likely to reject both lineups.

To test the extent to which this may have shaped our results, we examined different combinations of choosing across the blank lineup conditions: choose-choose (i.e., choosing from both the blank and real lineup), choose-reject, reject-reject, and reject-choose (see Table 2). There was little evidence that consistency in responding played a major role in our data. Overall, 55% of responses were consistent (choose-choose or reject-reject), with almost identical rates of consistent responding in target-present (55.1%) and target-absent lineups (54.9%). A 3 (lineup instructions) × 2 (real lineup target presence) × 4 (choosing combination) hierarchical loglinear analysis indicated that—unsurprisingly—the pattern of choosing combinations differed between target-present and -absent lineups, with a higher proportion of reject-reject combinations for target-absent lineups than target-present, $\chi^2(3, n = 331) = 9.09$, $p = .028$. The proportion of choosing combinations did not vary significantly with lineup instruction conditions, $\chi^2(2, n = 331) < 1$, $p = .911$, or the interaction between instructions and target-presence, $\chi^2(6, n = 331) = 6.94$, $p = .326$.

Identification Decisions

To address the extent to which blank lineup responses predicted accuracy on a second, real lineup, we compared initial choosers and non-choosers in each of the blank lineup

conditions in terms of identification accuracy rates combined across target-present and -absent lineups (see Table 1). Blank lineup response predicted accuracy in the series instruction condition, with a higher proportion of correct decisions made by initial non-choosers than initial choosers, $\chi^2(1, n = 108) = 4.21$, $p = .040$, $w = 0.198$, OR = 1.56. The response frequencies (see Table 1) indicate that the accuracy advantage for initial non-choosers in the series instruction condition was due to a combination of a higher proportion of (a) correct identifications from target-present lineups and (b) correct rejections of target-absent lineups. In contrast, the blank lineup response did not predict real lineup accuracy in the no instruction condition, $\chi^2(1, n = 109) = 1.61$, $p = .205$, $w = 0.121$, or the two lineups instruction condition, $\chi^2(1, n = 114) = 0.51$, $p = .476$, $w = 0.067$. In fact, accuracy was numerically higher for initial choosers than non-choosers in these two conditions. Thus, the response to a blank lineup was indicative of accuracy on a "real" lineup only if witnesses were told in advance that there would be a series of lineups.

To more closely examine the effects of blank lineup instructions on identification performance, we calculated signal detection measures of discriminability (ability to discriminate between targets and fillers) and response bias (willingness to pick from a lineup). We used a compound signal detection model (SDT-CD; Duncan, 2006) that has previously been used with eyewitness identification decisions (e.g., Palmer & Brewer, 2012).

We calculated inferential confidence intervals (ICIs; Tryon, 2001) to compare discriminability for initial non-choosers and choosers within each instruction condition (see Table 3). The ICIs showed that discriminability did not differ significantly between initial non-choosers and initial choosers in any of the instruction conditions. However, the pattern of

d′ estimates corroborated the pattern of accuracy rates: In the no instruction and two lineups conditions, *d′* was numerically higher for initial choosers than initial non-choosers. This is the opposite pattern to what would be expected based on Wells' (1984) results and the rationale behind the blank lineup procedure. In contrast, in the series instruction condition, *d′* was numerically higher for initial non-choosers than initial choosers. With the caveat that the difference in *d′* estimates was not statistically significant, these results align with the accuracy data reported above: In both sets of analyses, the series instruction condition is the only one that produced any evidence of the blank lineup procedure operating as an effective tool for screening witnesses.

Diagnosticity of suspect identifications. We also calculated diagnosticity ratios for suspect identifications. Diagnosticity reflects the informational utility of these identifications for assessing the likely guilt of a suspect (Wells & Lindsay, 1980). That is, given that the suspect was identified by the witness, to what extent should investigators adjust their judgments about the likely guilt of the suspect? Diagnosticity ratios are calculated as the probability of correctly identifying the suspect in target-present lineups divided by the probability of making an incorrect identification from target-absent lineups. For target-absent lineups, this probability was estimated by dividing the rate of foil identifications by the number of lineup members (see Brewer & Wells, 2006). The greater the ratio (over 1), the more probable it is that the suspect identified is guilty, while a value below 1 means the identification is more likely inaccurate than accurate. Within each instruction condition, we made inferential comparisons between the diagnosticity ratios for initial choosers and non-choosers by first calculating Cohen's *h* for each diagnosticity ratio (representing the

difference between the proportion of correct suspect identifications from target-present lineups and the proportion of incorrect suspect identifications from target-absent lineups). We then transformed the *h* values to *z* scores and evaluated the difference between initial choosers and non-choosers by calculating the 2-tailed *p* value corresponding to the difference in *z* scores divided by √2 (Rosenthal, 1991).[1]

The pattern of diagnosticity ratios (see Table 4) suggests that, in the series lineup condition, suspect identifications made by initial non-choosers were more informative than those made by initial choosers. However, the comparison of *h* values for the series condition yielded a non-significant difference in *z* scores, $z_1 - z_2 = 1.70$, $p = .089$. In contrast, there was no evidence that suspect identifications made by initial non-choosers were more informative than those made by initial choosers in the no instruction condition, or two lineups instruction conditions. Not only did the inferential tests comparing *h* values for these conditions yield non-significant results (*p* values > .159), the diagnosticity ratios were numerically higher for initial choosers than initial non-choosers in both conditions.

Comparisons with the single lineup condition. Wells (1984) found that accuracy on a real lineup was greater for witnesses who rejected a blank lineup than those who did not view a blank lineup. In our data, this was true only for initial non-choosers in the series instruction condition: The accuracy rate was higher for this group (.56) than the single lineup condition (.37), $\chi^2(1, n = 194) = 5.71$, $p = .017$, $w = 0.17$, OR = 1.49. In contrast, accuracy did not differ between the single lineup condition and initial non-choosers in the no instruction or two lineups instruction conditions, χ^2s < 1, *p* values > .365.

[1] We thank an anonymous reviewer for suggesting this method of comparing diagnosticity ratios.

In terms of SDT estimates of discriminability and response bias, we did not make specific comparisons between the single lineup condition and each of the blank lineup groups (the inferential confidence intervals calculated to compare initial non-choosers and choosers did not allow comparisons with the single lineup condition). However, estimated *d′* for the single lineup group (0.93) was numerically between the values for initial non-choosers and choosers in each of the instruction conditions, suggesting it is unlikely that the single lineup group differed significantly from any of the blank lineup groups in discriminability. Estimated response bias for the single lineup group was numerically lower (i.e., more conservative choosing) than all blank lineup groups, but the absence of inferential tests prevents conclusions being drawn about these comparisons.

The diagnosticity ratios (Table 4) suggest that suspect identifications made by initial non-choosers who received the series instruction (but not those who received the two lineups instruction or no instruction) were more informative than those made by witnesses who did not view a blank lineup.

Additional exploratory analyses. Our data contain an unusual pattern in the choosing rates for the blank lineup. Across all instruction conditions and final lineup types (target-present or -absent), participants chose from the blank lineup 45-48% of the time, with one exception: Participants in the series condition who saw a target-absent final lineup chose from the blank lineup 35% of the time.[2] This result appears consistent with the idea that the series instructions might prompt witnesses to be less willing to choose from the first lineup

[2] We thank an anonymous reviewer for pointing out this unusual pattern and suggesting these analyses to explore it.

they view. However, there is no apparent explanation for the variation between the target-present and -absent conditions within the series instruction condition, because, to the point where they responded to the blank lineup, participants in these cells had received identical instructions.

We considered this unusual result from two angles. First, we checked whether the low choosing rate in the target-absent series instruction condition might have been due to an odd distribution of stimuli in this condition. This does not seem to be the case, given that the three stimulus sets were represented approximately equally often within the target-absent series condition (burglary = 19; café = 16; car = 14). Second, we re-examined the results for combinations of choosing across both lineups (Table 2). Although the distribution of choosing combinations did not vary significantly between instructions conditions (or with the interaction between instructions and target-presence), the proportion of reject-reject responses was numerically highest for participants who received series instructions and viewed a target-absent real lineup. Conclusions based on this pattern must be treated as speculative, but we note that this pattern is consistent with the notion that random allocation might have resulted in a greater proportion of conservative responders in this specific cell of the design which, in turn, might have contributed to the results for identification accuracy and discriminability (whereby initial non-choosers outperformed initial choosers in the series condition).

Identification confidence. Another potentially interesting issue is the extent to which blank lineup instructions—and choosing from a blank lineup—might affect the confidence-accuracy relationship.[3] However, this issue was not our focus, and our study lacked sufficient

[3] We thank another anonymous reviewer for this suggestion.

data points per cell for confidence-accuracy characteristic (CAC) or calibration analyses (e.g., Brewer & Wells, 2006; Mickes, 2015). Exploratory analyses showed that, collapsing across conditions, correct picks (M = 51.13, SD = 24.78) were made with higher confidence than were incorrect picks (M = 43.73, SD = 24.86), although this difference was not statistically significant, $t(193) = 1.85$, $p = .066$, Cohen's $d = 0.30$. We do not report this comparison for individual cells in our design; once we omitted participants who rejected the second lineup (because confidence is unrelated to accuracy for lineup rejections) and divided the remaining participants into those who made correct and incorrect picks, there were too few data points in some cells to draw meaningful conclusions about the effects of blank lineup instructions on the confidence-accuracy relationship.

Discussion

This research tested the idea proposed by Wells (1984) that blank lineups can be used to screen witnesses in terms of their likely reliability: that is, witnesses who reject a blank lineup will be—on average—more reliable than those who choose from a blank lineup. The results suggest that this holds true only when witnesses are informed beforehand that they will be asked to view a series of lineups. When these instructions are provided, (a) identification responses made by witnesses who rejected the blank lineup are more likely to be accurate than those made by witnesses who picked from the blank lineup, and (b) suspect identifications made by witnesses who rejected the blank lineup are—at least to some extent—more informative of the likely guilt than those made by witnesses who picked from the blank lineup. Additionally, these initial non-choosers are much better able to discriminate the target from fillers in the lineup. In contrast, if witnesses are told that there will be two

lineups, or not given information about the number of lineups to be viewed, then choosing from a blank lineup does less to predict identification accuracy or the diagnosticity of suspect identifications from a second, "real" lineup. If anything, choosers provide slightly more accurate responses and greater discriminability compared to non-choosers in these conditions. Importantly, these results were obtained with a retention interval of approximately one week, more closely resembling the conditions of a real investigation than previous research on the blank lineup procedure. From a practical viewpoint and considering previous findings, these results suggest that if the blank lineup procedure is to be an effective screening tool, then perhaps instructions to witnesses should include a statement indicating a series of lineups will be viewed. We return to this point later in the Discussion.

A second notable finding was that the pattern of choosing rates in the blank lineup procedure differed from what was expected. We predicted that choosing from a blank lineup would be associated with reduced choosing from a second lineup due to commitment effects and confirmation bias. If there is one culprit, and the witness chose someone from the first lineup, logically the witness should infer that the culprit cannot be in a second lineup that contains different people (Palmer et al., 2012). However, this did not seem to be the case: Choosing rates from the second lineup were lower in all the blank lineup conditions than the single-lineup condition, regardless of whether witnesses chose from the blank lineup or not. This occurred even when witnesses were told explicitly that there would be two lineups, which should maximize commitment effects. Thus, although previous research shows evidence of commitment and confirmation effects (Palmer et al., 2012), the present data indicate that such effects do not always influence responding in the blank lineup paradigm.

This pattern of choosing rates shows that the presentation of a blank lineup can reduce witnesses' propensity to choose from the "real" lineup, regardless of whether witnesses pick from or reject the blank lineup. Why might this occur? One possibility is that the presentation of multiple lineups makes it more salient to witnesses that the culprit may not be present in any given lineup; if there are to be multiple lineups, then there must be at least one lineup that does not contain the culprit. The use of multiple lineups might emphasize this notion, over-and-above the effects of unbiased instructions. Note, however, that the results of Palmer et al. (2012) do not align with this explanation: Palmer et al.'s data suggest that *choosing from* a blank lineup is associated with reduced choosing from a real lineup, whereas the current data suggest that simply viewing a blank lineup is associated with reduced choosing from a real lineup. This discrepancy highlights that factors affecting choosing in the blank lineup paradigm—and multiple lineup scenarios more broadly (e.g., Horry, Palmer, & Brewer, 2012; Palmer et al., 2010; Smalarz et al., 2019; Smalarz & Wells, 2014; Steblay & Dysart, 2016)—are not yet comprehensively understood. Replication of the current study with larger samples should help in disentangling these factors.

Given the patterns of choosing rates, it is important to acknowledge that our data do not provide a clear understanding of why the series instructions were important for the blank lineup procedure to be an effective screening tool. We expected that the series instruction (compared to the two lineups instruction) would minimize the impact of commitment and confirmation bias effects in responses to a second, real lineup. The data do not support this conclusion because the choosing rates provided no evidence that commitment and confirmation bias effects occurred, even given the two lineups instruction. This raises the

question of what mechanism underpinned the effectiveness of the series instruction for screening reliable from unreliable witnesses? Our data do not speak directly to this issue. However, recent work by Smith et al. (2018) suggests one potential explanation: The series instruction may have influenced the way witnesses responded to the blank lineup, potentially creating slightly different groups of blank lineup choosers and non-choosers. Additional opportunities instructions prompt witnesses to respond more conservatively but—crucially—this does not simply translate to reduced choosing; it can also increase accuracy. Smith et al. propose that, when faced with a culprit-absent lineup, witnesses tend to set an overly lenient criterion for making a positive identification. This is perhaps due to inflated expectations of the likelihood that the culprit will appear in the lineup (Wells, 1984; 1993). Instructions about additional identification opportunities curb this tendency by not only reducing choosing overall, but by reducing false identifications to a greater extent than correct identifications (beyond similar effects produced by unbiased instructions).

In the blank lineup paradigm, such processes might alter the composition of blank lineup choosers and non-choosers. In the absence of a series instruction, witnesses who choose from the blank lineup likely include those with relatively poor memories of the culprit and some with decent memories who pick due to non-memorial influences such as expectations about the likelihood the culprit will be present. If the series instruction undermines the tendency to pick from a blank lineup based on inflated expectations that the culprit will appear, then witnesses with a decent memory of the culprit will be more likely to reject the blank lineup. In turn, the difference in average memory quality between initial choosers and non-choosers will increase under the series instruction, enhancing the efficacy

of the blank lineup procedure as a screening tool. However, we emphasize that this account remains speculative. Moreover, the blank lineup responses in the present study offer only minimal support for this explanation: Choosing rates from the blank lineup was numerically lower in the series instruction condition (.42) than the two lineups (.47) and no instruction conditions (.46), but this difference was very small, $\chi^2 < 1$, $w = .04$.

Drawing conclusions about blank lineup instructions is further complicated by the lower blank lineup choosing rate for witnesses in the series condition who would eventually be presented with a target-absent lineup. Although this choosing rate in the blank lineup was not significantly lower than any other condition, it is nonetheless possible that it contributed to our results. The additional analyses conducted did not shed any light on the matter, however we cannot ignore this peculiarity when interpreting the influence of the given instructions and the utility of the blank lineup.

Could methodological differences account for the discrepant results across blank lineup studies? There are several differences between our study and that of Wells (1984). Our no instructions condition was closest to the design used by Wells, yet as aforementioned there was no meaningful difference between blank lineup choosers and non-choosers in the current experiment. If the blank lineup was a robust diagnostic tool for witness reliability, we would expect to see initial non-choosers consistently outperform choosers. Yet we did not find this, even in the condition closest to the original study. The blank lineup used by Wells was likely more biased than the blank lineups used in Palmer at al. (2012) and the current study, and the retention interval in the current experiment was substantially longer than in previous blank lineup studies. However, it is unclear how such differences can account for the variation in

results across these studies. Ultimately, it seems there are multiple factors with uncertain influences on the choice witnesses make in both the blank and subsequent real lineup.

Conclusion

Researchers continue to develop eyewitness identification techniques that promote accurate identification decisions and evaluation of those decisions. One such technique involves the use of a blank lineup to screen reliable witnesses from unreliable ones. Our research highlights that it cannot be assumed the blank lineup procedure will successfully do this. There are two aspects to this conclusion. First, our results suggest that for blank lineups to be used as a screening tool, instructions should include a statement that witnesses will view a series of lineups. We make this conclusion tentatively, due to the unusual patterns of choosing and discrepant results across blank lineup studies to date. Second, on a broader and more important level, it is becoming clear that we lack a good understanding of how the blank lineup procedure functions and how to interpret different patterns of responses in this paradigm. Developing such an understanding will be important for determining whether the blank lineup procedure can be used as a screening tool, and how to effectively implement this.

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Table 1
Percentages and Frequencies of Responses in Target-Present and Target-Absent Lineups

		Target-present lineups						
		Correct identification		Foil identification		Incorrect rejection		
Lineup instructions	Blank lineup status	%	*n*	%	*n*	%	*n*	Total
Single lineup		28.8	19	36.4	24	34.8	23	66
No instructions	Non-chooser	11.1	4	30.6	11	58.3	21	36
	Chooser	17.2	5	34.5	10	48.3	14	29
Two lineups	Non-chooser	13.8	4	13.8	4	72.4	21	29
	Chooser	32.0	8	12.0	3	56.0	14	25

Series lineups	Non-chooser	25.8	8	19.4	6	54.8	17	31
	Chooser	17.9	5	28.6	8	53.6	15	28
Overall		21.7	53	27.0	66	51.2	125	244

		Target-absent lineups				
		Foil identification		Correct rejection		
		%	*n*	%	*n*	Total
Single lineup		53.8	35	46.2	30	65
No instructions	Non-chooser	34.8	8	65.2	15	23
	Chooser	19.0	4	81.0	17	21
Two lineups	Non-chooser	28.1	9	71.9	23	32

	Chooser	32.1	9	67.9	19	28
Series lineups	Non-chooser	15.6	5	84.4	27	32
	Chooser	35.3	6	64.7	11	17
Overall		34.9	76	65.1	142	218

Table 2

Proportions of different combinations of choosing across both lineups in the blank lineup conditions.

Target-present Lineups					
	Reject-Reject	Reject-Pick	Pick-Reject	Pick-Pick	Total
No instructions	.32 (21)	.23 (15)	.22 (14)	.23 (15)	65
Two lineups	.39 (21)	.15 (8)	.26 (14)	.20 (11)	54
Series Lineups	.29 (17)	.24 (14)	.25 (15)	.22 (13)	59
Total	.33 (59)	.21 (37)	.24 (43)	.22 (39)	178
Target-absent Lineups					
	Reject-Reject	Reject-Pick	Pick-Reject	Pick-Pick	Total
No instructions	.32 (21)	.23 (15)	.22 (14)	.23 (15)	65
Two lineups	.39 (21)	.15 (8)	.26 (14)	.20 (11)	54
Series Lineups	.29 (17)	.24 (14)	.25 (15)	.22 (13)	59

Total	.33 (59)	.21 (37)	.24 (43)	.22 (39)	178

Table 3

Accuracy rates and estimates of d' and c and associated 95% inferential confidence intervals (95% ICIs). Each 95% ICI was calculated to compare initial non-choosers vs initial choosers within a specific lineup instruction condition. For each specific pairwise comparison of initial non-choosers vs initial choosers, non-overlapping intervals indicate a difference between conditions at the alpha = .05 level.

Instruction condition	Blank lineup status	Proportion correct [95% CI]	*d'*	95% ICI	*c*	95% ICI
Single lineup		.37 [.30, .46]	0.93	-	0.74	-
No instructions	Non-chooser	.32 [.22, .45]	0.45	[-0.12, 1.03]	1.25	[0.99, 1.50]
	Chooser	.44 [.31, .58]	0.98	[0.45, 1.50]	1.11	0.90, 1.33]
Two lineups	Non-chooser	.44 [.33, .57]	0.76	[0.17, 1.34]	1.34	[1.07, 1.61]
	Chooser	.51 [.38, .64]	1.37	[0.90, 1.84]	0.98	[0.75, 1.21]
Series lineups	Non-chooser	.56 [.43, .67]	1.47	[1.04, 1.90]	1.14	[0.94, 1.34]
	Chooser	.36 [.23, .50]	0.80	[0.25, 1.35]	1.09	[0.85, 1.33]

Table 4

Diagnosticity Ratios for Suspect Identifications and effect size estimates (Cohen's h) *for the difference between the proportion of correct suspect identifications from target-present lineups and the proportion of incorrect suspect identifications from target-absent lineups.*

Lineup instructions	Blank lineup status	Diagnosticity ratio	Cohen's *h*
Single lineup		3.21	0.52
No instruction	Non-chooser	1.92	0.19
	Chooser	5.43	0.50
Two lineups	Non-chooser	2.94	0.32
	Chooser	5.97	0.74
Series lineups	Non-chooser	9.91	0.74

Chooser	3.04	0.38