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Eyewitness Identification and Distinctive Features: When Similarity Matters

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Abstract

In four experiments, we investigated theoretical and practical issues around eyewitness identification accuracy and confidence for tattooed suspects. We varied how tattoos were treated in lineups (Experiments 1 and 2) and the match between the suspect's tattoo the perpetrator's tattoo (Experiments 3 and 4). We replicated the finding that modifying lineup photographs to prevent a tattooed suspect from standing out mitigates the risk of innocent suspect identifications. We also demonstrated that sequential lineups (cf. simultaneous) do not mitigate the risk of biased lineups when the suspect stands out because of a tattoo. Contrary to previous research in which biased lineups did not impact correct identification rates differentially by lineup type, we found that biased lineups decreased correct identifications in sequential, but not simultaneous, lineups. Additionally, we found that the tattoo worn by an innocent suspect need not be identical to that of the perpetrator—similar placement and designs also inflate innocent suspect identifications, although a tattoo in a different location with a different design protected innocent suspects. Finally, our data indicate that when researching distinctive marks in lineups, researchers should request descriptions from the eyewitness-participants following the mock crime in order to determine whether the witness noticed the distinctive mark.

Keywords: eyewitness identification, tattoo, distinctive feature, biased lineup, simultaneous and sequential lineups

Eyewitness Identification and Distinctive Features: When Similarity Matters

Eyewitness identification evidence is compelling to triers of fact (e.g., Boyce, Beaudry, & Lindsay, 2007), so it is important that this evidence is as reliable as possible. That is, that an identification should be based on the eyewitness' memory for the perpetrator and not on any other source of information. The more obvious it is which lineup member is the suspect, the less an eyewitness needs to rely on their memory and the more they can rely on inference when making their decision (Buckhout, Figueroa, & Hoff, 1975; Colloff, Wade, & Strange, 2016; Lindsay, Wallbridge, & Drennan, 1987).

Lineups in which the suspect stands out relative to the known-innocent fillers are referred to as biased or unfair. Such lineups typically result in a larger increase in innocent suspect identifications than guilty suspect identifications (heretofore correct identifications). For example, Lindsay et al. (1987) found that when the suspect was the only member of a lineup wearing clothes that matched those worn by the perpetrator at the time of the crime, eyewitnesses identified the innocent suspect 28% of the time (cf. 11% when clothing varied among lineup members; an increase by a factor of 2.54), whereas eyewitnesses identified the guilty suspect 78% of the time (cf. 67% when clothing varied among lineup members; an increase by a factor of 1.16).

Unlike clothing, which can be easily changed, distinctive marks on a suspect are not easily removable. According to a Pew Research Center study (2010), 38% of Americans aged 18 to 29 have at least one tattoo and approximately 18% of those with tattoos have six or more. Although not all tattoos are visible, approximately 10% of males and 6% of females in this age range have at least one tattoo that is normally visible even when fully clothed. Given tattoos' popularity, it is crucial that we understand the effect these distinctive features have on eyewitnesses' identification decisions and confidence.

Marks such as tattoos raise complex issues for the construction of police lineups. Consider the recent case of Tyrone Lamont Allen (Bernstein, 2019a; 2019b). Allen, who has highly visible facial tattoos, was suspected by police of multiple bank heists. However, none of the eyewitnesses reported seeing tattoos on the face of the perpetrator, nor were any apparent in the CCTV footage. The police argued that Allen may have disguised himself during the robbery by using makeup to hide his tattoos. In an effort to present an unbiased lineup, the police altered their image of Allen by painting over the facial tattoos “almost like applying electronic makeup,” according to the news report. The result was that the bald, tattooed Allen with stubble now appeared to have close-cut hair, a mustache, and a beard. Although Allen now did not stand out in the lineup, his adjusted appearance more closely matched the appearance of the perpetrator captured by the CCTV camera—potentially making Allen more likely to be identified than he would have been had his unaltered picture been displayed. The court recently ruled that the lineup was admissible (Bernstein, 2019b). This case highlights the need to understand how police decisions about dealing with tattooed suspects influence eyewitness identification. How does the appearance of distinctive marks influence eyewitness identification accuracy and confidence?

Tattoos and Other Distinctive Marks

A small literature has explored the biasing influence of distinctive marks on simultaneous lineup decisions. Zarkadi, Wade, and Stewart (2009) explored two different methods to create unbiased lineups when face images at study had a distinctive mark (i.e., bruise, mole, piercing, mustache, scar, or tattoo). They found that replicating the mark on all lineup members led to more correct identifications (Experiment 1: ~.55, Experiment 2: ~.50).¹ than removing the mark from the suspect (Experiment 1: ~.36, Experiment 2: ~.29). No

¹ The approximated identification rates are based on our analysis of Zarkadi et al.’s (2009) figures using Engauge Digitizer Software, Mitchell, et al. (2019)

significant difference emerged between the two treatment conditions for mistaken identifications from target-absent lineups (Experiment 2: $\sim .65$ and $\sim .68$, respectively). This result makes sense as removing a mark will alter the appearance of a guilty suspect, while adding it to others provides an unbiased identification opportunity of a guilty suspect with an unaltered appearance. Using the same stimuli, Badham, Wade, Watts, Woods, and Maylor (2013) replicated these results but showed that replicating the mark (cf. removing it) was more beneficial for younger adult eyewitnesses than for older adults. Colloff et al. (2016) used a more ecologically valid approach by showing participants a single mock-crime video (as opposed to static images) prior to a photographic lineup decision. They also included a biased lineup condition (only the suspect had the distinctive mark) and tested possible conditions to create unbiased lineups by applying the same treatment to all lineup members: replicating the mark, pixelating the mark, or blocking the mark (e.g., with a black square). They found no differences between the various unbiased treatments on innocent (.09 for all three conditions) or correct identifications (.31, .28, and .29, respectively). However, their study was the first to demonstrate that these unbiased treatments of distinctive marks reduce innocent and correct identifications compared to a biased lineup (.36 innocent and .57 correct identifications).

Theoretical Considerations

A variety of theoretical frameworks are relevant to the issue of distinctive marks and lineup decision making. Zarkadi et al. (2009) discussed their results as consistent with hybrid similarity models of recognition (e.g., Knapp, Nosofsky, & Busey, 2006; Nosofsky & Zaki, 2003). The hybrid similarity global context model (HS-GCM) assumes that when we study something, we store each studied instance of it in our memory (in the eyewitness context this could mean each fixation or separate focus of attention on the perpetrator). When we later must decide whether a presented test image was studied, we compare the similarity between

the presented image and each stored instance and then sum these similarity judgments to obtain an overall familiarity value, which we use to make our decision (the summed similarity rule). These stored instances cluster within a continuous dimension of psychological space. Within the global context model (GCM), the summed similarity rule predicts better memory for typical items (i.e., items highly similar to other experienced items); however, this conflicts with the evidence that distinctive items (i.e., items highly dissimilar to other experienced items) tend to be remembered better than typical items, particularly in terms of faces (e.g., Vokey & Read, 1992). To resolve the conflict, the HS-GCM posits a mechanism derived from Tversky's (1977) feature contrast model—specifically, the concept of self-similarity. That is, the HS-GCM incorporates the idea that similarity between two items (e.g., a presented image and a stored instance) is a function of both the shared features of the two items and their positions in psychological space. Thus, when an image of the perpetrator is presented and the perpetrator had a distinguishing mark, the distinguishing mark increases the similarity ratings compared to similarity ratings for a perpetrator without a distinguishing mark—and, therefore, increases the overall feeling of familiarity with the image.

Thus, the HS-GCM (Nosofsky & Zaki, 2003) predicts the higher hit rate that Zarkadi et al. (2009) and Badham et al. (2013) found when they replicated distinguishing marks rather than removed them. The HS-GCM also successfully predicts the target-absent lineup findings from Colloff et al. (2016). Innocent suspect identifications were higher when the lineup was biased than unbiased presumably because only the suspect received a boost in familiarity due to the distinctive mark. In the case of the replication condition, the boost in familiarity due to the distinctive mark would have occurred for all lineup members. Thus, because the innocent suspect can be expected to be as similar to the perpetrator as any filler, the model predicts

that removal, replication, pixelation, and blocking can be expected to—and did in Colloff et al. (2016)—result in similar innocent suspect identification rates.

However, the findings from Colloff et al.'s (2016) target-present lineups are problematic for this model. The HS-GCM (Nosofsky & Zaki, 2003) cannot account for why there were more correct identifications in the biased lineup compared to the replication lineup or why the replication condition resulted in similar correct identifications as the pixelation and blocking conditions. Additionally, a critical limitation of the HS-GCM is that it does not provide a role for comparisons among test items—such as may occur when participants view an array of lineup members. Rather, the HS-GCM (Nosofsky & Zaki, 2003) focuses on comparisons between one's memory and a single test item. Thus, assuming eyewitnesses compare each lineup member to their memory for the perpetrator, the HS-GCM would predict that, for target-present lineups, correct identifications in the replication condition would be equivalent to the biased lineup and better than the pixelation or blocking conditions. Colloff et al. suggested that the equivalence in correct identifications between their three unbiased lineup conditions may have occurred because the pixelation and blocking lineups had indicators (blurry area, black square) that a distinctive mark—such as that seen on the perpetrator—could be present on the lineup member. They reasoned that an indicator of a distinctive mark in a relevant location may have decreased the likelihood that participants rejected the lineup on the basis of the missing feature, which could have occurred in Zarkadi et al. (2009) and Badham et al. (2013). Yet, this interpretation does not resonate well with the HS-GCM, which suggests that it is the actual match between the distinctive feature in the lineup and the previously-seen distinctive feature—and not the possibility of a match—that provides the memorial advantage.

A potential mechanism for the conflicting findings with target-present lineups is provided by the outshining hypothesis, which has been used to account for the finding that

reinstating context in recognition tasks sometimes increases hits and sometimes does not (Smith & Vela, 2001). Smith and Vela state that outshining “refers to the idea that the environment can be suppressed at test, diminishing the likelihood that ambient environmental information will be used in the construction of memory probes” (p. 206). Smith (1988), in his first description of the outshining hypothesis, explained that one cue could become more informative than another by virtue of factors such as how many other items were associated with the cue.

The outshining hypothesis has mixed support in the eyewitness context. Krafka and Penrod (1985) and Malpass and Devine (1981) found that imagining a mock crime’s context led to higher correct identifications than a no-imagination control condition. In contrast, Sanders (1984) found that presenting all lineup members with a background that was the same as the one they witnessed the perpetrator in (cf. a different one) had no effect on correct identifications. All three results are consistent with the outshining hypothesis. Context reinstatement was not associated with multiple items, just the target item (the perpetrator) in the last two studies, whereas in Sanders, the context cue (i.e., the background) was associated with many considered items (i.e., all lineup members) and, therefore, perhaps any memory boost provided by the cue was distributed equally to all items. Yet Smith and Vela (1992) found that a lineup decision made in the same environmental context as the event was witnessed increased correct identifications compared to when the decision was made in a different context. One potential explanation for this conflicting finding is that participants in Smith and Vela had weak memories for the perpetrator, so context was able to assist their memories (see Smith, Handy, Hernandez, et al., 2018), despite being associated with multiple items.

Although support for the outshining hypothesis in an eyewitness context is mixed, the outshining hypothesis can account for Colloff et al.’s (2016) finding that the biased lineup

resulted in more correct identifications than the replication lineup: the usefulness of the distinctive mark as a cue would be higher in the biased lineup than the replication lineup because it is associated with fewer items (i.e., just the suspect vs. the suspect and all fillers), provided we can reasonably assume a relatively weak memory for the perpetrator (which is plausible given that only .57 of participants identified the guilty suspect even when shown a biased lineup).

Both of the aforementioned approaches have limitations. The HS-GCM (Nosofsky & Zaki, 2003) does not consider how testing environment affects recognition. The outshining hypothesis (Smith, 1988) is difficult to test because of the challenge in knowing the strength of a participant's memory for the event. Two additional theoretical frameworks that do not suffer from these limitations—filler siphoning (Wells, Smalarz, & Smith, 2015) and the diagnostic feature detection (DFD) model (Wixted & Mickes, 2015)—are also relevant to eyewitnesses' responses to lineup members with distinctive features.

Wells, et al. (2015) proposed that fillers siphon identifications away from the suspect—so while overall selection rates do not change, fillers ensure they are spread amongst multiple lineup members rather than focused on the suspect. They posit that this is true to the extent that fillers are competitive (i.e., are relatively similar to the suspect). Smith, Wells, Smalarz, et al. (2018) discussed how filler siphoning accounts for the higher suspect identification rate that Colloff et al. (2016) found for their biased lineups. Specifically, fillers in unbiased lineups are competitive, whereas fillers in biased lineups are much less so; thus, selections were spread across all lineup members in the unbiased lineup and more heavily concentrated on the suspect in the biased lineup. Alternatively, Colloff et al. used the DFD model (Wixted & Mickes, 2014) to predict the same result but via a different mechanism. The DFD model posits that eyewitnesses evaluate the features of each lineup member that are not the same across all lineup members (i.e., the diagnostic features) and compare them to

their memory of the perpetrator. According to the DFD model, the suspect will be chosen at a higher rate to the extent that a tattoo—or any other feature—differentiates the suspect from other lineup members and also matches the eyewitness' memory for the perpetrator. Thus, filler siphoning and the DFD model make the same predictions about distinctive marks—a suspect will be chosen more often when a distinctive mark is unique to them, compared to when the mark is absent or is not distinctive/unique to them—and both can account for the extant results.

To summarize, all four theoretical approaches predict that a biased lineup with a visible distinctive mark will lead to 1) more innocent suspect identifications than any unbiased lineup and 2) more correct identifications than an unbiased lineup in which lineup members have no visible distinctive marks (although the outshining hypothesis predicts more or equivalent correct identifications). However, the various theories make different predictions about correct identifications when a tattoo is visible on only the suspect (i.e., biased) versus when a tattoo is replicated onto all lineup members (i.e., unbiased): The HS-GCM (Nosofsky & Zaki, 2003) predicts no difference, the outshining hypothesis (Smith, 1988) predicts either an increase or no difference (depending on memory strength), whereas filler siphoning (Wells, et al., 2015) and the DFD model (Wixted & Mickes, 2014) predict an increase. The theories also make differential predictions about the rate of correct identifications from a biased, tattoo visible lineup compared to a biased, tattoo not visible lineup (i.e., tattoo is blocked on the suspect): the HS-GCM predicts the former condition will result in more correct identifications than the latter, with the other theories predicting no difference. Finally, the HS-GCM predicts that innocent suspect identifications will be greater in a biased, tattoo visible lineup compared to a biased, tattoo not visible lineup while the remaining theories predict no difference between these two conditions. In the following experiments, we test different ways of presenting lineups with an eye towards replicating

prior results and procuring evidence that may shed light on the applicability of these theoretical frameworks to eyewitness identifications of tattooed suspects. Supplementary Table 1 summarises these predictions.

Confidence

Confidence provides more useful information about identification accuracy than previously believed (Brewer & Wells, 2006; Juslin, Olsson, & Winman, 1996; Wixted & Wells, 2017), but biased lineup construction can influence eyewitness confidence. In particular, Leippe and Eisenstadt (2009) found that when one lineup member stands out from the rest, eyewitnesses use this “standing out” as a cue that they have made the correct response, which subsequently inflates their confidence (i.e., the cue-belief model). Only Colloff et al. (2016) have considered confidence in terms of distinctive marks. They found overconfidence for biased lineups, consistent with Leippe and Eisenstadt’s prediction. In their supplementary materials, they also noted that even in their fair lineup condition, highly confident eyewitnesses were somewhat less accurate than is typically found, perhaps because the distinctive feature could not be used as a retrieval cue.

Findings from the environmental context reinstatement literature provide additional support for Leippe and Eisenstadt (2009). For instance, eyewitnesses reported higher confidence in correct identifications when they made them in the same (cf. different) context condition (Smith & Vela, 1992). Sanders (1984) also found higher confidence for their same context condition but this was true for both guilty and innocent suspect identifications. Krafka and Penrod (1985) found a stronger confidence–accuracy correlation when context was reinstated versus not. Malpass and Devine (1981) found context reinstatement increased confidence in correct identifications only. Thus, this literature suggests that confidence and the confidence–accuracy relationship might be increased by the presence of a distinctive mark, something we explore in the current experiments.

Current Experiments

More research is necessary to understand how distinctive marks affect eyewitness identifications and confidence. The inconsistent relationship between extant findings and theory, in particular, signals this need. In this paper, we aim to build on prior work by replicating previous findings and consider some practical challenges that might occur when police have suspects with distinctive marks. In four experiments, eyewitness participants viewed mock-crime videos in which perpetrators had a tattoo and then viewed lineups consisting of lineup members under various tattoo treatment conditions.

Our first experiment had several aims. First, we attempted to replicate Colloff et al.'s (2016) finding that distinctive marks (specifically tattoos) shown only on a suspect lead to an increase in suspect identifications compared to when the lineup is constructed in an unbiased way. Second, we attempted to replicate the finding of no difference between different unbiased lineups created for tattooed suspects (Badham et al., 2013; Colloff et al., 2016; Zarkadi et al., 2009). Finally, we explored how unbiased and biased lineups influence confidence in suspect identifications.

In Experiment 2 we considered a more practical issue. To our knowledge, all research examining distinctive marks on eyewitness identifications has been conducted using simultaneous lineups. None we are aware of has considered whether the treatments for distinctive marks are equally effective when used with sequential lineups, or whether the sequential lineup protects against biased tattoo lineups. In this experiment, we examine biased versus unbiased tattoo treatment with sequential lineups because many jurisdictions in Canada (Beaudry & Lindsay, 2006; Bertrand et al., 2018) and the United States of America (Wogalter, Malpass, & McQuiston, 2004; Greene & Evelo, 2014) use photographic sequential lineups.

Experiments 3 and 4 addressed more nuanced issues that may arise in practice. That is, people often obtain similar tattoos—perhaps because the tattoos indicate group membership (e.g., gangs) or because of fashion trends. In Experiment 3, we explore the extent to which an innocent suspect’s likelihood of identification is affected by having a tattoo design and location that is the same as, or different from, the perpetrator’s. Experiment 4 tests whether our Experiment 3 findings extend to subtler changes, wherein an innocent suspect’s tattoo (cf. the perpetrator’s) has the same or a similar design and appears in the same or a similar location. Taken together, these experiments explore how police should construct lineups when the perpetrator and suspect have a tattoo and examine how tattoo similarity influences the risk of misidentification of an innocent suspect. Furthermore, although we did not aim to test the various theoretical explanations, the results of these experiments could provide evidence of support (or lack thereof) for the four aforementioned theories.

Experiment 1

The current experiment investigates five possible approaches that police may take when a suspect has a distinctive feature. These approaches are all ones used in practice in Canada and the USA, based on previously unreported data from the Bertrand et al. (2018) survey (see Table 1). Two treatments resulted in biased lineups in which the suspect stood out as a result of being the only lineup member with a tattoo or a patch covering the tattooed area. Three treatments produced unbiased lineups in which the tattoo or patch was replicated on all lineup members, or the tattoo was digitally removed from the suspect to create a lineup without any tattooed members.

In addition to informing practical considerations when the police have suspects with distinctive features, the results of this experiment could also shed light on the four aforementioned theories in terms of how tattoo visibility and lineup bias affect guilty and

innocent suspect identifications. In terms of correct identifications, the HS-GCM (Nosofsky & Zaki, 2003) predicts that, regardless of the treatment of other lineup members, correct identifications will be higher when the tattoo is visible than not visible, with similar rates in the various conditions when the suspect's tattoo is visible. The outshining hypothesis (Smith, 1988) suggests correct identifications could be the same or higher in biased compared to unbiased lineups, regardless of tattoo visibility. Filler siphoning (Wells et al., 2015) and the DFD model (Wixted & Mickes, 2014) suggest that correct identifications will be higher in biased lineups than any unbiased treatment condition (with no expected differences in the unbiased tattoo treatment conditions).

In terms of innocent suspect identifications, the HS-GCM and the outshining hypothesis predict higher rates for the biased lineup in which the tattoo is visible, but similar rates amongst the remaining conditions. In contrast, the two eyewitness identification theories, filler siphoning (Wells et al., 2015) and DFD (Wixted & Mickes, 2014) predict higher innocent suspect identifications when the innocent suspect includes any cue that other lineup members do not share (i.e., a tattoo or a patch) and that could facilitate the conclusion that the suspect is the perpetrator compared to an unbiased lineup. We summarize the predictions from these theories in Supplementary Table 1.

In terms of confidence, based on the aforementioned research, we expected that confidence in guilty and innocent suspect identifications would be higher in biased than unbiased lineups. We also hypothesized that confidence would be higher in a biased lineup where the tattoo was visible compared to a biased lineup where the tattoo was concealed because the visible tattoo should result in a stronger match between the eyewitness' memory and the suspect. Following this logic, we predicted no difference in confidence in suspect identifications among the unbiased lineup conditions because no cue exists to "confirm" the identification decision is correct (Leippe & Eisenstadt, 2009).

Method

Ethical approval for all experiments reported in this manuscript was received from the relevant institutional review board.

Participants

Community members ($N = 101$; .66 female) volunteered to participate in this study. Participant ages ranged from 17 to 61 years ($M = 30.18$, $SD = 10.23$) and the majority (.78) self-reported as White.

Design

This experiment used a 5 (Tattoo Treatment: biased tattoo, biased blocking, unbiased blocking, unbiased replication, unbiased removal) x 2 (Target Presence: present, absent) mixed-factorial design with Tattoo Treatment manipulated between-subjects and Target Presence manipulated within subjects. Participants were randomly assigned to a Tattoo Treatment condition, which determined how the tattooed suspect and fillers were presented in the lineup (see Lineups subsection for further detail). All participants watched all eight mock-crime videos and completed four target-present and four target-absent lineups with counterbalancing used to ensure that each perpetrator appeared approximately equally often in target-present and target-absent lineups across participants.

Materials

Participants completed the experiment on a desktop computer and wore a set of headphones so they could hear the audio in the videos. We used MediaLab© (Jarvis, 2006) to program and run the experiment.

Mock-crime Videos. Brief videos (approximately 3 seconds) captured a perpetrator's face, neck, and shoulders while they spoke. The clear videos were presented full-size on a 15" screen. Each perpetrator was White and had a visible tattoo either on the side of, or across, their neck. The videos displayed one of two staged scenarios: being questioned by an

off-screen police officer after a robbery, or plotting a murder over the phone. Half of the videos presented the first scenario and the other half presented the second. We used eight perpetrators (half male and half female, split evenly across scenarios) to ensure appropriate stimulus sampling (Wells & Windschitl, 1999).

Lineups. Six-person simultaneous lineups contained five fillers and the perpetrator (target-present) or an innocent suspect (target-absent). Target-absent lineups were constructed by selecting six fillers, one of whom was randomly allocated to be the innocent suspect. All lineup members were independent (i.e., not used in more than one lineup) and matched the general description of the perpetrator. We selected lineup members from our pool of approximately 500 images using the iterative match-to-description procedure (Turtle, Lindsay, & Wells, 2003; Mansour et al., 2012). One filler was randomly selected to be the innocent suspect. Eyewitness participants could make one of three decisions for each lineup: (a) select one of the lineup members, (b) indicate the perpetrator was “not there”, or (c) respond “don’t know.”

Figure 1 illustrates how lineups varied according to Tattoo Treatment. Two of the Tattoo Treatment conditions were biased—either because a tattoo was visible on only the suspect [biased tattoo; Figure 1A] or because the suspect had the tattooed area blocked while no such blocked area was present on any other lineup members [biased blocking; Figure 1B]. In three of the five conditions, the lineups could be considered unbiased because the suspect did not differ from other lineup members in terms of how the tattoo had been treated. Either the area was blocked on all lineup members [unbiased blocking; Figure 1C], the tattoo was replicated on all lineup members [unbiased replication; Figure 1D], or the tattoo was edited off the suspect [unbiased removal²; Figure 1E].

² Zarkadi et al. (2009) referred to this treatment as concealment. To prevent confusion with our blocking condition, we use the term removal, following Badham et al. (2013).

Following data collection, we sought to determine whether our innocent suspect stood out from the other non-tattooed lineup members, even when shown without a tattoo. We presented each of the unbiased removal target-absent lineups to 36 independent participants and asked them to choose the face that most resembled the picture of the yoked perpetrator. Neither the perpetrator nor the target-absent lineup members were shown with a tattoo. Participants made their decisions while looking simultaneously at images of the perpetrator and the associated lineup. The average rate of selection of the innocent suspects was .07 (*Range*: 0–.17), indicating that the lineups used were not biased towards the innocent suspect when tattoos were absent (see also Supplementary Table 2).

Lineup Instructions. Prior to completing any mock-crime/lineup trials, participants read instructions about their task. First, it was explained that they would be watching a video and so should put on the headphones and alert the researcher if they could not hear the sound of the videos when they were presented. Next, they were informed that the perpetrator may or may not be present in the lineup and that they could choose a lineup member, say the perpetrator was not there, or respond “don’t know”. Participants were also informed that they could make only one selection per lineup and that they would not be able to change their decision after making it. Participants were then told that they would be asked for a confidence rating, had the scale described (0% [*not at all confident*] to 100% [*extremely confident*]), and were informed that no face would appear in more than one lineup. Finally, participants were asked to contact the experimenter if they had any questions.

Procedure

The entire procedure took place on a computer. After consenting to participate, participants provided demographic information, read the task instructions, and then completed the eight trials. Each trial involved watching a mock-crime video, making a lineup decision, and then providing a confidence judgement. Participants were not made aware of

the number of target-present and target-absent lineups. After completing the eight trials, participants were debriefed and thanked.

Measures and Indices

Lineup responses were coded as suspect identifications, filler identifications, or rejections (consisting of both rejections and don't know responses). Participants completed four target-present and four target-absent lineup trials, therefore they could receive an accuracy score of 0-4 for each lineup type. We present mean proportions of suspect identifications from target-present lineups (correct identifications) and target-absent lineups (innocent suspect identifications) as well as mean confidence in correct identifications and innocent suspect identifications. For analyses we account for participant-level effects but for our descriptives, we collapse across this variable. The differences in this approach versus using means for each participant to calculate a sample-level mean were minute and inconsequential to our conclusions.

In order to test our hypotheses, the levels of tattoo treatment were recategorized to create relevant binary variables (see Supplementary Table 1). First, a variable representing *lineup bias* was created such that one level comprised the biased tattoo and biased blocking conditions and the other level comprised the remaining conditions (unbiased blocking, unbiased replication, unbiased removal). Second, a binary *tattoo visibility* variable was created. For this variable, the biased tattoo and unbiased replication groups comprised one level and the remaining groups (biased blocking, unbiased blocking, unbiased removal) comprised the other.

Results

All analyses were conducted in R (R Core Team, 2019). Because the current experiment used a multiple trials approach, we used multilevel modelling to analyse the data with the lme4 package (Bates, Maechler, Bolker, et al., 2020). We also calculated Bayes

factors (BF) using the bayestestR package (Makowski, Lüdtke, Ben-Shachar, et al., 2020) and the BayesFactor package (Morey & Rouder, 2018). Reported BFs indicate evidence for the more complex model unless otherwise stated. Table 4 from Jarosz and Wiley (2014) provides guidance for interpreting Bayes Factors. That is, a BF of 3 or less is weak/anecdotal, 3–10 is positive/substantial, 10–20 is positive/strong, 20–30 is strong, 30–100 is strong/very strong, 100–150 is strong/decisive, and greater than 150 is very strong/decisive.

We followed Lorah (2018) for reporting effect sizes for our multilevel models. As we were only interested in fixed effects, we report Cohen's f^2 , which can be interpreted according to these guidelines: a small effect is ≥ 0.02 , a medium effect is ≥ 0.15 , and a large effect is ≥ 0.35 (Cohen, 1988). Estimated marginal means were calculated using the emmeans package (Length, Singmann, Love, et al., 2020).

Correct Identifications

Table 2 displays the mean proportion of each possible type of lineup response across participants and trials. Because each participant completed four target-present lineup trials, we first considered whether the data needed to be analysed using multilevel models. That is, trials are nested within participants, therefore for all analyses we first consider whether a random effect of participant should be included in our model. Indeed, the two-level model with no predictors resulted in a better fit than a one-level model (i.e., no random effect of participant) with no predictors, $\chi^2(2) = 36.58$, $p < .001$, BF = 214792.58, so we used the former as our basis for comparison. Next, we checked for practice effects since participants completed eight lineup trials and this may have influenced their responding (c.f. Mansour, Beaudry, & Lindsay, 2017). Adding trial number to the two-level null model did not improve model fit, $\chi^2(1) = 3.49$, $p = .062$, $f^2 = .01$, BF = 3.50 (for the null), so was not included in further models. We next compared the two-level null model to a model with tattoo treatment as a predictor (with biased tattoo as the reference group). This model was not a significantly

better fit to the data than the null two-level model, $\chi^2(4) = 4.34$, $p = .36$, $f^2 = .01$, $BF < 0.001$. However, because this model does not directly address our hypotheses, we performed additional tests.

Colloff et al. (2016) found more correct identifications from biased compared to unbiased lineups. To see if we replicate this result in our sample, we added our *lineup bias* variable as a predictor to the null two-level model and compared the resulting model to the null two-level model. Fit was not improved, indicating similar correct identification rates regardless of whether the lineup was biased or not, $\chi^2(1) = 0.33$, $p = .56$, $f^2 = .001$, $BF = 0.06$. This result is consistent with the outshining hypothesis only.

In order to test whether the visibility of the tattoo influenced correct identifications, we constructed a model with *tattoo visibility* as a predictor. This model did not improve fit relative to a null two-level model, $\chi^2(1) = 0.60$, $p = .44$, $f^2 = .002$, $BF = 0.07$, contrary to our hypothesis and inconsistent with the HS-GCM.

Next, we examined whether the predicted lack of difference between the three unbiased lineup conditions was present. To that end, we included tattoo treatment as a predictor of the data from only participants in the unbiased blocking, unbiased replication, and unbiased removal groups. Compared to the null two-level model, including tattoo treatment as a predictor did not improve model fit, $\chi^2(2) = 3.22$, $p = .20$, $f^2 = .02$, $BF = 48.06$ (for the null). This result is also inconsistent with the predictions of the HS-GCM. Overall, the outshining hypothesis is the only theory that successfully predicts the target-present results.

Innocent Suspect Identifications

We first considered whether a multilevel model was necessary to examine these data. Fit was improved significantly when we used the two-level compared to the one-level null model to predict innocent suspect identifications (compared to any other response), $\chi^2(2) = 79.18$, $p < .001$, $BF = 3.87 \times 10^{14}$. Adding trial number did not further improve fit, $\chi^2(1) =$

1.78, $p = .18$, $f^2 = .002$, $BF = 8.27$ (for the null). Next, we constructed a model with tattoo treatment as the predictor (using the biased tattoo group as the reference category). Fit to the data was significantly improved by including tattoo treatment as a predictor, $\chi^2(4) = 13.00$, $p = .011$, $f^2 = .05$, $BF = 0.004$. The very small Bayes Factor despite a significant result, suggests that the differences between the various treatment conditions are not all meaningful. Consistent with this interpretation, not all pairwise comparisons were significant. The biased tattoo lineup resulted in more innocent suspect identifications than the unbiased replication lineup, $t(101) = 2.75$, $p = .007$ ³ (consistent with all theories), and unbiased removal lineups, $t(101) = 3.17$, $p = .002$ (consistent with all theories), but unexpectedly, did not differ from the unbiased blocking lineups, $t(101) = 1.28$, $p = .20$ (inconsistent with all theories). The biased tattoo and biased blocking lineups also did not differ significantly, $t(101) = 0.87$, $p = .39$ (consistent with filler siphoning and the DFD model, inconsistent with the HS-GCM and the outshining hypothesis). We followed up with an analysis of *tattoo bias* in order to obtain the relevant Bayes Factor and effect size estimate for the effect of presenting a biased versus unbiased lineup, $\chi^2(1) = 8.53$, $p = .004$, $f^2 = .03$, $BF = 3.55$ (see also Table 2).

We conducted a separate analysis of the subset of participants who viewed biased lineups, which compared the model with *tattoo visibility* to the null two-level model; the results confirmed that tattoo visibility did not affect innocent suspect identifications, $\chi^2(1) = 0.42$, $p = .52$, $f^2 = .004$, $BF = 0.10$. Unlike the target-present lineup results, these two results are consistent with filler siphoning and the DFD model.

Finally, we expected that there would be no difference in innocent suspect identifications between participants who viewed the unbiased lineups. To test this hypothesis, we entered tattoo treatment as a predictor (using the unbiased replication lineups as the

³ We used Satterthwaite's (1946) approximation for degrees of freedom and p -values for this and all other pairwise comparisons of parameter estimates in multilevel models.

reference group) but excluded the biased lineup conditions from analysis. Contrary to our expectations, including tattoo treatment improved model fit (although the BF was in favour of the null), $\chi^2(2) = 7.86$, $p = .020$, $f^2 = .04$, $\text{BF} = 4.71$ (for the null). As illustrated by Table 2, innocent suspect identifications were higher in the unbiased blocking group than the unbiased replication, $t(60) = 2.15$, $p = .036$, or unbiased removal groups, $t(60) = 2.76$, $p = .008$ (inconsistent with all theories).

Confidence in Correct Identifications

As with accuracy, we first examined whether a multilevel model was a better fit to the confidence in correct identifications data than a single-level model, and indeed it was, $\chi^2(2) = 112903.76$, $p < .001$, $\text{BF} = 1.09 \times 10^7$. In contrast to the data for accuracy, however, including trial number in the model did improve fit, $\chi^2(1) = 4.08$, $p = .043$, $f^2 = .01$, $\text{BF} = 0.41$. Because trial number was a significant predictor, we included it as a predictor in the rest of our confidence models (i.e., this model became our base model). Adding tattoo treatment as a predictor (with biased tattoo as the reference group) did not further improve fit, $\chi^2(4) = 4.10$, $p = .40$, $f^2 = .02$, $\text{BF} < .001$. Table 3 provides descriptive information for confidence.

We hypothesized that confidence in correct identifications would be higher when the lineup was biased than unbiased. However, entering *lineup bias* as a predictor did not improve fit relative to the base model, $\chi^2(1) = 1.23$, $p = .27$, $f^2 = .01$, $\text{BF} = 10.05$.

We also hypothesized that for participants who viewed biased lineups, confidence would be higher when the tattoo was visible rather than concealed. To test this prediction, we examined the subset of participants who viewed biased lineups. Compared to the base model, a model with *tattoo visibility* as a predictor did not improve fit, $\chi^2(1) = 2.00$, $p = .16$, $f^2 = .02$, $\text{BF} = 4.39$ (for the base model).

Finally, we hypothesized that eyewitness confidence would not differ amongst the unbiased lineup conditions. For this analysis, we examined the subset of participants who

viewed unbiased lineups. Compared to the base model, and consistent with our hypothesis, including tattoo treatment as a predictor did not improve the fit, $\chi^2(2) = 0.90$, $p = .64$, $f^2 = .01$, $BF = 129.94$ (for the base model). No pairwise differences were found when we examined the parameter estimates (all $ps > .61$).

To summarize, the tattoo treatment conditions had no significant effect on eyewitness confidence in their correct identifications.

Confidence in Innocent Suspect Identifications

A null two-level model provided a significantly better fit to the data for confidence in innocent suspect identifications than a null one-level model, $\chi^2(1) = 23841.65$, $p < .001$, $BF = 0.58$. Adding trial number to this model improved fit to the data, $\chi^2(1) = 5.96$, $p = .015$, $f^2 = .12$, $BF = 2.84$. Thus, we used this model as the base model for further analyses. Adding tattoo treatment (with biased tattoo as the reference group) to the base model did not improve fit, $\chi^2(4) = 2.55$, $p = .64$, $f^2 = .05$, $BF = 645.24$ (for the base model; see Table 3).

Contrary to our hypothesis that innocent suspect identifications from biased lineups would be made with higher confidence if the tattoo was visible compared to concealed, a model with *lineup bias* was not a significantly better fit to the data than the base model, $\chi^2(1) = 0.18$, $p = .67$, $f^2 = .01$, $BF = 6.34$ (for the base model).

We conducted our next analysis on only participants in the biased tattoo and biased blocking conditions. Also contrary to our hypothesis, *tattoo visibility* did not significantly improve model fit compared to the base model, $\chi^2(1) = 1.22$, $p = .27$, $f^2 = .03$, $BF = 3.02$ (for the base model).

Finally, we examined the subset of participants in the unbiased lineup conditions. Our hypothesis that confidence would not differ across the unbiased lineup conditions was supported. The model with tattoo treatment (with unbiased replication as the reference group) did not improve model fit compared to the base two-level model, $\chi^2(2) = 1.02$, $p = .60$, $f^2 =$

.08, $BF = 10.23$ (for the base model). Examination of the parameter estimates indicated no significant pairwise differences (all $ps > .57$).

Thus, as with confidence in correct identifications, the tattoo treatment conditions had no significant effect on confidence in innocent suspect identifications.

Discussion

We replicated Colloff et al.'s (2016) finding that inserting a tattoo on all lineup members to create an unbiased lineup (i.e., tattoo replication) reduces innocent suspect identifications (cf. a biased presentation); we further demonstrated that editing a tattoo off of a suspect does not reduce correct identifications but does reduce innocent suspect identifications. Contrary to Colloff et al., we did not find that blocking decreased innocent suspect identifications or that the presence of a tattoo on only a guilty suspect facilitated correct identifications compared to replicating, concealing, or removing it for all lineup members. One might assume that this lack of a significant difference in correct identifications reflects differences in sample size as Colloff et al. tested approximately 1000 participants per condition. However, the Bayes Factor for a null effect in a model where the predictor reflects that the lineup is either biased tattoo or unbiased (unbiased replication, unbiased blocking, and unbiased removal) indicated that we have positive or strong evidence for a null effect ($BF = 17.01$). Thus, our null effect is not explained by data insensitivity.

Another possibility is that the effect is relatively small and, with their high-powered study, Colloff et al. were able to detect this small effect. They found almost double the rate of correct identifications when participants viewed a biased target-present lineup without any treatment of the target's distinctive feature compared to the three unbiased conditions; the reported proportions indicate a medium effect (Cohen's $h = .53 - .56$). Thus, the difference between our findings and theirs is not explained by the size of the effect. Another difference between their study and ours is that they used an online study, whereas we conducted our

study in the lab. It may simply be that participants online were more likely to identify an “oddball” (i.e., someone who had a feature that set them apart) than participants in the lab who had one-on-one interaction with experimenters. Finally, Colloff et al. used a single-trial paradigm whereas we used a multi-trial paradigm, although we did not find any learning effects on our correct identification rates. Perhaps most likely, it is the quality of the participants’ memory that matters. Our participants made considerably more correct identifications than Colloff et al.’s participants, regardless of condition (.79 in our experiment, .28–.31 in Colloff et al.). Perhaps participants are more reliant on cues that make the target stand out when they have a weak memory for the target. Such a view is consistent with the applied lineup theory of eyewitness decision making (Charman & Wells, 2007). It is easy to see why our participants were likely to have had a much better memory than Colloff et al.’s: Their participants engaged in other tasks for eight minutes before making an identification whereas our participants made their identification immediately after viewing the mock crime. Taken together, these findings suggest that biased lineups may facilitate correct identifications when memory quality is poor but not when it is good, whereas biased lineups may induce innocent suspect identifications regardless of memory quality.

Our results are inconsistent with the DFD model (Wixted & Mickes, 2014) explanation offered by Colloff et al. (2016) because we did not find that the tattoo was a diagnostic feature when presented in a target-present lineup. In fact, our target-present lineup results are consistent only with the outshining hypothesis (Smith, 1988), which states that when memory is good, providing a contextual cue will not improve memory because of the strong cueing effect of the to-be-remembered item itself (i.e., the face outshines all other cues).

In contrast, our target-absent lineup results are most consistent with filler siphoning (Wells et al., 2015) and the DFD model (Wixted & Mickes, 2014). That is, we found that two of our three unbiased lineup conditions resulted in fewer innocent suspect identifications than

the biased lineup conditions. The lack of difference between the unbiased blocking lineups and the biased lineups is inconsistent with all four theories we considered.

None of the theoretical approaches we considered were able to account for both the target-present and the target-absent results. This indicates that none fully capture eyewitness identification decision-making for tattooed targets. More research is needed to examine the mechanisms by which modifying a distinctive feature influences correct identifications and how memory quality might affect these decisions.

We predicted that distinctive features would influence the confidence with which participants made identification decisions. However, contrary to our predictions we found no evidence that this was the case and, in fact, the evidence for null effects on confidence was positive/strong for correct identification and positive/substantial for innocent suspect identifications. This finding is consistent with views that eyewitnesses calibrate their confidence, despite factors that influence the accuracy of their decisions (e.g., Semmler, Dunn, Mickes, et al., 2018), but contrasts with other research (e.g. Leippe & Eisenstaedt, 2009; Sauer, Palmer, & Brewer, 2019; Smith & Vela, 1992).

It is worth noting that we found weak practice effects on confidence for both correct and innocent suspect identifications, contrary to the lack of effects reported by Mansour et al. (2017). As such, these data suggest that using multiple trials to examine eyewitness identification confidence in this context (i.e., when perpetrators have distinctive features) must be done with caution. Looking at average confidence across trials, we generally see a flat line for correct and innocent suspect identifications. Scatterplots of the confidence ratings for each participant by trial indicate that for correct identifications, the range of confidence values used increased whereas for innocent suspect identifications, the range decreased as trial number increased. Further investigation should examine the factors that influence eyewitness confidence, particularly how system variables, such as the different ways of

constructing lineups for suspects with distinctive features, influence eyewitness confidence and perhaps interact with memory quality.

Experiment 2

In Experiment 1, we found that biased (cf. unbiased) simultaneous lineups increase innocent suspect identifications but not correct identifications. With one exception, we found no differences in correct identification rates between the different biased treatments or between the different unbiased treatments. We also found that tattoo treatment was unrelated to confidence in suspect identifications. With Experiment 2, we consider whether these findings generalize to sequential lineups. Given our Experiment 1 findings and our available resources, we simplified our tattoo treatment conditions by retaining only the biased tattoo and the unbiased removal conditions.

There is reason to think that a sequential lineup (cf. simultaneous) will offer more protection for an innocent suspect in a biased lineup. Lindsay et al. (1991) found that innocent suspect identifications were significantly lower when the lineup was presented sequentially compared to simultaneously for both biased and unbiased lineups. Sequential lineups not only significantly lowered the rate of innocent suspect identifications when the lineup was biased with one factor (e.g., clothing), but also significantly lowered innocent suspect identifications when lineup instructions, filler similarity, and clothing were combined to construct an outrageously biased lineup. Thus, we considered whether sequential presentation would mitigate the effect of a tattoo bias.

This is an important line of enquiry not only because sequential procedures are carried out worldwide but also because some police officers are reluctant to cover tattoos on suspects (Harrison & Pike, 2015). In the UK, a form of the sequential lineup is legally mandated. Empirical evidence that treatment of a tattoo is or is not beneficial with sequential lineup presentation will help ensure policies are properly evidence-based.

Given these considerations, we tested three hypotheses relevant to lineup decision accuracy in Experiment 2. First, we predicted that we would replicate the typical findings of more correct and innocent suspect identifications with simultaneous than sequential lineups (Stebly, Dysart, & Wells, 2011). Second, we predicted that the effect of tattoo treatment would depend on lineup type. For simultaneous lineups, we predicted that we would replicate our previous finding that removal of a tattoo (cf. no treatment of it) would reduce innocent suspect identifications without influencing correct identifications. Based on Lindsay et al. (1991), we predicted that the sequential lineup would mitigate the effect of lineup bias, such that we expected no difference in the rate of innocent suspect identifications when the tattoo was removed (cf. no treatment of it).

We also examined confidence to see if we could replicate our unexpected Experiment 1 finding that tattoo treatment did not influence confidence ratings. Furthermore, because we used sequential and simultaneous lineups in the current experiment, we were able to test whether the Dobolyi and Dodson's (2013) finding of a sequential overconfidence effect replicates. Dobolyi and Dodson found higher confidence in innocent suspect identifications when participants viewed sequential compared to simultaneous lineups. They reasoned that because sequential lineups reduce willingness to choose compared to simultaneous lineups, any identifications made from sequential lineups would be made with greater confidence.

Method

Participants

Staff and students at a UK university and members of the public ($N = 90$) participated in this experiment, either on a voluntary basis or for course credit (students). The sample comprised mostly females (.77), and participants ranged in age from 18 to 74 years ($M = 25.18$, $SD = 11.38$). The majority of participants reported being White (.90).

Design

We used a 2 (Lineup Type: simultaneous, sequential) x 2 (Tattoo Treatment: biased tattoo, unbiased removal) x 2 (Target Presence: present, absent) mixed factorial design.

Lineup Type and Tattoo Treatment were manipulated between subjects and participants were randomly assigned to these conditions. We used the same eight mock crimes as in Experiment 1, with Target Presence again manipulated within subjects. We used the same counterbalancing for Target Presence and mock crime as in Experiment 1.

Materials

The materials and apparatus used in Experiment 1 were also used in Experiment 2, with the exception that we modified the simultaneous lineups to create sequential lineups and added an intervening task (as described below).

Lineups. In the biased tattoo condition, the suspect was the only lineup member with the tattoo seen on the perpetrator (see Figure 1A). In the unbiased removal condition, the suspect was shown without the tattoo seen on the perpetrator (see Figure 1E). None of the other lineup members had a tattoo. The same individual images of each lineup member from the simultaneous lineups were used for the sequential lineups and the order of lineup member presentation in the sequential lineup matched the position of the particular lineup member in the simultaneous lineup (see Figure 1F).

The lineup instructions for the simultaneous lineups were nearly identical to Experiment 1. They differed in that the “don’t know” option was not used as providing this option in the sequential lineup would significantly complicate analyses. To maintain parity between conditions, we did not include this response option for either lineup type.

For sequential lineups, participants were informed that they could select either “Yes” or “No” in response to whether they thought that the photograph displayed was of the person from the video. Participants in the sequential lineup condition could say “Yes” to more than one lineup member. If more than one selection was made from a sequential lineup and one of

those identifications was of the suspect, then the response for that trial was treated as a suspect identification. Participants in the sequential lineup condition were not informed of the number of pictures in the lineup, although this might have become obvious over the course of the experiment.

Intervening Task. In order to increase ecological validity relative to Experiment 1, after watching each mock crime video and before viewing the lineup, participants completed a Where's Waldo.⁴ visual search task. Participants viewed one of three Where's Waldo images after watching each mock crime and were instructed to answer as many of the questions relating to that image on the sheet provided (e.g., "How many dogs can you see in this picture?"). Each image was displayed for 60 seconds.

Procedure

The procedure was the same as for Experiment 1, except for the changes due to using a sequential lineup and the incorporation of the intervening task between viewing the mock-crime video and the lineup decision. After completing the eight trials, participants were debriefed and thanked.

Measures

We used the same measures for Experiment 2 as in Experiment 1; however, we did not construct the composite indices of tattoo visibility and tattoo bias because the current experiment used only two tattoo conditions, which varied in both visibility and bias.

Results

We followed the approach of Experiment 1 because the eight trials of interest (i.e., lineup decisions and confidence) were nested within participants.

Correct Identifications

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Table 4 reports the relevant proportions. A two-level null model was a significantly better fit to the data than a one-level null model; therefore, we used the former as our basis for comparison, $\chi^2(2) = 21.34$, $p < .001$, $BF = 119.67$. We next entered trial number as a predictor but this model was not a significantly better fit to the data than the two-level null model, $\chi^2(1) = 0.88$, $p = .35$, $f^2 = .003$, $BF = 12.25$ (for the null). The model with tattoo treatment alone was a significantly better fit to the data than the null two-level model, $\chi^2(1) = 13.67$, $p < .001$, $f^2 = .05$, $BF = 48.95$. Removing the tattoo from the suspect reduced correct identifications compared to when the tattoo was not changed. A model with lineup type as the only predictor did not fit the data significantly better than the null two-level model, $\chi^2(1) = 3.46$, $p = .063$, $f^2 = .01$, $BF = 0.30$, although the results were in the expected direction: simultaneous lineups resulted in somewhat more correct identifications than sequential lineups. To test for a significant interaction of lineup type with tattoo treatment, we created a model with lineup type, tattoo treatment, and their interaction as predictors. The new model was a significantly better fit to the data than the model with lineup type alone, $\chi^2(2) = 16.44$, $p < .001$, $f^2 = .06$, $BF = 10.31$, and the model with tattoo treatment alone, $\chi^2(2) = 6.23$, $p = .044$, $f^2 = .02$, $BF = 0.06$. Separate contrasts of tattoo treatment for each level of lineup type indicated that, contrary to our predictions, removing the tattoo reduced correct identifications (cf. biasing the lineup with the tattoo) in sequential, $t(94.19) = 3.83$, $p < .001$, but not simultaneous lineups, $t(94.19) = 1.61$, $p = .11$.

Innocent Suspect Identifications

Table 4 reports the proportion of lineup decisions across conditions. A null two-level model fit the data significantly better than a null one-level model, $\chi^2(2) = 21.33$, $p < .001$, $BF = 118.86$. Adding trial number to the null two-level model did not improve fit, $\chi^2(1) = 0.002$, $p = .97$, $f^2 = .00004$, $BF = 18.96$ (for the null). Consistent with our hypothesis, adding tattoo treatment to the model significantly improved model fit relative to the null two-level model,

$\chi^2(1) = 18.10, p < .001, f^2 = .08, BF = 450.05$. Innocent suspect identifications were significantly more likely in the biased than the unbiased condition. Contrary to our hypothesis, the model with lineup type only was not a significantly better fit to the data than the null two-level model, $\chi^2(1) = 0.24, p = .62, f^2 = .001, BF = 0.06$. Finally, we created a model with lineup type, tattoo treatment, and their interaction as predictors in order to test our hypotheses. The overall model did not improve fit relative to the model with tattoo treatment alone, $\chi^2(2) = 0.68, p = .71, f^2 = .003, BF = 256.30$ (for the null). Thus, the identification data suggest that sequential lineups did not mitigate the biasing effect of a tattoo on innocent suspect identifications and actually harmed correct identifications relative to a simultaneous lineup.

Confidence in Correct Identifications

For correct identifications, a null two-level model fit the data significantly better than a null one-level model, $\chi^2(2) = 99547.38, p < .001, BF = 1.55$. Adding trial number to the null two-level model did not improve fit, $\chi^2(1) = 0.06, p = .81, f^2 = .0001, BF = 16.88$ (for the null). We hypothesized that mean confidence in correct identifications would not significantly differ between sequential and simultaneous lineups. Consistent with this hypothesis, the two-level model with lineup type did not fit the data better than the null two-level model, $\chi^2(1) = 0.13, p = .72, f^2 = .001, BF = 16.27$ (for the null). The model with tattoo treatment as a predictor was not a better fit to the data than the null two-level model, $\chi^2(1) = 3.44, p = .064, f^2 = .02, BF = 0.32$. This finding replicates the results of Experiment 1 with weak evidence, but is inconsistent with our original hypothesis about the effect of tattoo treatment on confidence in correct identifications. Finally, the model with lineup type, tattoo treatment, and their interaction, $\chi^2(3) = 4.28, p = .23, f^2 = .02, BF = 0.002$, was not better a fit to the data than the null two-level model. Supplementary Table 3 provides additional descriptive information.

Confidence in Innocent Suspect Identifications

For confidence in innocent suspect identifications, the null two-level model fit the data significantly better than the null one-level model, $\chi^2(2) = 33082.33$, $p < .001$, $BF = 3530.61$. Adding trial number to the null two-level model did not improve fit, $\chi^2(1) = 2.36$, $p = .12$, $f^2 = .03$, $BF = 2.66$ (for the null). As hypothesized, eyewitnesses' confidence in innocent suspect identifications was significantly higher for sequential ($M = 82.60$ [74.80, 90.39]) than simultaneous lineups ($M = 68.89$ [60.50, 77.28]), $\chi^2(1) = 5.47$, $p = .019$, $f^2 = .11$, $BF = 1.78$. Unlike Experiment 1, but consistent with our original hypothesis, tattoo treatment was a significant predictor of confidence in innocent suspect identifications, $\chi^2(1) = 14.39$, $p < .001$, $f^2 = .33$, $BF = 154.00$. Specifically, eyewitnesses expressed significantly higher confidence when they made innocent suspect identifications from the biased tattoo lineups ($M = 83.06$ [76.95, 89.18]) than the unbiased removal lineups ($M = 59.81$ [50.32, 69.31]). However the model with tattoo treatment, lineup type, and their interaction was a significantly better fit to the data than either the model with tattoo treatment alone, $\chi^2(2) = 9.35$, $p = .009$, $f^2 = .18$, $BF = 1.43$, or the model with lineup type alone, $\chi^2(2) = 18.27$, $p < .001$, $f^2 = .41$, $BF = 123.58$. As Figure 2 illustrates (see also Supplementary Table 3), the effect of lineup type on confidence was greater for unbiased than biased lineups, although a simple effects analysis indicated the effect of lineup type was significant for both tattoo treatment conditions ($ps \leq .046$).

Discussion

The interaction of tattoo treatment and lineup type was the main consideration in this experiment. Many police departments worldwide use sequential lineup presentation, but no data currently exist regarding whether the various methods of dealing with distinctive marks are effective with this procedure, despite their use in practice. There is some evidence that the criminal justice system recognizes the potential value of distinctive marks as memory cues

and are keen for them to be used as such (Harrison & Pike, 2015). If sequential lineups mitigated the negative biasing effect of tattoos, then they should be explored as a means to increase correct identifications. Indeed, prior research suggested that perhaps the impact of a tattoo on innocent suspect identifications could be attenuated in a sequential lineup compared to a simultaneous lineup (Lindsay et al., 1991). Our results, however, do not support that position. Instead, biased lineups led to similarly high rates of innocent suspect identifications for simultaneous and sequential lineups. Thus, contrary to our expectations, our results suggest that sequential lineups should not be used if the only lineup member with a tattoo is the suspect. Further research should examine whether other treatments (e.g., replication and blocking) also interact with lineup type or produce more accurate decisions.

Compared to the biased tattoo condition, we found that removing the tattoo (i.e., unbiased treatment) significantly reduced both correct and innocent suspect identifications from sequential lineups, but reduced only innocent suspect identifications from simultaneous lineups. Thus, our findings indicate that tattoo removal is more effective with simultaneous than sequential lineups. However, more research is needed because appropriate treatment of a suspect's tattoo may depend on whether the witness viewed and reported seeing a tattoo on the perpetrator. In the case described earlier, Tyrone Lamont Allen was shown in a lineup with his tattoos edited off because the CCTV footage of the perpetrator of the crime he was suspected of did not appear to have tattoos (Bernstein, 2019a, 2019b). Although editing the tattoos off of Allen's image ensured he did not stand out in the lineup, this action also made him look more like the perpetrator. The initial representation of the perpetrator to the police (via CCTV or witness description) may interact with tattoo treatment. In addition to investigating how eyewitness awareness of a perpetrator's tattoo interacts with tattoo treatment, future research should explore whether other tattoo treatments (e.g., replication) are more effective with sequential lineups.

The results for our simultaneous lineups are consistent with Colloff et al.'s (2016) finding that biased (cf. unbiased) lineups increase correct identifications and different from our findings in Experiment 1. The sequential lineup results are inconsistent with Lindsay et al. (1991), which found that correct identifications were not significantly influenced by bias. At first glance, the correct identification results for sequential lineups may appear consistent with the outshining hypothesis (Smith, 1988), but they are actually inconsistent. Prior research suggests that a contextual cue will improve hits (correct identifications) only when memory quality is somewhat poor (Smith et al., 2018). However, memory quality was quite good in the current experiment as the overall correct identification rate was high—similar to Experiment 1 and considerably higher than in Colloff et al. (2016). Our simultaneous unbiased lineups elicited .83 correct identifications compared to Colloff et al.'s rate of .31. In our study, participants completed multiple trials, with the perpetrator having a tattoo in all trials, so it is possible that the experimental design helped direct participants' attention to the tattoo. However, this explanation is not supported by the data as we found no effect of trial number on correct identifications.

Our finding that the biased tattoo condition produced more correct identifications than the unbiased removal condition in sequential lineups is consistent with filler siphoning (Wells et al., 2015). Consider Table 4. In simultaneous lineups, the overall choosing rate (i.e., identifications of suspects and fillers) from target-present lineups is nearly identical in the unbiased ($.83 + .12 = .95$) and biased ($.92 + .01 = .93$) conditions, with the latter resulting in .11 identifications shifted to the suspect rather than fillers. The effect is also present with the sequential lineups, although overall choosing is lower in the unbiased ($.68 + .13 = .82$) than the biased condition ($.91 + .03 = .94$); nonetheless, the biased lineup still resulted in fewer filler identifications than the unbiased lineup.

We expected to find the typical pattern of more guilty suspect and innocent suspect identifications for simultaneous compared to sequential lineups (Stebay et al., 2011). Our correct identification results supported our prediction; however, we found no effect of lineup type on innocent suspect identifications. This unexpected lack of difference in innocent suspect identifications from the two lineup types may also mask a difference in how tattoo treatment affects innocent suspect identifications in sequential lineups. As such, more research is needed to further understand how lineup type affects innocent suspect identifications when targets have distinctive features.

We also predicted that lineup type would affect confidence, particularly for innocent suspect identifications. Specifically, we hypothesised that eyewitnesses would report higher confidence ratings in innocent suspect identifications made from sequential lineups than simultaneous lineups. Our current results support this prediction, though weakly/anecdotally ($BF = 1.78$). These results are consistent with Dobolyi and Dodson (2013), who suggested that sequential lineups require greater memorial information than simultaneous lineups, which therefore decreases the likelihood that the eyewitness will make a selection from the sequential lineup. As a result, when eyewitnesses do make selections from sequential lineups, they do so with higher confidence than eyewitnesses who made selections from simultaneous lineups. In terms of confidence in correct identifications, we hypothesised that confidence would not significantly vary by lineup type. Our data provides positive/strong support for this prediction ($BF = 16.27$), which is also consistent with Dobolyi and Dodson (2013) who suggest that the presence of the perpetrator exceeds the different criteria used to make identifications from the two lineups.

In terms of our tattoo manipulation, we originally predicted that confidence would be higher for suspect identifications from biased than unbiased lineups. The results of Experiment 1 did not support this hypothesis, but the results of Experiment 2 do. The cue-

belief model suggests that the presence of a tattoo makes the suspect stand out, which increases the probability that the eyewitness will use this “standing out” to both select the suspect and as a cue that they have made a correct response, thereby inflating their confidence. One plausible explanation for the lack of an effect in Experiment 1 but an effect in Experiment 2 is that in Experiment 2, we incorporated a delay between witnessing and the identification task. It may be that, in the absence of delay between the event and the memorial task, no confidence effects emerged because participants were relying on a very strong memory trace in all conditions. However, further research is needed. It is important to consider that we found conflicting results across the two experiments and that when we take a Bayesian perspective, the evidence for this effect on confidence in Experiment 2 was so weak ($BF = 0.32$) that it is better interpreted as positive/substantial evidence for no effect ($BF = 3.10$ for no effect).

Experiment 3

Experiments 1 and 2 examined how the treatment of a tattoo within lineups can affect eyewitness identification accuracy and confidence. In particular, we were interested in how police could prevent an innocent suspect with distinctive features (e.g., tattoo) from being identified based on their presence in a lineup. Although our results provide insight into how eyewitnesses respond to an innocent suspect with a tattoo in the same location and with the same design as a mock-perpetrator, they tell us little about how eyewitnesses will behave when shown an innocent suspect with a tattoo in a location and with a design that differs from the perpetrator's. This question has real-world relevance because it is much more likely that an innocent suspect will have a similar tattoo to that of the perpetrator than one in the exact same location with the exact same design. Indeed, an innocent suspect might end up in a lineup precisely because they have a tattoo similar to one described by an eyewitness. To investigate this issue, Experiment 3 examined whether eyewitness identifications of innocent

suspects from biased lineups depend upon the similarity of the location and design of the suspects' tattoos. Specifically, we used four target-absent conditions to examine the impact of distinctive feature location (same vs. different) and design (same vs. different) on innocent suspect identifications from biased simultaneous lineups.

We expected that the innocent suspect identification rate would be higher when the innocent suspect's tattoo location and design were the same (cf. different) as that seen on the perpetrator. To the extent that the tattoo is a cue to prior exposure, we predicted that lower similarity between the cue and the presented face would lower the probability of identification. All four theories we have considered generally support this prediction, with the HS-GCM providing an explicit mechanism for it (i.e., the summed similarity rule; Nosofsky & Zaki, 2003). We also expected that the degree of match for the tattoo would be more influenced by the tattoo's location than the design because of the salience of a change in position. Thus, we predicted that innocent suspect identifications would be lower in the different location condition than the different design condition.

Beyond the four target-absent conditions that varied in terms of tattoo location and design, we also included target-present and target-absent control conditions. For the target-present lineup, we used the biased tattoo lineup from Experiments 1 and 2; we included this lineup to ascertain how this sample was responding, as compared to the previous samples. For the target-absent control, we used the unbiased removal lineup from Experiments 1 and 2, which allowed us to assess how the mere presence of a tattoo influences eyewitness choosing rates from target-absent lineups. It is possible that the mere presence of a tattoo—regardless of the match to the original tattoo seen on the target—could be enough to elicit a selection, for at least some witnesses. This condition also allowed us to further examine whether the effect of tattoo removal on innocent suspect identifications replicates.

Method

Participants

The participants ($N = 287$; .50 females) ranged in age from 17–30 years ($M = 19.88$, $SD = 2.03$) and identified primarily as Asian (.52) or European (.27).

Design

The current experiment was a 2 (Tattoo Location: same, different) x 2 (Tattoo Design: same, different) + 1 (Target-present control) + 1 (Target-absent control) between-subjects design. Tattoo Location refers to the degree of similarity between the perpetrator's and innocent suspect's tattoo location. Tattoo Design refers to the degree of similarity between the perpetrator's and innocent suspect's tattoo design. All lineup fillers appeared without tattoos (see Figure 1A). The target-present control was the only situation with a guilty suspect, and this individual was presented with a tattoo, whereas the target-absent control condition showed the innocent suspect without a tattoo. Participants were randomly assigned to conditions. We aimed to collect 30 participants per cell but discovered a coding error after completing data collection which led to under-recruitment in some conditions. We collected additional data to ensure that we had at least 30 participants per condition. However, as Table 5 illustrates, this resulted in unbalanced cell sizes.

Materials

We used the same eight mock-crime videos used in Experiments 1 and 2. For each target, we used three of the simultaneous lineups from Experiments 1 and 2, and we created three new simultaneous lineups. Specifically, our target-present control lineup was our previous target-present biased tattoo lineup (see Figure 1A). Our target-absent control lineup was the previous target-absent unbiased removal lineup (see Figure 1E). We also used the prior target-absent version of the biased tattoo lineup (see Figure 1A) for our same location + same design condition.

We constructed three new lineups to test our hypotheses about the role of similarity of tattoo location and tattoo design. We attempted to make our *different* conditions obviously different by significantly varying the location and design of the tattoo worn by the suspect. Thus, we constructed the following additional target-absent biased tattoo lineups (see Figure 3): same location + different design, different location + same design, and different location + different design. As Figure 3 illustrates, the *different* levels for the location and design are obviously different than the *same* levels. As in Experiment 1, participants were able to respond “don’t know” to the lineup.

In this experiment, we employed a different intervening task than in Experiment 2. The intervening task was one of two unrelated experimental tasks that took approximately 30 minutes. The tasks involved either detecting human targets on screen or evaluating faces followed by a surprise old/new recognition task. Both tasks involved faces to mimic the types of interactions witnesses may engage in after witnessing a crime.

Procedure

Participants provided demographic information, watched one mock-crime video, and then were informed that they had become witnesses to a mock crime. They then engaged in the intervening task for approximately 30 minutes, made one lineup decision, and indicated their confidence in that lineup decision. Next, participants were asked whether they knew they would have to identify someone from a lineup prior to visiting the lab and, if they responded “yes”, were asked to provide more information. Finally, participants were debriefed and thanked.

Measures

As in previous experiments, we coded lineup decisions as suspect identifications, filler identifications, or rejections. We then coded whether participants made a suspect

identification or other decision. We also calculated mean confidence in correct identifications and innocent suspect identifications for each condition.

Results

Participants made only one identification decision, so mixed effects modelling was unnecessary. Instead, we conducted logistic regressions or analyses of variance (ANOVAs) using the stats package in R (R Core Team, 2019) using the fmsb package in R to calculate Nagelkere R^2 (Nakazawa, 2019). As is typical, we report odds ratios (OR) as effect sizes for our logistic regressions and Cohen's d or partial eta squared (η_p^2) as effect sizes for our ANOVAs. Cohen's d was computed using the compute.es package in R (Del Re, 2020) and partial eta squared was computed using the effectsize package in R (Ben-Shachar, Makowski, Lüdtke, et al., 2020). To facilitate comparisons with the previous experiments, we note that OR can be converted to Cohen's d using the formula: $\text{LogOR} \frac{\sqrt{3}}{\pi}$ (Borenstein, Hedges, Higgins, et al., 2009, Chapter 7). Thus, small, medium, and large effect sizes using Cohen's d (i.e., 0.2, 0.5, and 0.8) correspond to ORs of approximately ≥ 1.44 , ≥ 2.48 , and ≥ 4.27 , respectively. As in the previous experiments, we also estimated Bayes Factors for our model comparisons.

A small proportion (.07) of participants indicated they were aware that they would be doing an eyewitness identification study in advance. Excluding these participants did not change the nature of the results with two exceptions (which we note), therefore we included all participants in the reported analyses.

The target-present control condition indicated that the current participants performed similarly to those in previous experiments. Specifically, despite the differences in intervening task procedures, number of trials, and awareness that the study was about eyewitness memory at the time of encoding, the biased target-present lineup produced similar correct

identification rates in Experiment 1 (.89), Experiment 2 (.88), and Experiment 3 (.84). Table 5 displays the full response frequencies for all lineup conditions in Experiment 3.

Innocent Suspect Identifications

We conducted a logistic regression to examine the effects of tattoo location and tattoo design on innocent suspect identifications. Thus, we excluded the target-absent and the target-present control conditions from this analysis. The full model was significant, $\chi^2(3) = 10.85$, $p = .013$, $R_{\text{Nagelkerke}}^2 = .10$, $\text{BF} = 0.08$. The rate of innocent suspect identifications did not significantly differ when the tattoo was in the same location (.13) compared to a different location (.07) from the perpetrator, $\chi^2(1) = 2.31$, $p = .13$, $\text{OR} = 5.20$. However, we found a main effect of tattoo design such that participants made significantly more innocent suspect identifications when the tattoo's design was the same (.19) compared to different (.06), $\chi^2(1) = 4.36$, $p = .037$, $\text{OR} = 10.40$. The interaction was not significant, $\chi^2(1) = 1.31$, $p = .25$, $\text{OR} = 0.24$ ⁵. Contrary to our hypothesis, tattoo design influenced innocent suspect identifications more than tattoo location. Given the very weak evidence for the full model, we ran a model with tattoo design alone; the evidence for this model was weak/anecdotal, $\chi^2(1) = 7.45$, $p = .006$, $\text{OR} = 3.50$, $\text{BF} = 2.89$.

We predicted that participants would make fewer innocent suspect identifications the more the innocent suspect's tattoo differed from the tattoo seen on the perpetrator. To evaluate this hypothesis, we created a continuous variable (we again excluded the control lineup conditions in this analysis). Given our finding that tattoo design had more influence than tattoo location, we operationalized lineup condition such that we coded the different location + different design condition as very dissimilar, the same location + different design

⁵ When participants who were aware the study involved identifications were excluded, this and the following model fail to converge because no participants in the different design + different location condition identified the suspect. However, the patterns of proportions are nearly identical to the patterns found using the full sample.

condition as somewhat dissimilar, the different location + same design condition somewhat similar, and the same location + same design condition as very similar. The model was significant, $\chi^2(1) = 9.32$, $p = .002$, $R_{\text{Nagelkerke}}^2 = .09$, $\text{BF} = 7.37$, with each increase in similarity increasing the likelihood of a suspect identification 1.89 times. These results are consistent with our hypothesis that increased similarity between the suspect's tattoo and the perpetrator's tattoo increases identifications.

We also predicted that the mere presence of a tattoo would increase innocent suspect identifications. To test this hypothesis, we constructed a model with only the same location + same design condition and the target-absent control condition. Contrary to our expectations, however, we found no effect of tattoo presence, $\chi^2(1) = 0.67$, $p = .41$, $R_{\text{Nagelkerke}}^2 = .01$, $\text{BF} = 0.16$. Thus, the mere presence of a tattoo was not sufficient to elicit significantly more innocent suspect identifications than if the suspect was presented without a tattoo.

Confidence in Innocent Suspect Identifications

We considered confidence next; however, given the low rates of innocent suspect identifications in some conditions, these results should be interpreted with caution.

We entered tattoo location, tattoo design, and their interaction as predictors of the confidence with which eyewitnesses made innocent suspect identifications into an ANOVA. We excluded both control conditions (biased target-present and unbiased target-absent lineups) from this analysis. Neither the main effect of tattoo location, $F(1, 18) = 0.54$, $p = .47$, $d = 0.32$, $\text{BF} = 0.53$, nor the main effect of tattoo design, $F(1, 18) = 0.50$, $p = .49$, $d = 0.34$, $\text{BF} = 0.55$, nor their interaction had significant effects on confidence, $F(1, 18) = 0.10$, $p = .75$, $\eta_p^2 < .001$, $\text{BF} = 0.21$.

In order to determine whether removal of the tattoo from the innocent suspect affected confidence in innocent suspect identifications, we again created a binary predictor with unbiased removal (i.e., the target-absent control condition) in one group and the four biased

target-absent lineups in the other. Although the pattern of means was in the expected direction, an independent samples t -test indicated no significant difference in confidence when the tattoo was removed ($M = 49.50$ [35.15, 63.85]) compared to visible ($M = 65.18$ [52.34, 78.02]), $t(24) = 0.99$, $p = .33$, $d = 0.54$, $BF = 0.33$. However, a t -test comparing only the target-absent control condition and the same location + same design conditions resulted in a significant difference, $t(11) = 2.44$, $p = .033$, $d = 1.47$, $BF = 4.61$.⁶ Eyewitnesses who identified an innocent suspect without a tattoo expressed lower confidence than those who had identified an innocent suspect with a tattoo identical to that of the perpetrator ($M = 77.11$ [63.92, 90.93]).

Confidence in Accurate Versus Inaccurate Suspect Identifications

Confidence is predictive of accuracy such that high confidence tends to be associated with high accuracy and low confidence with low accuracy when conditions are ideal (Wixted & Wells, 2017). It would be useful to know whether a mismatch between the perpetrator and an innocent suspect's tattoo leads to lower identification confidence, as compared to confidence in a correct identification. To explore this, we first compared confidence in correct identifications from the target-present control condition to confidence in innocent suspect identifications from three biased lineup conditions (excluding the same location + same design condition because this condition involves a matching tattoo). Indeed, participants were more confident in correct identifications ($M = 92.40$ [88.55, 96.24]) than innocent suspect identifications ($M = 56.92$ [38.09, 75.75]), $t(13.02) = 3.62$, $p = .003$ (equal variances not assumed), $d = 1.14$, $BF = 56250.80$. Even when the analysis included identifications from the same location + same design condition ($M = 65.18$ [52.34, 78.02]), we found a significant effect, $t(24.84) = 3.98$, $p < .001$ (equal variances not assumed), $d =$

⁶ This result is nonsignificant ($p = .061$, $d = 1.41$, $BF = 2.63$) when we exclude participants who indicated they were aware they were participating in an identification experiment.

1.04, $BF = 7353.18$. The effect persisted when confidence in same location + same design innocent suspect identifications ($M = 77.11$ [63.92, 90.30]) was compared to confidence in correct identifications, $t(50) = 2.92$, $p = .005$, $d = 1.07$, $BF = 8.24$.

Discussion

This experiment provides evidence for a boundary condition for the biasing effect of distinctive marks. Participants rarely identified an innocent suspect whose tattoo was in a different location and had a different design than the perpetrator. We found that the greater the difference in an innocent suspect's tattoo compared to the perpetrator's tattoo, the less likely they were to be identified. A difference in tattoo design had a greater influence on innocent suspect identifications than a difference in tattoo location. Unexpectedly, and contrary to Experiment 1, the mere presence of a tattoo did not lead to more innocent suspect identifications than when they were presented without a tattoo. However, we had considerably less power to detect this difference in the current experiment and innocent suspect identifications were quite rare in both experiments.

Taken together, our results suggest that a mismatching cue reduces eyewitnesses' willingness to identify an innocent suspect. Thus, an innocent suspect seems to be at risk unless they have a feature that causes them to stand out as different from the perpetrator. An obviously different tattoo, particularly in terms of the tattoo design, protects an innocent suspect by reducing the likelihood of identification. Looking back to the case of Tyrone Lamont Allen, this result suggests that presenting him in the lineup with his tattoos may have provided protection for him from identification if he is innocent, even though they would have made him stand out from other lineup members.

In contrast, confidence in innocent suspect identifications was unaffected by the degree of match between the tattoo seen on the perpetrator and the innocent suspect. Thus, confidence does not provide an indicator of the degree of match between an innocent suspect

and the eyewitness' memory. However, we did find that confidence was lower if an innocent suspect was presented without a tattoo as compared to being presented with a tattoo in the same location with the same design as the perpetrator. This finding indicates that eyewitnesses who identified the innocent suspect might be using the tattoo as a memory cue and that, in its absence, they have lower confidence in that identification. However, given the low rate of innocent suspect identifications in the current experiment, these results should be interpreted with caution. More importantly, though, confidence in correct identifications was higher than confidence in innocent suspect identifications, which suggests that confidence is useful for differentiating accurate and inaccurate identifications.

Experiment 4

Experiment 3 demonstrated that the mere presence of a tattoo might not be enough to elicit innocent suspect identifications and that having an obviously-different tattoo can actually protect an innocent suspect from misidentification. Yet, a similar tattoo also placed an innocent suspect at greater risk than a different tattoo. Unexpectedly, an identical tattoo on only the innocent suspect did not increase risk compared to a lineup in which no one was presented with a tattoo. In Experiment 4, we sought to reduce the saliency of our similarity manipulations in order to further examine the risk of a similar tattoo on an innocent suspect. In this experiment, the change in location and/or design from the perpetrator's tattoo was much less obvious (e.g., blue butterfly rather than blue bird; same side of the neck but higher). That is, we aimed to make the perpetrator's and innocent suspect's tattoos mistakable if relying on memory but obviously different if the two individuals were viewed side-by-side. We expected to find that differences in tattoos between the perpetrator and innocent suspect would not longer protect the innocent suspect from identification.

Additionally, in Experiment 4 we aimed to learn more about the relationship between witnessing a tattooed perpetrator and viewing a lineup with a tattooed suspect by collecting

perpetrator descriptions and examining the ability of eyewitnesses to identify the tattoo worn by the perpetrator. The importance of this issue is demonstrated in a currently-unpublished study by Colloff (2016). Colloff had participants watch a 30-second video of a perpetrator with or without a distinctive mark (tattoo or bruise) and asked them to describe the perpetrator immediately after the video. Participants then viewed a lineup wherein the suspect did or did not have the same distinctive mark. The vast majority of participants who saw a distinctively-marked perpetrator provided some description of the mark (.87), but only .03 of describers did so in detail. Less than half of the describers (.42) provided information about the location of the tattoo, and only .10 provided both location and detail information. In the biased lineups, correct identifications were unaffected by whether or not the participant described the distinctive mark, but were more likely than if the perpetrator had no distinctive mark. In contrast, innocent suspect identifications from these biased lineups were higher if the participant had described the distinctive mark (cf. did not describe it or did not see a distinctively-marked perpetrator). Notably, participants in this study had considerable time to encode the video and were specifically reminded to provide as much detail about the perpetrator's facial appearance as possible. In the current experiment, we consider the extent to which eyewitnesses describe tattoos after a briefer view of the perpetrator.

Our appreciation of the need to consider whether eyewitnesses noticed and could remember the tattoo occurred after data collection began and coincided with the lead researcher moving universities. As such, we refer to the data from Experiment 4 as being collected in two waves (Experiment 4a and 4b), and we included sought a description of the perpetrator and showed participants a tattoo lineup in Experiment 4b only. We report the face identification and confidence results from both waves together because their procedures were similar and they both contained the same manipulated variables.

Method

Design

Experiment 4 used a nearly identical design to Experiment 3: 2 (Tattoo Location: same, similar) x 2 (Tattoo Design: same, similar) + 1 (Target-present control) between-subjects design. We again included the target-present control condition in order to compare eyewitness performance with the prior experiments, but omitted the target-absent control condition as we were not assessing the effect of a tattoo versus no tattoo on innocent suspect identifications and confidence. As in previous experiments, we used eight mock-crime videos, and participants were randomly assigned to conditions and mock-crimes.

Participants

We recruited participants for this study ($N = 251$) in two waves due to changes in the researchers' University affiliations over time. In the first wave (Experiment 4a), undergraduate students and community volunteers ($n = 126$; .45 female) were recruited in 2011/2012 from a Canadian university. Participant ages ranged from 18–63 years ($M = 34.71$, $SD = 11.46$) and the majority (.72) self-reported as White. The second wave (Experiment 4b) was conducted in 2016/2017 and comprised students at a UK university or community volunteers ($n = 125$). This wave comprised mostly females (.69); participants ranged in age from 18–68 years old ($M = 30.30$, $SD = 13.70$) and identified primarily as White (.92).

Materials

Mock Crimes and Face Lineups. The majority of the materials for this experiment were nearly identical to those used in Experiment 3. However, in the current experiment, we aimed to make the *different* tattoos more similar to the perpetrator's tattoo than in Experiment 3. Our operational definition of *similar* was that the innocent suspect's tattoo was noticeably different if the perpetrator and innocent suspect were seen side-by-side, but mistakable if you saw them in succession (see Figure 4). As in Experiment 3, we manipulated the innocent suspect's tattoo such that the tattoo location (same vs. similar) was crossed with the tattoo

design (same vs. similar), resulting in four conditions: same location + same design (biased tattoo target-absent lineup from Experiments 1–3), same location + similar design, similar location + same design, and similar location + similar design. All lineups were biased simultaneous lineups, such that the suspect was the only lineup member with a visible tattoo (see Figure 1A). In addition to the four target-absent conditions, we also included the target-present biased tattoo control condition used in Experiments 1–3 to assess whether the sample was responding similarly to our prior samples.

Tattoo Lineups. In Experiment 4b, participants viewed a tattoo lineup (see Figure 5) as well as a face lineup (seen by participants in Experiments 4a and 4b). Participants in Experiment 4b received the following instructions when presented with the tattoo lineup:

The person (“perpetrator”) from the video MAY or MAY NOT have had a tattoo. Next, you will see an array of tattoo pictures.

-If the tattoo is in the array of pictures, click the number associated with it.

-If the tattoo is NOT in the array of pictures, you should click the button that says “Not There.”

-If you did NOT see a tattoo in the video, you should click the button that says “No Tattoo.”

The tattoo lineups were six-image simultaneous arrays and always contained the tattoo worn by the perpetrator and five similar tattoos (i.e., all tattoo lineups were target-present). For half of the targets, the innocent suspect’s tattoo was one of the five similar tattoos in the lineup.

Intervening Task. In Experiment 4a, participants completed a visual search task for 60 seconds between viewing the mock crime and the lineup. This visual search task involved searching for a specific letter among a series of other letters (e.g., an L among Ts). For Experiment 4b, participants also completed a visual search task, but they completed the

Where's Waldo task (see footnote 4) from Experiment 2. The duration of the delay used was also longer in Experiment 4b than in Experiment 4a to increase ecological validity and increase variability in responding. Participants in Experiment 4b spent 21 minutes on the visual search task.

Procedure

We used a similar procedure in Experiment 4 as in Experiment 3.

Experiment 4a. After providing informed consent, participants provided demographic information, watched the mock-crime video, and then were informed that they had become eyewitnesses to a mock crime. Participants then completed the 60-second intervening task, made a lineup decision, and provided their confidence in their lineup decision. Finally, participants were asked whether they recognized anyone in the study from somewhere other than the experiment, as these participants came from the same pool of people as those recruited for the mock crimes. Finally, participants were debriefed and thanked.

Experiment 4b. Participants in Experiment 4b completed essentially the same measures and experienced the same manipulations as participants in Experiment 4a, but completed additional measures and experienced a longer, different intervening task. After watching the mock-crime video, participants typed a description of the perpetrator before completing the 21-minute intervening task. Next, participants made a decision from the face lineup and rated their confidence in that decision. Participants then made a decision from the tattoo lineup and provided their confidence in that decision. Finally, the participants provided demographic information, were asked whether they were aware in advance that they would be doing an eyewitness identification study, and then debriefed and thanked.

Measures

We used the same measures as Experiment 3, but we collected additional measures in Experiment 4b related to the description of the perpetrator and the eyewitnesses' responses to

the tattoo lineup. We coded participants' descriptions of the perpetrator to determine whether a tattoo or distinguishing mark was mentioned and, if so, whether *tattoo location* or *tattoo design* were mentioned. We applied a relatively liberal coding scheme to the *tattoo design* variable such that if the participant mis-perceived the tattoo as jewellery (e.g., "had a choker necklace") or the nature of the tattoo (e.g., "She had a tattoo of a word on her" when the tattoo was a picture), we coded them as having described a tattoo. We coded the tattoo lineup decisions as a correct identification of the perpetrator's tattoo, identification of a filler tattoo, incorrect rejection of the tattoo lineup, or failing to notice a tattoo on the perpetrator. We also calculated the mean confidence for each decision category.

Results

In Experiment 4a, seven participants (.06) indicated that they recognized the perpetrator from outside the experiment; in Experiment 4b, 25 participants (.20) indicated awareness that the study was about eyewitness identification. All analyses were conducted with and without these participants. However, the results did not change; therefore, we include the full sample in our reported analyses. The rate of correct identifications from the target-present control lineup was similar to previous experiments (.84).

Innocent Suspect Identifications

Table 6 shows the proportions of responses by condition for Experiments 4a and 4b, as well as collapsed across both experiments (overall). Because the data were collected in two waves and the procedures were somewhat different, we explored if there were any differences in performance that needed to be accounted for. That is, if data were nested within data collection wave. Before examining our hypotheses, we tested whether the inclusion of a second-level variable, data collection wave (Experiment 4a, Experiment 4b), improved model fit in the same way as we tested the need for two-level models in Experiments 1 and 2. Only responses for the target-absent lineups were examined. The two-level null model was

singular, indicating that a multilevel approach was inappropriate for these data (i.e., data collection wave accounted for zero or nearly zero variance in identification decisions).

Inspection of the decision rates in Table 6 support this conclusion. Therefore, we proceeded with the one-level model.

In Experiment 3, we hypothesized that the more the innocent suspect's tattoo differed from the perpetrator's tattoo, the less likely eyewitnesses would be to identify the innocent suspect. In the current experiment, we hypothesised that subtle differences would not be protective (i.e., we predicted null effects). To test this prediction, we entered tattoo location, tattoo design, and their interaction as predictors in a logistic regression, $\chi^2(3) = 2.62, p = .45$, $R_{\text{Nagelkerke}}^2 = .02$, $\text{BF} = 769.64$ (for the null model). Inspection of the model indicated no significant effect of tattoo location, $\chi^2(1) = 2.09, p = .15$, $OR = 2.20$, tattoo design, $\chi^2(1) = 0.34, p = .56$, $OR = 1.33$, or their interaction, $\chi^2(1) = 0.36, p = .55$, $OR = 1.59$.

In order to assess the evidence for a null effect of tattoo location, we compared a null model to a model with tattoo location alone as its predictor, $\chi^2(1) = 2.19, p = .14$, $\text{BF} = 4.75$ (for the null). We conducted the same analysis for tattoo design, $\chi^2(1) = 0.08, p = .78$, $\text{BF} = 13.63$ (for the null).

Finally, in Experiment 3, we tested whether a continuous variable reflecting degree of match between the perpetrator's tattoo and the suspect's (very dissimilar, somewhat dissimilar, somewhat similar, very similar) predicted innocent suspect identifications and we found that it did. We conducted the same analysis in the current experiment and, as expected given the reduced salience of the difference in the tattoos of the perpetrator and innocent suspect, the model was not significant, $\chi^2(3) = 2.62, p = .45$, $\text{BF} = 769.64$ (for the null).

Confidence in Innocent Suspect Identifications

We examined whether our manipulations to the innocent suspect's tattoos influenced confidence in innocent suspect identifications. As with our analysis of innocent suspect

identifications, we used a single-level approach because a two-level model with trials nested within data collection wave was singular (i.e., data collection wave accounted for no variance). We entered tattoo location, tattoo design, and their interaction as predictors of confidence in innocent suspect identifications in a univariate ANOVA. Neither the main effect of tattoo location, $F(1, 30) = 0.004, p = .95, d = 0.02, BF = 0.34$, nor the main effect of tattoo design, $F(1, 30) = 0.26, p = .61, d = 0.18, BF = 0.33$, nor their interaction, $F(1, 30) = 2.03, p = .16, \eta_p^2 = .06, BF = 0.09$, were significant. The non-significant effects are unsurprising given the small number of innocent suspect identifications made ($n = 34$).

Confidence in Accurate Versus Inaccurate Suspect Identifications

We next considered whether confidence in suspect identifications differentiated between guilty and innocent suspects. A t -test comparing confidence in correct identifications from the target-present control condition ($M = 75.07 [67.88, 82.27]$) to confidence in innocent suspect identifications from three biased lineup conditions ($M = 63.32 [53.38, 73.26]$; excluding the same location + same design condition, as in Experiment 3) found no significant difference, $t(62) = 1.88, p = .065, d = 0.49, BF = 0.73$.

Description of the Tattoo

Participants in Experiment 4b provided a description of the perpetrator. Of these participants, only about one-third (.34) referred to the tattoo at all. Supplementary Table 4 provides a breakdown of (face) lineup decisions by whether the participants reported a tattoo. Of the participants that referred to the tattoo, most (.86) mentioned the location of the tattoo (e.g., “on her neck”, “round his neck”, “on the left hand side of their neck”). Of those that referred to the tattoo, nearly half (.44) provided information about the tattoo’s design. The types of design information provided included details about the tattoo’s colour (e.g., “black tattoo”), size (e.g., “big tattoo”), and pattern (e.g., “tribal tattoo”, “butterfly tattoo”). Nearly half of participants who referred to the tattoo (.40) provided both location and design

information. Generally, if a participant provided design information, they also provided location information. The two participants (.09) that provided neither location nor design information just mentioned a tattoo was present, with no additional detail. As most participants described what the mock-perpetrator said and/or other aspects of the mock-perpetrator's appearance, the lack of information provided about tattoos is unlikely to be explained by disengagement with the description task.

Tattoo Lineup

Participants performed poorly on the target-present tattoo lineup (Supplementary Table 5). Participants frequently indicated they had not noticed their perpetrator had a tattoo (.12–.81, depending on the target; $M = .45$). The rate of correct tattoo identifications was similar across our manipulations of tattoo location and design in the face lineups (0–.43, $M = .12$). We compared the rate of correct tattoo identifications to chance (.167), and found that the values did not differ significantly, $z = 1.64$, $p = .10$. Confidence ratings for the tattoo lineup decisions (see Supplementary Table 5) were highly variable; we did not analyse the data given the small proportion of participants who made correct tattoo identifications.

Discussion

A lineup is presumed to be biased when there is a tattoo on a suspect and no other lineup member; the results of Colloff et al. (2016) and Experiments 1 and 2 establish that without some sort of treatment to the lineup members and/or the innocent suspect, these biased lineups produce high rates of innocent suspect identifications. However, in the real world, it seems unlikely that an innocent suspect will have a tattoo in the identical location with an identical design as the perpetrator of a crime. As such, it is important to know the extent to which similarity in tattoo location and design also results in lineup bias. We predicted that the more that a perpetrator's and innocent suspect's tattoos differ, the less likely eyewitnesses would be to identify that innocent suspect *when the tattoo was saliently*

different (as in Experiment 3) but not when the tattoo was *mistakable* (as in Experiment 4). Indeed, we found evidence that small differences in tattoo location and small differences in tattoo design were not detected by our participants (the Bayes Factors suggested we had positive/substantial evidence and positive/strong evidence for these conclusions, respectively). Eyewitnesses identified an innocent suspect with an identical distinctive mark as frequently as one that appeared in a slightly different location and had a slightly different design.

However, it is worth noting that we intentionally made the tattoos in the current study only slightly different from the perpetrator's tattoo in terms of location and/or design. We reasoned that if a witness reports a tattoo of a bird, for example, the police would be unlikely to show the witness a lineup unless the suspect had a bird tattoo of some sort (e.g., they would not show the witness someone with a spiderweb tattoo). However, two things are apparent from our data that suggest this assumption may be inappropriate. First, many of the participants who described the perpetrator failed to report the presence of a tattoo of any kind; only about a third of the participants spontaneously reported that the perpetrator had a tattoo. Our target-present control lineup indicated that the eyewitnesses in our study had a relatively good memory for the perpetrator: .84 of eyewitnesses correctly identified the perpetrator. However, this strong memory for the perpetrator did not extend to their memory for the tattoo—only .15 of those same eyewitnesses correctly identified the perpetrator's tattoo from the tattoo lineup, with .42 reporting that the mock-perpetrator did not have a tattoo.

Second, of small proportion of participants who did correctly mention the perpetrator had a tattoo, most did not provide considerable detail about it. Most (.86) that mentioned the tattoo noted location information but less than half (.44) mentioned described the design. Although other researchers have demonstrated that real witnesses frequently provide little

detail in their descriptions (e.g., Lindsay, Martin, & Webber, 1994; Meissner & Sporer, 2007), this is the first demonstration that the lack of detail in descriptions extends to distinctive features like tattoos. This result is in contrast to Colloff (2016, Chapter 4) where most participants described a distinctive mark—perhaps because they were prompted to be thorough in their descriptions. Still, like our participants, Colloff’s participants were more likely to describe the mark’s location (.42 of their describers) than to describe it in detail (.03 of their describers) or both (.10 of their describers). These results suggest a caveat to current best practice recommendations, which encourage police officers to match lineup fillers to the witnesses’ descriptions (Wells et al., 2020; Wells, et al., 1998). Our findings suggest that it might be important to request a detailed description in order for this recommendation to be effective. More research is needed to further explore differences between eyewitnesses’ memory for tattoos and the actual tattoos on perpetrators.

General Discussion

Our four experiments demonstrate that more information is needed about how eyewitnesses’ decisions and metacognitions are affected by witnessing a perpetrator with a distinctive mark. Contrary to the small, extant literature (i.e., Colloff et al., 2016), we found that a lineup, biased by the inclusion of a tattoo on the suspect only, does not increase correct identifications from simultaneous lineups (Experiments 1 & 2). Further complicating matters, we found that a biased sequential lineup did increase correct identifications compared to fair sequential lineups.

Clearly, more than just lineup bias influences correct identifications by eyewitnesses who saw a tattooed perpetrator. Given the considerably higher rate of correct identifications in our experiments compared to Colloff et al.’s, and the prediction of the outshining hypothesis that contextual cues will only be relied on when memory quality is poor (Smith et al., 2018), we suggest that memory quality may be a moderating factor. Indeed, our findings

support Charman and Wells' (2007) proposition that eyewitnesses use more effective decision strategies when memory quality is good versus poor. Related to memory quality is another potential contributing factor: whether the eyewitness notices that the perpetrator had a tattoo. Experiment 4b demonstrated that participants can have a strong enough memory to identify the perpetrator when present (as evidenced by the correct identification rate) while having a poor memory for the perpetrator's tattoo; a similar proportion of those that made correct identifications mentioned the tattoo in their description (.38) as the proportion across the full sample (.34).

The lack of consistent findings across our experiments and in the broader literature also indicate the need for more theorising about the influence of distinctive features on eyewitness lineup decisions. The correct identification rates from Experiment 1 and 2 from the simultaneous lineups were consistent with the outshining hypothesis (Smith, 1988), whereas the sequential lineup correct identification rates from Experiment 2 were consistent with filler siphoning (Wells, et al., 2015) and the DFD model (Wixted & Mickes, 2014). The results from neither experiment were consistent with the HS-GCM (Nosofsky & Zaki, 2003). Researchers studying distinctive marks should routinely begin measuring whether participants detect a perpetrator's distinctive mark and examining whether their results differ depending on whether or not participants notice a perpetrator's distinctive mark. Such data may help illuminate which theory best accounts for eyewitness behaviour under which conditions.

Another key finding from our work is that—if the suspect has a tattoo—the replication and removal approaches for making a lineup unbiased seem to be effective. In Experiment 1, we found similar rates of correct and innocent suspect identifications, respectively, across the unbiased lineup treatment conditions (unbiased blocking, unbiased replication, and unbiased removal), with the exception that blocking the tattoo area on all

lineup members produced more innocent suspect identifications than the other unbiased treatments. Colloff et al. (2016), in contrast, found no difference between such blocking and the other unbiased lineup conditions they tested (replication and pixelation). We cannot offer an explanation for this conflicting finding because it stands in contrast to the four theories that could explain the effects of tattoo treatment on eyewitness identifications. Replication of this finding is necessary to provide further evidence about the effectiveness of blocking the area on all lineup members.

In our Experiments 3 and 4, we considered a practical question that prior eyewitness identification research has not addressed: the extent to which the similarity of an innocent suspect's tattoo to the perpetrator's tattoo affects the likelihood of innocent suspect identifications. Based on the summed similarity rule of the HS-GCM (Nosofsky & Zaki, 2003), we expected that the greater the similarity, the greater the likelihood of an innocent suspect identification. Our findings from both Experiment 3 and 4 provided (at least) positive/substantial evidence consistent with this prediction, indicating some participants encoded the tattoo and relied on it as a cue. Considering Tables 5 and 6, it is clear that when an innocent suspect has a tattoo in the same location and with the same design as the perpetrator, they are the more likely to be identified than if their tattoo is similar or different in terms of location, design, or both. Likewise, when an innocent suspect has a tattoo in a clearly different location with a clearly different design, they are least likely to be identified. In this circumstance, the innocent suspect's tattoo actually served as an excluding feature, protecting the innocent suspect from being identified.

The protective nature of a very different tattoo suggests that if Tyrone Lamont Allen is innocent, the uniqueness of his tattoos may have served a protective role. Therefore, modifying Allen's lineup photograph to make him look more similar to the CCTV footage may indeed have put him at greater risk as his lawyer argued. However, the results of

Experiments 1 and 2 suggest that, had there been no CCTV footage, the police had acted appropriately to ensure Allen did not stand out in the lineup. We found that removing the tattoo from the suspect (and the fillers) reduced innocent suspect identifications compared to a biased presentation (where the innocent suspect was shown with an identical tattoo to the perpetrator or a patch in that area) but did not impact correct identifications from simultaneous lineups. This unusual case in combination with our findings highlight the importance of considering the eyewitnesses' description and the suspect's appearance when selecting a lineup construction method.

Despite our evidence that the mock-perpetrators' tattoos were encoded by at least some participants, only about a third spontaneously mentioned the tattoo when describing the perpetrator immediately after the mock-crime video. Colloff (2016) concluded that even participants who failed to describe a distinctive mark used that distinctive mark in their lineup decision. In our data some participants clearly remembered the tattoo but failed to report it when describing the perpetrator as about half (.45) of participants indicated that they had not noticed a tattoo on the mock-perpetrator when they viewed the tattoo lineup (cf. the two-thirds you would expect based on the perpetrator descriptions). Supplementary Table 4 shows that participants who identified the suspect but did not report a tattoo in their description were mostly ones who saw a target-present lineup, and thus may have relied on the similarity between their memory and the lineup member (i.e., *ecphory*, Tulving, 1982) to make their correct identifications. Only when participants saw an innocent suspect with the same tattoo design and location, did they identify the suspect even when they had not reported a tattoo.

Taken together with the identification results in relation to tattoo similarity, our findings indicate that while some participants may have noticed but not reported a tattoo, others clearly did not notice a tattoo and used the presence of a tattoo to exclude lineup members. A possible explanation for this failure to encode the tattoo is that participants were

given a short exposure to the perpetrator (approximately 3 seconds). Yet, the high rate of correct identifications across all experiments (e.g., .68 in an unbiased sequential lineup, .88 and .83 in an unbiased simultaneous lineup) demonstrate that participants had sufficient opportunity to encode the perpetrator. Again, more research is needed to examine how eyewitness identification decision making is related to reporting or not reporting a tattoo on a witnessed perpetrator.

There has been a limited consideration of how confidence may be affected by distinctive marks. Colloff et al. (2016) found that biased lineups resulted in overconfidence and other work has suggested that confidence will be inflated for identifications from biased compared to unbiased lineups (e.g., Leippe & Eisenstadt, 2009). In Experiment 1, confidence was unaffected by tattoo treatment or bias (correct and innocent suspect identifications). In Experiment 2, confidence was inflated in biased lineups compared to unbiased lineups (correct and innocent suspect identifications), but the evidence was very weak for this effect. Yet, in Experiment 3, confidence was inflated for innocent suspect identifications when both the tattoo location and design matched the perpetrator's tattoo compared to when the innocent suspect had no tattoo. Encouragingly, however, confidence in correct identifications were significantly higher than for innocent suspects shown with a tattoo in the same location with the same design. Finally, in Experiment 4 confidence did not vary across tattoo conditions or target presence. Taken together, these results suggest that confidence did not differentiate well between biased and unbiased lineups (Experiments 1 and 2) or more and less biased lineups (Experiments 3 and 4) but did differentiate between accurate and inaccurate identifications (Experiment 3). However, more research is needed as the rate of innocent suspect identifications in Experiments 3 and 4 was quite low (thus, our tests of confidence were low powered).

The variability in our methods across experiments is both a strength and a limitation. It is a strength to the extent that we demonstrate the generalizability of some patterns (e.g., the negative effect of biased lineups) but cannot rule out methodological explanations for non-replicating findings (e.g., the effect of tattoo removal on innocent suspect identifications). We concluded that memory quality likely played a role in some of the inconsistencies in results across Experiments 1–2 and Experiments 3–4; only additional research can address these. Experiments 1 and 2 involved multiple trials which may have drawn attention to the presence of tattoos whereas Experiments 3 and 4 involved only a single trial and therefore the participants could not be primed to expect the presence of a tattoo. There were also differences in the intervening tasks across the experiments and within Experiment 4. Although this kind of variability is important for ensuring that findings are generalizable, it also makes it more difficult to draw conclusions about future directions.

What are the key questions that should be investigated about tattooed suspects and eyewitness identification? We think that researchers need to explore if decisions are affected by whether the eyewitness described the tattoo. Prior research, including our own first three experiments, has assumed participants noticed the tattoo, however our Experiment 4 demonstrates that this is likely not a valid assumption. It is possible that our conclusions about the treatment of tattoos in lineups will change when we look separately at participants who described a tattoo and those that did not. At this point, it is unclear whether participants who identify tattooed suspects from biased lineups have chosen the only person with the remembered tattoo, or whether they have selected the “oddball” in the lineup. Differing rationales such as these may explain why the results for correct identifications sometimes align poorly with theories of memory.

Another future direction to consider is whether and how distinctive mark lineups can be used to gather independent evidence about the suspect. Distinctive marks are salient

memorial cues and prior research demonstrates that the more features an eyewitness can correctly identify, the more likely they are to be accurate (Kalmel, 2009; Pryke, Lindsay, Dysart, et al., 2004). Although eyewitnesses may generally perform poorly on a distinctive mark lineup, the additional probative value provided may come at little or no cost. However, this requires further investigation. Our results and some prior work (Colloff et al., 2016) indicate that a lineup procedure in which the distinctive mark is covered or obscured elicits similar performance to one where the distinctive mark is replicated on all lineup members. However, Zarkadi et al. (2009) and Badham et al. (2013) both found replication to be superior to other unbiassing approaches; therefore, further study is required.

In conclusion, our results indicate more research is needed to understand how police should treat eyewitnesses who viewed perpetrators with distinctive marks. In particular, eyewitness descriptions should be collected and considered in relation to the identification results. Also, whether biased tattoo lineups influence correct identifications and how confidence is affected requires further exploration. Encouragingly, we replicated prior results that replicating or removing the tattoo can effectively produce unbiased lineups for tattooed suspects. Our findings do indicate, however, that researchers should further study whether hiding a tattoo by blocking it is an effective approach. Finally, our results suggest that a mismatch between an innocent suspect's tattoo and the perpetrator's tattoo will not protect an innocent suspect unless the mismatch is salient (i.e., an obvious difference in location and design).

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Table 1*Use of Different Approaches to Suspects with Distinctive Marks in Canada and the USA*

Approach	Canada		USA	
	<i>n</i>	% of Sample	<i>n</i>	% of Sample
Try to find lineup members with similar marks	47	70.1	76	59.4
Try to add similar marks to other lineup members	6	9	16	12.5
Cover up the marks on the suspect	1	1.5	8	6.2
Cover the position of the markings on all lineup members, including the suspect	7	10.4	15	11.7
Do nothing	6	9	13	10.2

Note: Participants were asked “What do you usually do when a suspect has a distinctive

facial marking like a birthmark, mole, scar, or tattoo?

Table 2*Proportion of Lineup Responses by Tattoo Treatment in Experiment 1*

Tattoo Treatment	Lineup Decision							
	Target-Present Lineups				Target-Absent Lineups			
	Suspect IDs	Filler IDs	Rejections	Don't Know	Suspect IDs	Filler IDs	Rejections	Don't Know
Biased tattoo	.89 [.83, .96]	.05 [.002, .09]	.05 [.002, .09]	.01 [0, .04]	.21 [.13, .30]	.29 [.19, .38]	.43 [.32, .53]	.07 [.02, .13]
Biased blocking	.85 [.77, .93]	.10 [.03, .17]	.04 [0, .08]	.01 [0, .04]	.16 [.08, .24]	.40 [.29, .51]	.39 [.28, .49]	.05 [.002, .10]
Unbiased blocking	.89 [.82, .96]	.02 [0, .06]	.08 [.02, .13]	.01 [0, .04]	.14 [.06, .21]	.28 [.18, .37]	.55 [.44, .66]	.04 [0, .08]
Unbiased replication	.79 [.70, .88]	.09 [.02, .15]	.10 [.03, .17]	.02 [0, .06]	.05 [.002, .10]	.44 [.33, .55]	.50 [.39, .61]	.01 [0, .04]
Unbiased removal	.88 [.80, .95]	.08 [.02, .13]	.02 [0, .06]	.02 [0, .06]	.02 [0, .06]	.44 [.33, .55]	.49 [.38, .60]	.05 [.002, .10]

Note: We report 95% confidence intervals. All values are calculated across trials and participants.

Table 3*Mean Confidence in Guilty Suspect and Innocent Suspect Identifications in Experiment 1*

Tattoo Treatment	Correct identifications		Innocent Suspect Identifications	
Biased tattoo	91.11	[85.39, 96.83]	66.24	[53.38, 79.10]
Biased blocking	85.56	[79.66, 91.45]	76.90	[62.81, 90.99]
Unbiased blocking	87.50	[81.65, 93.34]	64.55	[49.44, 79.67]
Unbiased replication	85.45	[79.40, 91.50]	75.83	[53.61, 98.04]
Unbiased removal	83.42	[77.55, 89.29]	69.35	[37.60, 100.00]
Biased	88.42	[84.32, 92.52]	71.02	[61.70, 80.33]
Unbiased	85.46	[82.05, 88.87]	68.08	[56.73, 79.42]
Tattoo visible	90.97	[85.39, 96.54]	65.11	[50.81, 79.42]
Tattoo not visible	85.44	[79.70, 91.18]	75.67	[59.91, 91.44]
Overall	86.61	[83.98, 89.23]	70.58	[61.51, 79.64]

Note: We report 95% confidence intervals. All values are calculated across trials and

participants. Models included trial number as a predictor.

Table 4*Proportion of Lineup Responses by Tattoo Treatment and Lineup Type in Experiment 2*

Lineup	Lineup Decision					
	Target-Present Lineups			Target-Absent Lineups		
	Suspect IDs	Filler IDs	Rejections	Suspect IDs	Filler IDs	Rejections
Simultaneous	.88 [.83, .93]	.07 [.03, .10]	.06 [.02, .09]	.19 [.14, .25]	.22 [.16, .28]	.59 [.52, .66]
Unbiased	.83 [.75, .91]	.12 [.06, .19]	.04 [.002, .09]	.09 [.03, .15]	.31 [.21, .40]	.60 [.50, .70]
Biased	.92 [.87, .98]	.01 [0, .03]	.06 [.01, .12]	.29 [.20, .39]	.13 [.06, .20]	.58 [.48, .68]
Sequential	.79 [.74, .85]	.08 [.04, .12]	.12 [.07, .17]	.22 [.16, .28]	.19 [.14, .25]	.58 [.51, .66]
Unbiased	.68 [.59, .78]	.13 [.06, .20]	.18 [.10, .26]	.10 [.04, .16]	.26 [.17, .35]	.64 [.54, .74]
Biased	.91 [.85, .97]	.03 [0, .07]	.06 [.01, .10]	.35 [.25, .45]	.12 [.06, .19]	.52 [.42, .63]
Overall	.84 [.80, .87]	.08 [.05, .10]	.09 [.06, .12]	.21 [.17, .25]	.21 [.16, .25]	.59 [.54, .64]
Unbiased	.76 [.69, .82]	.13 [.08, .18]	.12 [.07, .16]	.09 [.05, .14]	.28 [.22, .35]	.62 [.55, .69]
Biased	.92 [.88, .96]	.02 [.001, .04]	.06 [.03, .10]	.32 [.25, .39]	.13 [.08, .18]	.55 [.48, .62]

Note: We report 95% confidence intervals. All values are calculated across trials and participants.

Table 5*Proportion of Responses to Lineups in Experiment 3*

Lineup	Proportion Lineup Decisions				<i>n</i>
	Suspect IDs	Filler IDs	Rejections	Don't Know	
Target-present	.84	.08	.04	.04	51
Target-absent					
No tattoo	.13	.19	.58	.10	31
Same location, same design	.20	.18	.53	.09	45
Same location, different design	.09	.16	.69	.06	77
Different location, same design	.17	.27	.47	.10	30
Different locaiton, different design	.02	.26	.66	.06	53

Table 6*Proportion of Responses to Face Lineups in Experiment 4*

Lineup Shown	Suspect IDs	Filler IDs	Rejections
Experiment 4a			
Target-present	.88	.12	0
Target-absent			
Same location, same design	.16	.16	.68
Same location, similar design	.21	.29	.50
Similar location, same design	.15	.31	.54
Similar location, similar design	.26	.18	.56
Experiment 4b			
Target-present	.81	.04	.15
Target-absent			
Same location, same design	.30	.18	.52
Same location, similar design	.16	.32	.52
Similar location, same design	.08	.21	.71
Similar location, similar design	0	.22	.78
Overall			
Target-present	.84	.08	.08
Target-absent			
Same location, same design	.23	.17	.60
Same location, similar design	.18	.31	.51
Similar location, same design	.12	.26	.62
Similar location, similar design	.14	.20	.66

Figure 1*Lineups Illustrating the Different Tattoo Treatment Conditions Used*

Note: A) biased tattoo, B) biased blocking, C) unbiased blocking, D) unbiased replication, and E) unbiased removal. Panel F illustrates how faces were presented for sequential lineups (Experiment 2). We used the first five panels in Experiment 1 (A through E). We used panels A, E, and F in Experiment 2. We used panels A and E in Experiment 3. We used panel A in Experiment 4.

Figure 2

Confidence in Innocent Suspect Identifications as a Function of Lineup Type and Tattoo Treatment in Experiment 2

Note: Error bars indicate 95% confidence intervals around the estimated marginal mean.

Figure 3

Example Stimuli Illustrating the Innocent Suspects' appearance in Experiment 3 by Tattoo Location and Tattoo Design

Note: The top panel shows a perpetrator from the mock-crime video. The lower two panels illustrate the innocent suspect's appearance across the manipulations.

Figure 4

Example Stimuli Illustrating the Innocent Suspects' Appearance in Experiment 4 by Tattoo Location and Tattoo Design

Note: The top panel shows a perpetrator from the mock-crime video. The lower two panels illustrate the innocent suspect's appearance across the manipulations.

Figure 5

How Tattoos were used in the Experiments

Note: Panel A provides a screen shot of a mock crime video and Panel B shows how a guilty suspect appeared in a biased tattoo lineup, both used in Experiments 1 to 4. Panel C depicts the appearance of an innocent suspect with a similar tattoo (Experiment 4). Panel D shows a tattoo lineup presented to participants in Experiment 4b; also note that the tattoo from Panel B (but not Panel C) appears in the lineup (position 2).

Supplementary Table 1*Summary of Theory-Based Predictions for Experiment 1 Conditions*

Tattoo Treatment	Predictions Informed by Relevant Theories								Recategorization of Tattoo Treatment (Experiment 1)	
	HS-GCM		Outshining Hypothesis		Filler Siphoning		DFD		Lineup Bias	Tattoo Visibility
	GS IDs	IS IDs	GS IDs	IS IDs	GS IDs	IS IDs	GS IDs	IS IDs		
Biased Tattoo	↑	↑	= or ↑	↑	↑	↑	↑	↑	Biased	Visible
Biased Blocking	↓	↓	= or ↑	↓	↑	↑	↑	↑	Biased	Not visible
Unbiased Blocking	↓	↓	= or ↓	↓	↓	↓	↓	↓	Unbiased	Not visible
Unbiased Replication	↑	↓	= or ↓	↓	↓	↓	↓	↓	Unbiased	Visible
Unbiased Removal	↓	↓	= or ↓	↓	↓	↓	↓	↓	Unbiased	Not visible

Note: ↑ indicates expected higher rates of suspect identifications compared to other conditions. ↓ indicates expected lower rates of suspect identifications compared to other

conditions. = indicates expected equal rates of suspect identifications to other conditions. Conditions within columns that share symbols are presumed to not differ

significantly from one another. GS IDs = guilty suspect (correct) identifications. IS IDs = innocent suspect identifications. HS-GCM = hybrid similarity global context

model. DFD = diagnostic feature detection model.

Supplementary Table 2*Frequency of Mock Witness (n = 36) Choices for Target-Absent (Unbiased Removal) Lineups*

Perpetrator	Filler Identified (position)					
	1	2	3	4	5	6
M02	.19	.14	.08	.14	.28	.17
M05	.03	.28	.25	.36	0	.08
M17	.06	0	0	.08	.72	.14
M20	.39	.25	.03	.03	.22	.08
F03	.11	.33	.17	.14	.19	.06
F08	.08	.14	.25	.22	.19	.11
F09	.44	.11	0	.19	.14	.11
F14	.31	.22	.06	.14	.03	.25

Note: the innocent suspect's position is bolded. Perpetrator labels with an M indicate a male

perpetrator (e.g., M20) while labels with an F indicate a female perpetrator (e.g., F14).

Supplementary Table 3*Confidence in Correct Identifications and Innocent Suspect Identifications in Experiment 2*

Tattoo Treatment	Correct Identifications		Innocent Suspect Identifications	
Simultaneous Lineups	85.97	[82.97, 88.96]	69.40	[62.16, 76.63]
Biased	87.15	[83.28, 91.02]	76.52	[69.80, 83.24]
Unbiased	84.59	[79.92, 89.26]	45.38	[33.73, 57.02]
Sequential Lineups	87.02	[84.13, 89.91]	82.65	[76.73, 88.57]
Biased	90.09	[86.70, 93.48]	86.94	[80.27, 93.60]
Unbiased	83.13	[78.32, 87.94]	67.89	[60.74, 75.03]
Biased	88.58	[85.99, 91.16]	82.09	[77.20, 86.98]
Unbiased	83.91	[80.57, 87.25]	57.29	[48.82, 65.76]
Overall	86.47	[84.38, 88.55]	76.47	[71.63, 81.30]

Note: We report 95% confidence intervals. All values are calculated across trials and

participants.

Supplementary Table 4*Number of Lineup Decisions by Whether Participants Reported a Tattoo in Experiment 4b*

Lineup Shown	Suspect IDs		Filler IDs		Rejections	
	Reported Tattoo	No Report of Tattoo	Reported Tattoo	No Report of Tattoo	Reported Tattoo	No Report of Tattoo
Target-present	8	13	0	1	2	2
Target-absent						
Same design, same location	5	3	0	5	6	8
Same design, similar location	2	0	1	4	2	15
Similar design, same location	4	0	2	6	6	7
Similar design, similar location	0	0	0	5	5	13

Supplementary Table 5*Tattoo Lineup Accuracy and Confidence in Experiment 4b According to Face Lineup Condition*

Face Lineup	Correct Tattoo IDs		Filler Tattoo IDs		Incorrect Rejections		No Tattoo	
	Proportion	Confidence <i>M</i> (<i>SD</i>)	Proportion	Confidence <i>M</i> (<i>SD</i>)	Proportion	Confidence <i>M</i> (<i>SD</i>)	Proportion	Confidence <i>M</i> (<i>SD</i>)
Target-present	.15	68.25 (20.95)	.23	47.33 (19.00)	.19	61.00 (24.60)	.42	51.73 (32.32)
Target-absent								
Same design, same location	.11	30.00 (22.00)	.30	40.62 (26.25)	.18	53.80 (31.70)	.41	62.36 (29.71)
Same design, similar location	.17	70.00 (21.60)	.21	35.80 (19.70)	.04	19.00*	.58	50.50 (28.51)
Similar design, same location	.12	63.33 (15.28)	.24	47.67 (25.87)	.28	59.28 (35.64)	.36	58.22 (37.38)
Similar design, similar location	0	--	.35	59.25 (27.96)	.17	56.25 (36.43)	.48	61.09 (29.46)
Overall	.11	59.50 (24.10)	.26	46.91 (24.46)	.18	56.04 (30.58)	.45	56.39 (30.48)

Note: * Only one participant made this response, therefore there is no deviation.