Evaluating Earwitness Identification Procedures: Adapting Pre-Parade Instructions and
Parade Procedure

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**PLEASE NOTE, THIS IS AN UPDATED VERSION OF A PREVIOUS PRE-PRINT
(‘EVALUATING EARWITNESS IDENTIFICATION PROCEDURES: THE EFFECT
OF PRE-PARADE INSTRUCTIONS’). FOLLOWING REVIEWERS’ COMMENTS
WE REDESIGNED THE EXPERIMENT AND RECOLLECTED ALL THE DATA.
THIS CHANGED OUR CONCLUSIONS.**

THE PREVIOUS PRE-PRINT IS AVAILABLE TO VIEW HERE:

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Abstract

Voice identification parades can be unreliable, as earwitness responses are error-prone. In this paper we tested performance across serial and sequential procedures, and varied pre-parade instructions, with the aim of reducing errors. The participants heard a target voice and later attempted to identify it from a parade. In Experiment 1 they were either warned that the target may or may not be present (standard warning) or encouraged to consider responding 'not present' because of the associated risk of a wrongful conviction (strong warning). Signal detection analyses revealed greater sensitivity in parades with strong warnings. In contrast to previous findings, performance was similar on serial and sequential parades. Experiment 2 ruled out the possibility that this inconsistency was caused by the number of times participants heard the parades before responding. Taken together, our results suggest that adapting pre-parade instructions provides a simple way of reducing the risk of false identifications.

Keywords: voice identification, lineup instructions, earwitness, serial, sequential

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Earwitness evidence is relevant when a witness is able to hear a perpetrator's voice while not being able to see their face. Such cases often relate to serious crimes, like rape or murder, where disguises may be worn (*R v Khan and Bains*, 2002, discussed in Nolan, 2003; *R v Flynn and St John*, 2008). Voice identification is error-prone; earwitnesses are likely to identify an incorrect voice (Kerstholt, Jansen, Van Amelsvoort, & Broeders, 2004, 2006; Smith et al., 2020). As highlighted within the context of eyewitness identification, if an innocent suspect has been apprehended, a false identification increases the risk of an erroneous conviction (Innocence Project, 2020). Finding ways to reduce such errors is vital, but methods of adapting voice identification procedures to optimize earwitness performance are under-researched. In this paper we investigate the effect of pre-parade instructions and procedure type, with the aim of informing procedural changes that support earwitness performance.

Parade Types

During a voice parade¹ the suspect is presented amongst 'foil', or distractor voices. If the 'target' (i.e. perpetrator) is present the witness might identify the target voice (a 'hit') or a foil voice (a 'miss'). If the target is absent the witness may incorrectly identify someone (a 'false alarm'), despite having the option to respond 'not present'. There are various ways of presenting parade voices, but the Home Office (2003) recommends a serial procedure, requiring witnesses to listen to all 9 parade voices before making a decision. Working memory demands, which require storing the target voice along with all other voices in the parade, may contribute to task difficulty. An alternative method, the sequential procedure,

¹ We refer to parades rather than a lineups because the term 'parade' is used by practitioners in England and Wales.

potentially reduces these demands as it involves responding ‘yes’ or ‘no’ after hearing each voice. Smith et al. (2020) found higher hits and lower false alarms with the sequential compared to the serial parade. Voice identification is subject to interference from intervening identity information (Stevenage, Howland, & Tippelt, 2011). Posing a question after each voice may mitigate the effect of interference by demarcating the voices (Smith et al., 2020).

Patterns of performance across parade procedures have been thoroughly investigated in the context of face identification (e.g. Brewer, Weber, Wootton, & Lindsay, 2012; Carlson, Gronlund, & Clark, 2008; Lindsay, Mansour, Beaudry, Leach, & Bertrand, 2009). Although false alarms might be lower when faces are presented sequentially rather than simultaneously (Clark, Howell, & Davey, 2008; Steblay, Dysart, & Wells, 2011), the sequential procedure appears to lead to a stricter decision standard and overall lower rates of choosing (Ebbesen & Flowe, 2002; Mickes, Flowe, & Wixted, 2012). This highlights the importance of considering both hit and false alarm rates when designing parades. However, the findings may not generalise across modalities because (1) cognitive processes involved in recognising faces and voices are not identical (Belin, Bestelmeyer, Latinus, & Watson, 2011; Belin, Fecteau, & Bedard, 2004; Young, Frühholz, & Schweinberger, 2020), (2) faces can be presented simultaneously while voices cannot, and (3) listening to a voice likely involves focusing on meaning rather than identity-specific sound information (Fenn et al., 2011; Vitevitch, 2003).

The Effect of Instructions

Witnesses receive instructions prior to completing an identification parade. We know that the content of these instructions can influence an eyewitness’s decision, and stronger warnings can reduce false alarms (Brewer & Wells, 2006; Clark, 2005; Malpass & Devine, 1981; Meissner, Tredoux, Parker, & MacLin, 2005; Steblay, 1997). No previous earwitness studies have systematically manipulated the content of instructions. This is an important omission considering the high false alarms associated with voice parades.

Unbiased instructions (i.e. warning witnesses that the perpetrator may not be present), are mandatory in England and Wales (Police and Criminal Evidence Act, Code D, 1986) and are included in guidelines in other common law jurisdictions (Fitzgerald, Rubínová, & Juncu, 2020). *Biased* instructions do not include this warning, creating an insinuation that the perpetrator is indeed present. Unbiased instructions are associated with lower false alarms on target-absent face parades (Brewer & Wells, 2006; Clark, 2005), with the potential to halve the number of mistaken identifications (Malpass & Devine, 1981). While there is some debate about whether the reduction in false alarms is associated with lower rates of choosing and so a reduction in correct identifications (see Clark, 2005; Steblay, 1997), unbiased instructions seem to encourage witnesses to raise their criterion for selecting the lineup member who looks most familiar (Brewer & Wells, 2006).

Accepting that an unbiased warning is needed, it is necessary to consider what form it should take. Lampinen et al. (2020) and Wilcock, Bull and Vrij (2005) found no difference between a standard and enhanced version, in which eyewitnesses are additionally reminded that an incorrect identification might lead to false imprisonment. However, the instructions may have been too complicated (Wilcock et al., 2005), or may not have produced a sufficient impression (Lampinen et al., 2020; Meissner et al., 2005). Meissner et al. (2005) compared the standard instruction to stronger, criterion-based instructions, in which participants were told to make a positive identification only if they were 100% sure. The stronger instruction improved discrimination, reducing false alarms but not hits, and replicating previous findings (see Steblay, 1997).

The Current Study

The earwitness literature has not addressed the effect of pre-lineup instructions. Here we compare standard unbiased instructions (standard warning) to a strong warning, encouraging participants to consider responding ‘not present’. We test the effect of warnings

in serial parades (Home Office, 2003), and sequential parades, as these might be more appropriate for assessing voice identification performance (Smith et al., 2020). While the cognitive processes involved in memory for faces and voices may not be identical, there is likely to be overlap (Belin et al., 2011; Bruce & Young, 1986; Jenkins et al., 2021). We include participants' Extended Cambridge Face Memory Test (CFMT+) scores as a scaled covariate to account for potential variability associated with face memory ability. As previous research suggests that participant age (Hoffman, Dobie, Losonczy, Themann, & Flamme, 2017) and self-rated confidence (Sarwar, Allwood, & Zetterholm, 2014) might affect results, these were also added as scaled covariates. We expected that hits would be low, and the false alarms would be high, but that the strong warning would make participants less likely to false alarm in both types of parade. We expected that accuracy on target-present and target-absent parades would be higher for the sequential compared to the serial procedure.

Experiment 1: Pre-Parade Instructions and Parade Procedure

Method

Design. We used a 2 x 2 x 2 between-subjects factorial design. The factors were parade type (serial, sequential), parade instructions (strong warning, standard warning), and target presence (present, absent). Identification accuracy (1 = correct, 0 = incorrect) and self-rated confidence (0-10) were the dependent variables.

Participants. We recruited 561 participants. All participants had previously completed the Cambridge Face Memory Test + (CFMT+; [BLINDED] database) and agreed to be contacted about future experiments. We removed data from 35 participants (7 with missing CFMT+ scores and 28 who reported uncorrected hearing problems). The final sample included 526 participants (336 female, 189 male, 1 prefer not to say) with an age range of 18–75 years ($M = 45.96$, $SD = 12.47$). Mean CFMT+ scores ($M = 86.37$, $SD = 9.62$) were somewhat higher than typical (70.7; Bobak, Pampoulov, & Bate, 2016). The experiment

was approved by the [BLINDED] University's Business, Law and Social Science College Research Ethics Committee.

Apparatus and materials. The voice stimuli were taken from the Dynamic Variability in Speech Database (DyViS) (Nolan, McDougall, De Jong, & Hudson, 2009). This database features 100 male speakers between the ages of 18 and 25, all with a Standard Southern British English (SSBE) accent. The speakers are recorded performing spoken tasks, such as a simulated police interview. They were recorded in a sound-treated booth using a sampling rate of 44.1 kHz (Nolan et al., 2009). The recordings used in each voice parade were the same as those used by Smith et al. (2020): Thirty speakers were randomly selected from the database and assigned into three 10-speaker groups based on fundamental frequency (F0) (low, medium, and high). From each group, a target-absent and target-present parade were constructed; this meant that overall there were three target-present parades and three target-absent parades. In the target-present parade the target either appeared in an early position (position 3), or a late position (position 7). The three target voice samples were taken from the recording of a telephone conversation during which the speakers discussed a crime. These recordings were used for the encoding stage. The recording was edited so that it was 60s long and featured only the targets' side of the conversation. As all speakers were responding to the same scripted questions, the content was similar for all three targets. The voice parade speech samples were selected from the simulated mock police interview recordings. These recordings were used for the test stage. All interviewer speech content was removed and only excerpts featuring the interviewees were combined to produce 15s samples. The voice samples for each speaker were from different, randomly selected sections of the police interview, meaning that the content of speech varied across speakers. The content of the telephone-recording and interview samples did not overlap. All of the parades were fair and unbiased (Malpass & Lindsay, 1999), as reported in Smith et al. (2020): None

of the parades were found to be biased towards the target, and there were several viable alternatives to each target among the foils; Tredoux's E varied from 3.80 to 7.22 across parades.

Participants completed a wordsearch containing words for different types of fruit during the retention interval. The axes of the wordsearch were numerically labelled and participants were required to enter the coordinates of the X and Y axes for the first letter of each word. While completing the wordsearch, a recording of ambient noise featuring unintelligible speech sounds played in the background.

Procedure. Participants completed the experiment online hosted on Gorilla.sc (Anwyl-Irvine, Massonnié, Flitton, Kirkham, & Evershed, 2019). They gave informed consent, set their volume, calibrated their headphone volume, completed a headphone screening test (Woods, Siegel, Traer & McDermott, 2017), and were randomised to a parade (1, 2 or 3) and a condition. Each participant completed a single trial.

Prior to the presentation of the target voice, participants were instructed that they would hear a voice recording and be asked questions relating to what they had heard; participants were not informed that they would be undertaking a voice parade. Participants were not able to go back to previously viewed pages at any point. On pages where timing was critical, it was not possible to progress until the task had been completed. Participants were asked to listen once to the 60s target voice sample. They then completed the filler task (wordsearch) for 5 minutes. After 5 minutes had passed, the instructions for the parade appeared. Participants were reminded that at the beginning of the experiment they had heard a perpetrator discussing a crime. They were instructed that they were going to listen to a voice parade in order to try and identify the perpetrator they had heard speaking in the initial recording. In the standard-warning instruction, it was highlighted that “the voice you heard in the original recording may or may not be present”. In the strong-warning condition, they

additionally received the following instruction: “Please consider your response(s) carefully.

In a real case, selecting someone from the lineup when the perpetrator is not present could lead to a wrongful conviction”.

At test, participants either completed a serial or a sequential parade:

Serial parade. Each recording was presented on a separate page with the voice number visible while the recording was playing. Participants listened to each recording (15s) once. They were informed that they were going to hear 9 voices played one after the other. After listening to all 9 voices they read the following instruction in the standard-warning condition: “The perpetrator may or may not be present. If you think the perpetrator was present, please select the correct voice below. If you think the perpetrator was not present, please select ‘The perpetrator was not present’. In the strong-warning condition, in addition to these instruction participants were reminded to, ‘Please consider your response carefully. In a real case, selecting someone from the lineup when the perpetrator is not present could lead to a wrongful conviction”. Below the instruction, participants were given one of ten options: voice 1–9 or, “The perpetrator was not present”. When they had made a selection, they were asked to assess the confidence in their decision (0 = Not at all confident, 10 = Extremely confident).

Sequential parade. Participants were informed that they would hear a series of voice recordings with the objective of trying to identify the perpetrator. They were informed that following each of the voices they would be asked to decide if the voice belonged to the perpetrator. The participants were not informed about the number of voices that would be presented, only that after they had responded ‘yes’, no further voices would be played. Participants listened to a voice recording (15 s), and in the standard-warning condition were then asked: “Do you think Voice [number] belongs to the perpetrator?” In the strong-warning condition, in addition to this, they were reminded: “Please consider your response carefully.

In a real case, selecting someone from the lineup when the perpetrator is not present could lead to a wrongful conviction”. Participants selected “yes” or “no”, and provided a confidence rating (0 = Not at all confident, 10 = Extremely confident) when they responded “yes” to a voice, or after they had responded “no” to all 9 voices.

After completing the parade, participants in both the serial and sequential condition were invited to answer brief questions about their experience. Two of these questions served as a manipulation check, with responses provided on an 11-point slider rating scale and having a default starting position of 5. Participants were asked, “to what extent did you consider responding that the perpetrator was not present/responding ‘no’ to each voice? (0 = Did not consider it at all, 10 = Strongly considered it)”, and “Before completing the parade, you were warned that the perpetrator may or may not be present. To what extent did this warning influence your decision(s)? (0 = The warning had no influence on my decision, 10 = The warning had a strong influence on my decision)”. Our sample showed higher mean scores for strong warnings for both manipulation checks, supporting that our participants adapted their behaviour according to the presented instructions (see Appendix A for an overview).

Results

Data were analysed using Bayesian mixed models (Gelman et al., 2014; McElreath, 2016). We obtained the evidence for the alternative hypothesis from Bayes Factors (BF) using the Savage-Dickey method (Dickey et al., 1970; Wagenmakers et al., 2010).² We summarized the posterior as the most probable population value and the interval containing 95% of the posterior probability mass (i.e. Highest Posterior Density Interval (HPDI)).³ The

² A BF larger than 3 indicates substantial support for the alternative hypothesis and a BF larger than 10 indicates strong support (Baguley, 2012; Jeffreys, 1961; Lee & Wagenmakers, 2014). For example a BF of 10 indicates that the alternative hypothesis is 10× more likely than the null hypothesis.

³ The R (R Core Team, 2020) package brms (Bürkner, 2017, 2018) was used to model the data. Models were run with 30,000 iterations on 3 chains with a warm-up of 15,000 iterations and no thinning.

benefits of using a Bayesian approach for hypothesis testing (Kruschke et al., 2012; Kruschke, 2014) and parameter estimation (Lambert, 2018; Lee & Wagenmakers, 2014) are well documented in the literature.

For all models we scaled and included CFMT+ scores (i.e. face memory ability), age and confidence ratings⁴ as covariates. Models were fitted with random intercepts for parade (1, 2 or 3 – i.e. the 3 variations of the parade). Here we report the results in brief.

Supplementary information for Experiment 1 analyses are presented in Appendices B-D.

Data and analysis scripts can be found on

osf.io/x2dpc/?view_only=b74edc32c9494dcda5802a6efb4f0981⁵.

Accuracy analysis. We analysed response accuracy (0 = incorrect response, 1 = correct response) using Bayesian logistic mixed models. Predictors were included for main effects and interactions of parade instructions (levels: standard warning, strong warning), parade type (levels: sequential, serial), and target presence (levels: present, absent). There was substantial evidence for higher accuracy on parades with a strong warning compared to a standard warning ($\beta = -1.75$, HPDI: $[-3.15 - -0.22]$, BF = 9.45). Accuracy was higher on target-present parades compared to target-absent parades ($\beta = -3.82$, HPDI: $[-5.36 - -2.44]$, BF >100). The cell means are shown in Figure 1. Indeed, pairwise comparisons revealed substantial support indicating a higher accuracy for strong FA warnings in parades without target (sequential parades: $\beta = -0.68$, HPDI: $[-1.59 - 0.15]$, BF = 3.39; serial parades: $\beta = -$

Model convergence was confirmed by the Rubin-Gelman statistic ($\hat{R} = 1$) (Gelman & Rubin, 1992) and inspection of the Markov-chain Monte-Carlo chains.

⁴ Confidence ratings were not scaled and used as covariates when confidence ratings were analysed as outcome variable.

⁵ This link will be shorter when unmasked.

0.89, HPDI: [-1.79 – -0.04], BF = 7.94) but negligible evidence⁶ for parades that included the target voice (sequential parades: $\beta = -0.38$, HPDI: [-1.07 – 0.36], BF = 1.29; serial parades: $\beta = 0.06$, HPDI: [-0.68 – 0.76], BF = 0.73). Evidence for all interactions was negligible, as well as all other predictors; see Appendix B for full results.

Figure 1

Posterior response accuracy with 95% HPDIs, Experiment 1

Signal-detection analysis. We evaluated voice identification in the context of signal-detection theory so that we could evaluate: (1) the decision or response criterion (2) the signal strength or discriminability (d' ; e.g. Wixted et al., 2016). The criterion is an indicator of the overall willingness to make a positive identification, estimated from target-absent parades. The parameter d' can be understood as a measure of sensitivity to the signal; i.e. being aware of the target's voice in a parade. A higher d' value indicates how well a listener is able to distinguish between a target and a foil. Signal-detection theory allows us to extract d' by accounting for differences in the response criterion.

⁶ While Bayes Factors are useful for testing null hypotheses (Dienes et al., 2018), this is only appropriate when specifically testing for the null, not when drawing conclusions from null effects.

As discussed in the introduction, we predict that a strong warning will increase sensitivity to the signal d' . However, it is also plausible that strong warnings will modulate the response criterion by reducing the choosing rate. In other words, decisions might be more conservative.

Bayesian generalised mixed models were used to infer the model parameters (DeCarlo, 1998, 2010; Rouder et al., 2007; Rouder & Lu, 2005). Main effects of parade type and parade instructions as well as their interaction were calculated from the posterior.

From target-present parades we calculated the frequency of hits (correct target identifications) and misses (incorrect foil identifications/incorrect parade rejections). From target-absent parades we calculated false alarms (incorrect foil identifications) and correct rejections (Wixted et al., 2016). Table 1 provides a descriptive overview and Figure 2 provides a visual representation of the model results. For a full summary of the results (including the cell means for criterion and d'), please see Appendix C.

Table 1

Decision frequency with percentages in parentheses, Experiment 1

Parade Type	Pre-parade Instructions	Target-present			Target-absent	
		Target	Foil	Reject	Foil	Reject
Sequential	Standard Warning	25 (38%)	38 (58%)	3 (5%)	55 (85%)	10 (15%)
	Strong Warning	30 (46%)	32 (49%)	3 (5%)	48 (73%)	18 (27%)
Serial	Standard Warning	32 (47%)	30 (44%)	6 (9%)	53 (85%)	9 (15%)
	Strong Warning	28 (45%)	21 (34%)	13 (21%)	52 (72%)	20 (28%)
Total		115 (44%)	121 (46%)	25 (10%)	208 (78%)	57 (22%)

Figure 2*Parameter-value estimates from the signal-detection model, Experiment 1*

The response criterion was lower than zero in all conditions, indicating that participants had a bias to respond that the target was present. More specifically, there was weak evidence for this bias in strong warning sequential (BF = 3.01) and serial parades (BF = 4.39), strong to substantial evidence for this in standard warning sequential parades (BF = 8.98) and standard warning serial parades (BF = 16.58). This indicates that, overall, there is a strong predisposition for listeners to make a positive identification when the target is not present in the parade. Evidence for all main effects and interactions was negligible for the response criterion.

The signal sensitivity d' was different from zero for parades with a strong warning (serial: BF = 28.2; sequential: BF = 30.28), but not for parades with a standard warning (serial: BF = 2.84; sequential: BF = 1.14). This means that participants were only able to distinguish the target voice from the foils when they received strong warnings. However, we found only weak evidence supporting a lower signal sensitivity for standard warnings compared to strong warnings ($d' = -0.81$, HPDI: [-1.8 – 0.03], BF = 2.51). Evidence for a

main effect of parade type (serial or sequential) and the parade type by parade instruction (standard warning or strong warning) interaction was negligible suggesting that any advantage for strong warnings was similar in both parade types. Pairwise comparisons between strong and standard FA warning revealed negligible evidence for differences in response criterion (sequential parades: $c = -0.31$, HPDI: $[-0.78 - 0.18]$, $BF = 0.96$; serial parades: $c = -0.36$, HPDI: $[-0.83 - 0.14]$, $BF = 1.4$) and weak evidence for the signal sensitivity in sequential parades ($d' = -0.55$, HPDI: $[-1.13 - 0.1]$, $BF = 2.2$) but negligible evidence for serial parades ($d' = -0.36$, HPDI: $[-1.01 - 0.28]$, $BF = 1.19$).

There was substantial evidence reflecting higher sensitivity for participants with higher self-rated confidence ($d' = 0.17$, HPDI: $[0 - 0.38]$, $BF = 5.49$). Evidence for CFMT+ scores and age were negligible.

Confidence ratings. Confidence ratings (0 – 10) were analysed in cumulative mixed models for ordinal data (Bürkner & Vuorre, 2019; Liddell & Kruschke, 2018). The posterior cell means for each condition are shown in Figure 3.

Figure 3

Posterior confidence ratings with 95% HPDIs, Experiment 1

We investigated the relationship between confidence ratings and accuracy for each condition of parade type and parade instructions. There was strong evidence for a positive relationship between accuracy and confidence rating for serial parades with strong warning ($\beta = 0.83$, HPDI: [0.19 – 1.43], $BF = 9.55$); evidence was negligible for all other conditions (serial parade / standard FA warning: $BF = 1.64$; sequential parade / standard FA warning: $BF = 0.88$; sequential parade / strong FA warning: $BF = 1.01$). In other words, participants were more confident about correct responses (than about incorrect responses) when receiving a strong warning but only in serial parades.

Discussion

As predicted, we found a higher identification accuracy for strong warnings than for standard warnings (Meissner et al., 2005; Steblay, 1997). Contrary to previous findings (e.g. Smith et al., 2020), we found no differences between serial and sequential parades.

Signal detection analyses revealed that strong warnings improved participants' ability to distinguish between target and foil voices. These results indicate that the wording of this warning is sufficient to produce an effect on participants (Lampinen et al., 2020). The strong warning reduces false alarms in both serial and sequential procedures, but not at the expense of hits (Meissner et al., 2005; Steblay, 1997).

We did not observe any differences between serial and sequential parades. This is inconsistent with the results of Smith et al. (2020), who reported more accurate performance on sequential voice parades. In Smith et al. (2020), participants listened to the serial parade twice before making a decision, but only once in the present experiment. In Experiment 2 we address whether this might explain why we did not observe lower accuracy for serial compared to sequential parades.

The results are consistent with previous literature finding that overall accuracy was low (<50%), and false alarms were high (e.g. Kerstholt et al., 2004, 2006; Smith et al., 2020).

There was no evidence that face memory ability or participant age predicted overall accuracy. However, higher sensitivity was observed in participants who reported being more confident in their decision. That said, confidence does not appear to be a reliable predictor of performance; although there was strong evidence for a positive accuracy-confidence relationship for serial parades with a strong warning, this was not observed in other conditions. Overall confidence ratings were surprisingly high considering the low accuracy.

Experiment 2: The Number of Laps

Home Office (2003) guidelines recommend that participants listen to each serial parade voice at least once before making a decision. In Smith et al. (2020) participants listened to all voices in the serial parade twice before making a decision, while in Experiment 1, participants heard serial parade voices once. The second ‘lap’ of voices was removed in Experiment 1 to facilitate comparisons across procedures, given that voices in the sequential parade are only heard once.

The lap effect has been addressed in the eyewitness literature, but not in the earwitness literature. Thus far there is no evidence to suggest any performance benefit for 2 laps. Indeed, there is an associated risk that participants will adopt a more lenient response criterion on the second lap, being more likely to make a positive identification (Horry, Brewer, Weber, Palmer, & 2015; Lindsay, Lea, & Fulford, 1991; Maclin & Phelan, 2007; Steblay, Dietrich, Ryan, Raczynski, & James, 2011). The lap effect has only been tested in the context of sequential parades, where participants respond after seeing each face, and so have the opportunity to identify different targets on each lap. This is different from implementing a second lap in a serial parade and only allowing participants to make a decision after considering all parade members twice. This is our focus in Experiment 2.

Based on higher accuracy in sequential parades than serial parades in Smith et al. (2020) (2 laps), but observing no difference in Experiment 1 (1 lap), we expected a single

serial lap to be associated with higher accuracy than 2 laps. We compare performance using a standard warning because this is consistent with the instructions provided in Smith et al. (2020). There is no reason to believe that providing a strong warning when completing 2 laps would optimize performance beyond the benefit observed for the 1 lap condition (Experiment 1).

Method

The method was identical to Experiment 1 except for the following amendments:

Design. We used a 2 x 2 between-subjects factorial design. The factors were number of laps (1 laps, 2 laps), and target presence (present, absent). Identification accuracy (1 = correct, 0 = incorrect) and self-rated confidence (0-10) were the dependent variables.

Participants. We recruited 112 participants. We removed data from: 1 participant with a missing CFMT+ score, and 4 participants who reported having uncorrected hearing problems. The final sample included 107 participants (69 female, 38 male) with an age range of 23-73 years ($M = 48.72$, $SD = 12.14$).

Procedure. All participants completed a serial parade with a standard warning and were randomly allocated to the target-present or target-absent condition. Participants were informed that they would listen to the parade twice before making a decision. The 9 voice samples were presented in the same order both times.

Results

The data were combined with the subset of trials from the serial parade, standard warning conditions in Experiment 1 ($n = 130$). While the participants from Experiment 1 listened to the serial parade once (1 lap) before making a decision, the participants recruited for Experiment 2 listened to the serial parade twice (2 laps) before making a decision. There were no other differences across experiments.

We used the same tools for data analysis as in Experiment 1. We scaled and included CFMT+ scores, confidence ratings⁷, and age as covariates. Random intercepts were included for parade (1, 2 or 3). Here we report the results in brief. Supplementary information for Experiment 2 analyses are presented in Appendices E – G.

Accuracy analysis. We analysed response accuracy (0 = incorrect response, 1 = correct response) using Bayesian logistic mixed models. The predictors were the main effects of number of laps (levels: 1 lap, 2 laps), target presence (levels: present, absent), and their interaction. The evidence for a difference between 1-lap and 2-lap parades ($BF = 0.99$) as well as the by-target presence interaction was negligible ($BF = 0.87$). In other words, increasing the number of voice presentations from 1 to 2 does not seem to affect the response accuracy. We found higher accuracy for parades that included the target voice compared to parades without target voice ($\beta = -3.93$, HPDI: $[-5.29 - -2.64]$, $BF > 100$); see Figure 4. Moderate evidence was found for a positive association between accuracy and confidence ratings ($\beta = 0.42$, HPDI: $[0.08 - 0.79]$, $BF = 3.57$) indicating that participants who were more confident in their responses typically responded more accurately. Evidence for a negative relationship between age and accuracy was weak ($\beta = -0.39$, HPDI: $[-0.73 - -0.06]$, $BF = 2.34$). Evidence for all remaining predictors was negligible; see Appendix E.

⁷ Confidence ratings were not scaled when used as outcome variable.

Figure 4*Posterior response accuracy with 95% HPDIs, Experiment 2*

Signal-detection theory model. As in Experiment 1 we evaluated voice identification in the context of a signal-detection theory model. Table 2 provides a descriptive overview of target and foil identifications, as well as parade rejections. For the model fit we included number of laps (levels: 1 lap, 2 laps) as a fixed effect and estimated the response criterion and the signal sensitivity for each condition. The conditional parameter-value estimates are summarized by condition in Figure 5.

Table 2*Decision frequency with percentages in parentheses, Experiment 2*

Number of passes	Target-present			Target-absent	
	Target	Foil	Reject	Foil	Reject
1 pass	32 (47%)	30 (44%)	6 (9%)	53 (85%)	9 (15%)
2 passes	24 (45%)	23 (43%)	6 (11%)	50 (93%)	4 (7%)
Total	56 (46%)	53 (44%)	12 (10%)	103 (89%)	13 (11%)

There was substantial evidence for the 1-lap condition ($BF = 7.58$) and strong evidence for the 2-lap condition ($BF = 21.93$) having criterion values below zero; again, this shows that there was a strong predisposition for listeners make a positive identification when the target is not present in the parade. Importantly there was substantial evidence showing that the signal sensitivity was larger than zero for 1-lap parades ($BF = 4.54$), but negligible evidence for 2-lap parades ($BF = 1.00$). Therefore, in 1-lap, but not 2-lap conditions participants could distinguish the target voice from foils. The evidence for a difference between parades with 1 lap and 2 laps was negligible for both signal sensitivity ($BF = 0.71$) and response criterion ($BF = 0.68$). Also, weak evidence was found for a larger signal sensitivity for participants with a higher self-reported confidence ($d' = 0.34$, HPDI: [0.01 – 0.69], $BF = 2.98$). Evidence for all other predictors was negligible; see Appendix F for a full summary.⁸

⁸ There was no support in favour of an unequal variance model; see Appendix G.

Figure 5*Parameter-value estimates from the signal-detection model, Experiment 2*

Confidence ratings. Confidence ratings (0 – 10) were analysed using cumulative mixed models for ordinal data (Bürkner & Vuorre, 2019; Liddell & Kruschke, 2018). Posterior confidence ratings were equally high in 8.14 (HPDI: 7.08 – 8.66) for 1-lap parades and 8.18 (HPDI: 7.10 – 8.71) for 2-pass parades. There was substantial evidence for a positive relationship between accuracy and confidence ratings in the 2-pass condition ($\beta = 0.80$, HPDI: [0.11 – 1.49], BF = 5.13); this association was negligible for the 1-lap condition ($\beta = 0.60$, HPDI: [-0.05 – 1.27], BF = 1.66).

Discussion

We found no evidence that performance is greatly influenced by hearing the serial parade once rather than twice before making a decision. Therefore, it is unlikely that the number of laps explains the sequential advantage found in Smith et al. (2020), but not in Experiment 1. There is tentative evidence that the response criterion was more conservative for 2 lap parades, and signal sensitivity was higher for 1 lap parades. However, the variability

in parameter estimates warns against concluding that there is a systematic difference according to the number of laps. The results do suggest there is no advantage in hearing serial parades twice before making a decision; this may have important implications for guidance and could ultimately save police forces time when implementing voice parades.

As in Experiment 1, overall accuracy was low and false alarms were high. Consistent with Experiment 1, there was no relationship between CFMT+ scores and voice identification performance. There was a positive association with accuracy and confidence in the two lap condition, where correct responses were associated with higher confidence. However, participants do not appear to have reliable metacognitive awareness of the difficulty of voice identification; confidence ratings were high despite low accuracy.

General Discussion

The current paper reports that people are less likely to select an incorrect voice when given a ‘strong’ warning which asks them to consider their responses carefully to reduce the risk of a wrongful convictions. Crucially, they are not less likely to select the target. This is true for both serial and sequential parades. Furthermore, despite our predictions, there were no overall differences between performance on serial and sequential parades.

In keeping with previous literature, our results reveal overall low accuracy, high choosing rates, and particularly error-prone performance when the target voice is absent (Kerstholt et al., 2004; Öhman, Eriksson, & Granhag, 2011, 2013a, 2013b; Philippon, Cherryman, Bull, & Vrij, 2007; Smith et al., 2020). Although voices differ from each other (between-person variability), the same voice can sound very different across utterances (within-person variability) (Lavan, Burton, Scott, & McGettigan, 2019). Lower target-absent accuracy might indicate a bias towards (mis)attributing differences across the encoding and parade samples to within-person variability, making people unlikely to respond, ‘not present’.

However, on a target-present parade, people can extrapolate stable features across encoding and parade samples and so accuracy is higher (Kerstholt et al., 2006).

Our results highlight the potential for mitigating high error-rates on target-absent voice parades by including strong warnings. According to the cue-belief model (Leippe, Eisenstadt, & Rauch, 2009), parade decisions are informed by a sense of familiarity, and a subjective likelihood judgment about memory accuracy. Testing conditions influence which kind of information is relied upon. As an extrinsic cue, the strong warning may communicate task difficulty and encourage a reliance on the subjective likelihood of being accurate. This may make participants less likely to commit false alarms. However, the strong warning does not simply reduce choosing rates. Consistent with previous research, sensitivity was higher in the strong warning condition (Meissner et al., 2005). This suggests that the fidelity of familiarity cues remain intact and can override the inclination to reject the parade. Sensitivity was at chance level for standard warnings, meaning that participants were unable to distinguish between target and non-target voices. However, sensitivity was above chance level for strong warnings. A strong warning is a simple but effective way of safeguarding innocent suspects, who would otherwise likely only be afforded chance-level protection owing to low accuracy on target-absent voice parades (Kerstholt et al., 2004; Öhman et al., 2011, 2013a, 2013b; Philippon et al., 2007; Smith et al., 2020).

The present results underline the clear need for replication and thorough testing before policy recommendations are made (Malpass et al., 2008). Experiment 1 did not replicate higher accuracy for sequential compared to serial voice parades (Smith et al., 2020). The results of Experiment 2 suggest that this was not due to improved performance associated with listening to serial parades once (Experiment 1) rather than twice (Smith et al., 2020). There was a slight superiority for 1-lap parades, which enabled participants to distinguish between signal and noise. This was not the case for 2-lap parades, where d' was not different

from 0. However, the difference was small, and our failure to replicate is more likely due to the relatively noisy data, with error-prone performance subject to a host of factors encompassed by individual differences and stimulus effects.

Nevertheless, preliminary evidence from Experiment 2 suggests that there is no benefit from presenting the serial parade twice, which is consistent with conclusions drawn from the eyewitness sequential lap effect literature (Maclin & Phelan, 2007; Steblay et al., 2011; Lindsay et al., 1991; Horry et al., 2015). Even if voice representations are strengthened during the second listen, this does not improve the ability to compare the parade voices to memory of the target voice. In fact, familiarity cues to the target might even be diluted because all of the voices have been heard previously.

In the sequential procedure we implemented a strict stopping rule, in which the parade was terminated following the first ‘yes’ response. In comparison to a procedure where multiple responses are permitted, this may have harmed overall performance by prompting a conservative criterion shift (see Horry, Fitzgerald, & Mansour, 2020). Although this procedure facilitates a comparison with Smith et al. (2020), who also used a first-yes-counts sequential voice parade, we cannot rule out the possibility that alternative versions of the sequential parade might elicit more accurate performance. However, as false alarms are so high in voice identification, it is reasonable to test performance using a procedure designed to encourage earwitnesses to make absolute rather than relative judgments, as absolute judgments are less likely to lead to positive identifications (Dunning & Stern, 1994; Wells, 1984; Wells et al., 1998). Whilst Horry et al. (2020) found that a first-yes-counts protocol reduced the hit rate and compromised discriminability for eyewitnesses in comparison to a sequential control condition with no first-yes-counts procedure, this is not what the present study or Smith et al. (2020) observed for earwitnesses when comparing the sequential first-yes-counts procedure to a serial procedure. A serial procedure bears similarities to Horry et

al.'s (2020) sequential control condition: Parade members are presented one after the other, and participants encounter all parade members regardless of their decision.

There may be some overlap in the cognitive mechanisms driving face and voice identity processing (Belin et al., 2011; Bruce & Young, 1986; Jenkins et al., 2021). However, we did not observe any associations between participants' ability to memorise faces and voices in any of our analyses. This lack of association highlights that there are also important differences between face and voice memory: Voice identification performance is particularly error-prone (McAllister, Dale, & Keay, 1993; Stevenage et al., 2011). This could be because speech content is prioritized over identity-specific sound quality information (Fenn et al., 2011; Vitevitch, 2003). Whilst our aim here was not to pinpoint which specific cognitive mechanisms that might differ across faces and voices, the results do support our argument that findings from the eyewitness literature may not necessarily generalize across modalities. There is a clear need for earwitness procedures to be based on voice-specific research.

Although a positive relationship between accuracy and confidence tends to be observed in the context of unfamiliar face identification (Palmer, Brewer, Weber, & Nagesh, 2013; Wixted & Wells, 2017), for voices the relationship is often weak or even non-existent (Kerstholt et al., 2004; Ohman et al., 2011; Olsson, Juslin, & Winman, 1998). In the current study the relationship was unreliable, and varied according to testing conditions. We observed low accuracy but high confidence, perhaps because people conflate the ease of familiar voice identification with the difficulty of unfamiliar voice identification (Stevenage, 2018).

Our participant sample were aged between 18 and 75 years (mean 46 years). For the purposes of this research, a sample with a lower mean age might have been preferable to ensure effects were less likely to be due to age-related hearing loss (Hoffman et al., 2017). However, this would reduce generalizability of findings and exclude participants who might

be earwitnesses and be asked to listen to a voice parade. To account for potential age effects, we included participants age in all statistical models, and ensured data quality by removing data from participants who reported hearing problems. We do not believe that age limits our overall conclusions, but if anything may have added additional noise. There was very limited evidence that age predicted responses, with a weak relationship only observed in Experiment 2.

While a larger sample size would undoubtedly have increased the power of both experiments, our sample is substantially larger than those reported historically in earwitness literature with similar designs (e.g., Perfect et al., 2002; Kerstholt et al., 2006; Phillippon et al., 2013; Smith et al., 2020). In a recent meta-analysis on earwitness research, Johannsen, Wichert, and Leue (2021) suggest that a sample size of 151 participants is sufficient to detect a medium strength effect; the current study exceeded this minimum. We argue that any effect strong enough to have sufficient practical utility to be recommended as a procedural change would have been detected with our sample. Unlike eyewitness studies where researchers have the option of running several lineups and obtaining multiple data points per participant (Mansour, Beaudry, & Lindsay, 2017), low voice identification accuracy (Kerstholt et al., 2004, 2006; Smith et al., 2020) and the higher risk of interference (Stevenage et al., 2011) makes this unwise in earwitness studies. The cost of larger samples is therefore prohibitive. As put forward by Lakens (2021), our sample size was justified by both resource constraints and heuristics (the general norm followed in the literature).

Conclusion

Our results underline the value of system variable research in voice identification. We show that the serial procedure recommended by the Home Office can be adapted to provide additional protection for innocent suspects by adapting pre-parade instructions encouraging more conservative response behaviour. We also demonstrate that there appears to be no

advantage to asking listeners to listen to voices in a serial parade twice before making an identification decision.

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⁹ This link will be shorter when unmasked.

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Appendix A: Manipulation checks**Table A1**

Mean scores of post-experimental manipulation checks (standard deviation in parentheses).

Parade Type	Influence of Pre-parade Instructions ^I		Consider 'Not Present' ^{II}	
	Strong Warning	Standard Warning	Strong Warning	Standard Warning
Sequential	4.42 (3.15)	3.26 (2.95)	4.75 (3.35)	3.76 (3.14)
Serial (1 pass)	4.93 (3.03)	3.24 (2.7)	5.28 (3.43)	3.7 (2.7)
Serial (2 pass)	.	3.79 (2.81)	.	3.86 (3.33)

Note. ^I 'Before completing the parade, you were warned that the perpetrator may or may not be present. To what extent did this warning influence your decision(s)?' 0: The warning had no influence on my decision; 10: The warning had a strong influence on my decision.

^{II} 'To what extent did you consider responding that the perpetrator was not present/responding 'no' to each voice?' 0: Did Not Consider it at all; 10: Strongly Considered it.

Appendix B: Accuracy analysis (Experiment 1)**Table B1**

Accuracy results with main effects and interactions of FA warning, Parade type, Target presence (Experiment 1)

Predictor	β with HPDI	BF
Main effects		
Target presence	-3.82 [-5.36, -2.44]	> 100
Parade type	-0.63 [-2.18, 0.79]	1.13
FA warning	-1.75 [-3.15, -0.22]	9.45
Interactions		
FA warning $\times$ Target presence	-1 [-2.52, 0.41]	1.91
Parade type $\times$ Target presence	0.35 [-1.14, 1.76]	0.79
FA warning $\times$ Parade type	-0.13 [-1.66, 1.25]	0.78
Target presence $\times$ FA warning $\times$ Parade type	0.56 [-0.93, 1.97]	0.97
Covariates		
CFMT+	0.07 [-0.13, 0.27]	0.13
Confidence	0.22 [0.01, 0.42]	0.88
Age	-0.11 [-0.29, 0.1]	0.16

Note. β = most probable parameter value; HPDI = Highest Posterior Density Interval; BF = Bayes Factor in support of the alternative over the null hypothesis; main effects and interaction are sum coded; covariates are standardised.

Appendix C: Signal detection analysis (Experiment 1)**Table C1**

Estimates of the signal-detection theory model. Criterion c represents willingness to respond target present and d' indicates the signal sensitivity (Experiment 1).

Predictor	Response Criterion		Signal Sensitivity	
	c with HPDI	BF	d' with HPDI	BF
Main effects and Interaction				
FA warning	-0.63 [-1.35, 0.04]	1.82	-0.81 [-1.8, 0.03]	2.51
Parade type	0.21 [-0.45, 0.92]	0.44	-0.2 [-1.14, 0.64]	0.5
Parade type $\times$ FA warning	0.05 [-0.59, 0.74]	0.35	-0.14 [-1.04, 0.71]	0.46
Covariates				
Age	0.16 [0.02, 0.31]	1.03	0.02 [0, 0.23]	0.21
CFMT+	0.08 [-0.06, 0.24]	0.13	0.06 [0, 0.29]	1.13
Confidence	-0.14 [-0.29, 0.03]	0.32	0.17 [0, 0.38]	5.49
Cell means				
Sequential parade: standard warning	-0.89 [-1.5, -0.25]	8.98	0.08 [0, 1.15]	1.14
Sequential parade: strong warning	-0.62 [-1.16, 0.05]	3.01	0.77 [0.09, 1.83]	30.28
Serial parade: standard warning	-1.08 [-1.65, -0.4]	16.58	0.35 [0, 1.41]	2.84
Serial parade: strong warning	-0.71 [-1.25, -0.04]	4.39	0.8 [0.13, 1.91]	28.2

Note. HPDI = Highest Posterior Density Interval; BF = Bayes Factor in support of the alternative over the null hypothesis

Appendix D: Model comparison (Experiment 1)

The predictive performance was compared using leave-one-out cross-validation. The out-of-sample predictive performance was determined via Pareto smoothed importance-sampling (Vehtari et al., 2015, 2017) and estimated as the expected log predictive density ($elpd$) and the difference between the two models ($\Delta elpd$). We found negligible evidence that would support the use of an unequal variance model: $\Delta elpd = -6.75$ (SE = 4.96), fit of equal variance model: $elpd = -319.00$ (SE = 10.32).

Appendix E: Accuracy analysis (Experiment 2)**Table E1**

Accuracy results with main effects and interactions of FA warning, Parade type, Target presence (Experiment 2)

Predictor	β with HPDI	BF
Main effects and Interaction		
No. of laps	0.52 [-0.77, 1.89]	0.99
Target presence	-3.93 [-5.29, -2.64]	> 100
No. of laps $\times$ Target presence	0.53 [-0.87, 1.84]	0.87
Covariates		
CFMT+	-0.18 [-0.5, 0.17]	0.28
Confidence	0.42 [0.08, 0.79]	3.57
Age	-0.39 [-0.73, -0.06]	2.34

Note. β = most probable parameter value; HPDI = Highest Posterior Density Interval; BF = Bayes Factor in support of the alternative over the null hypothesis; main effects and interaction are sum coded; covariates are standardised.

Appendix F: Signal detection analysis (Experiment 2)**Table F1**

Parameter estimates of the signal-detection theory. Criterion c represents willingness to respond target present and d' indicates the signal sensitivity (Experiment 2)

Predictor	Response criterion		Signal sensitivity	
	c with HPDI	BF	d' with HPDI	BF
Main effect				
No. of laps	-0.41 [-1.03, 0.19]	0.68	-0.44 [-1.28, 0.19]	0.71
Covariates				
Age	0.02 [-0.2, 0.23]	0.11	0.01 [0, 0.2]	0.16
CFMT+	0.22 [-0.01, 0.46]	0.63	0.02 [0, 0.33]	0.98
Confidence	-0.08 [-0.34, 0.24]	0.17	0.34 [0.01, 0.69]	2.98
Cell means				
1 lap	-0.99 [-1.78, -0.15]	7.58	0.72 [0, 1.97]	4.54
2 laps	-1.34 [-2.25, -0.53]	21.93	0.07 [0, 1.3] 1.00	1.00

Note. HPDI = Highest Posterior Density Interval; BF = Bayes Factor in support of the alternative over the null hypothesis.

Appendix G: Model comparison (Experiment 2)

We compared the signal-detection model to an unequal variance model, a model with two different variance components for the distributions of target-present and target absent trials, which are frequently used in the literature (see Wixted et al., 2016). Apart from the two variance components, all parameter values were the same. Predictive performance was compared using leave-one-out cross-validation. The out-of-sample predictive performance was determined via Pareto smoothed importance-sampling (Vehtari et al., 2015, 2017) and estimated as the expected log predictive density ($elpd$) and the difference between the two models ($\Delta elpd$). We found negligible evidence that would support the use of an unequal variance model: $\Delta elpd = -0.49$ (SE = 5.05), fit of equal variance model: $elpd = -128.08$ (SE = 8.48)