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An Examination of the Effects of Post-Identification Feedback on Jurors

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Abstract

Post-identification feedback (PIF) occurs when witnesses are given feedback following their identification choices. This feedback has been shown to alter witnesses' retrospective judgments regarding their witnessing experience (e.g., they are more confident that they made a correct identification). PIF effects are robust; they impact witnesses' memory of their experience, and also appear to act as confirmation to jurors who are asked to assess witness reliability. A current recommendation for eyewitness procedures is that identifications should be recorded and shown to jurors at trial, but this might be harmful if jurors are also negatively impacted by this suggestive feedback. The goal of the current study was to further evaluate how PIF can impact jurors' assessments of witnesses. Across two experiments, I failed to find evidence that viewing eyewitnesses receiving PIF impacted mock-jurors' perceptions of the witness or of their own performance. Additionally, the presence of PIF warnings in Experiment 2 did not impact participants' perceptions of either the witness or their own performance. Given the recent push for videotaped identification procedures, the findings from these studies are timely and beneficial to the legal system.

Introduction

Eyewitness testimony is an important component in legal proceedings, however, both real world cases and laboratory studies have revealed that these identifications are not always accurate (Wells, 1993). To date, DNA evidence has exonerated 361 innocent individuals from serving time for crimes that they did not commit. Of these wrongful convictions, 70% involved at least one instance of mistaken identification (innocenceproject.org, accessed 2019). Myriad factors can contribute to wrongful witness identifications. The purpose of the proposed studies was to evaluate this problem at a broader scale. Specifically, the current study explored how jurors interpret eyewitness evidence and incorporate that information when assessing the witness. When considering the nature of mistaken eyewitness identifications in wrongful convictions, the issue is truly a two-stage problem for the legal system. First, the witness erroneously identifies an innocent individual. Second, jurors find the witness believable and find their testimony to be credible. The current studies explored factors that may influence jurors as they evaluate trial evidence.

Eyewitness identification

Eyewitnesses are one of many types of evidence that can be presented during legal proceedings. Fortunately, there has been a plethora of research conducted that has focused on exactly how different factors can impact eyewitness accuracy (Wells & Olson, 2003). These factors are commonly classified into two categories: estimator and system variables (Wells, 1978). Estimator variables are factors that are not under the control of the legal system (e.g., the age of a witness, the viewing conditions the witness had, delay between crime and identification). Alternatively, system variables are factors that are within the control of the legal system (e.g., type of lineup administered, utilizing expert witnesses, lineup composition). There

has been substantial information acquired from investigations of both system and estimator variables. For example, research on estimator variables has shown eyewitness performance can be impaired when witnesses did not have a good opportunity to view the culprit (Ellis, Davies, & Shepherd, 1977), when the witness is a different race from the culprit (Meissner & Brigham, 2001), when a weapon is present (Steblay, 1992), and when there is a longer delay between the crime event and when witnesses are questioned about it (Lipton, 1977). Although these are things that cannot be controlled by the legal system, knowledge of how these factors come into play in a specific crime event can help evaluators know when identifications may be more or less likely to be accurate.

System variables on the other hand, can arguably serve two roles. First, they can inform law officials regarding what are best practice guidelines. Second, they can help evaluators recognize the dangers (e.g., to identification accuracy) when these guidelines are not followed. Research has shed light onto numerous factors that can aid in eyewitness accuracy. For example, warning witnesses that a culprit may or may not be in the lineup (Malpass & Devine, 1981), making sure all lineup members are physically similar to the suspect (Lindsay & Wells, 1980), and using simultaneous lineups (Neuschatz, Wetmore, Key, Cash, Gronlund, & Goodsell, 2016) have all been shown to result in higher rates of witness accuracy. An additional factor to consider is that of either unconscious or conscious behavioral influence. Prior research has demonstrated that lineup administrators can use both verbal and nonverbal cues to suggest a specific suspect to the eyewitness (Phillips, McAuliff, Kovera, & Cutler, 1999; Wells, 1993). An additional danger is that this information can be conveyed either intentionally or unintentionally (as demonstrated in Garrioch & Brimacombe, 2001) to the witness. In this manner, the behavior of the lineup administrator can influence the eyewitness's decision. One solution to this is to make sure the

lineup administrator does not know the identity of the suspect. This is referred to as a *double-blind* lineup, where neither the witness nor the lineup administrator have knowledge of who the suspect is (Wells & Luus, 1990). Specifically, the witness should only rely on their memory, and not have any indication about who the police most suspect. Wells and Luus (1990) argued that this approach is similar to the recommendation that researchers should not know what condition participants are in order to avoid influencing the results of a study (Rosenthal, 1976). In this view, the identification procedure can be seen as analogous to a study where the police officer has a hypothesis that the suspect is a culprit. They test this hypothesis by presenting a lineup to a witness, where an identification (or lack thereof) can provide evidence to support (or refute) that claim. Although the difference in witness accuracy as a function of double- versus single-blind (only the witness does not know who the suspect is) lineup administration remains relatively understudied, laboratory studies confirm that single-blind methods lead to more erroneous identifications (Perlini & Silvaggio, 2007; Phillips, McAuliff, Kovera, & Cutler, 1991). Additionally, the recommendation to employ double blind lineup administration is strongly encouraged by researchers (Wells, 1993; Wells & Seelau, 1995; Wells, Small, Penrod, Malpass, Fulero, & Brimacombe, 1998) and governing bodies alike (Technical Working Group, 1999). This notion highlights the danger that witnesses may not be solely incorporating information from the crime scene, but may also be including information that they obtain after the event occurs – for better or worse.

Eyewitness suggestibility

Contrary to the popular conceptualization that memory is like a video camera, research in both the basic and applied literatures have shown that memory is malleable (Loftus, 2005; Schacter, 2001). This reconstructive nature of memory also extends to witnesses' memory for a

crime. For example, in a series of highly influential studies, Loftus showed how eyewitness reports could be altered by information provided *after* an event had occurred (also known as the misinformation effect). Her novel paradigm had participants view a short film of an automobile accident. After, participants were asked follow up questions about the event where some participants were asked "how fast were the cars going when they *smashed* into each other" while other participants saw other verbs such as *bumped* or *hit* instead. When participants provided their speed estimates, participants who had received the *smashed* verb provided higher speed judgments than did those who saw the *bumped* or *hit* verbs (Loftus & Palmer, 1974). Additionally, Loftus also demonstrated that memory for details could be altered by post-event information. For example, Loftus found that participants would report different colored vehicles (Loftus, 1975), report having seen a barn that was never present (Loftus, 1975), and report having seen a stop sign when they had actually seen a yield sign (Loftus, Miller, & Burns, 1978) when leading questions suggested these pieces of information to be factual.

In addition to being a heavily documented phenomenon, there are also well known factors that can influence the strength of these effects. For example, having a longer delay between the original event and the post-event information presentation increases the misinformation effect (Loftus, Miller, Burns, 1978). Furthermore, when the post-event information comes from a more credible source, it is much more likely to be incorporated than when the information comes from a less credible source (Smith & Ellsworth, 1987; Underwood & Pezdek, 1998). Age has also been shown to impact susceptibility, with younger children and older adults being more prone to these memorial inaccuracies (Davis & Loftus, 2005; Roediger & Geraci, 2007). However, the misinformation effect has been shown to be mitigated when participants are able to recognize inconsistencies between their memory and that of the post-

event information (Putnam, Sungkhasettee, & Roediger, 2017; Tousignant, Hall, & Loftus, 1986).

The theoretical underpinnings of the misinformation account have been an area that has received some debate. Loftus had initially posited that the misinformation effect was due to a destructive updating process (Loftus, 1979; Loftus, Miller, & Burns, 1978). Specifically, when participants are exposed to the misleading post-event information, the novel information overrides the original, and deletes that information from memory. McCloskey and Zaragoza (1985) argued for an alternate explanation of misinformation effects. Specifically, they noted that the phenomenon may not be due to memory deletion, but rather that the memory for that specific information may or may not be there to begin with. Their argument centered on the notion that participants may not have encoded the detail (e.g., stop sign) at all, therefore, when participants select the misleading detail (e.g., yield sign), this is because the post-event information served to fill in a memorial gap for the participants. This notion was supported by using what they termed a Modified Test procedure to examine the misinformation effect. In their study, participants saw an office theft where there was a handyman who was seen holding a hammer. Participants in the experimental condition were then given the incorrect misinformation that the handyman was holding a screwdriver instead. Participants were then either given the Standard or Modified test where they chose between the original and a novel item (hammer vs. wrench). The results revealed that participants in the experimental condition were just as likely to select the original item as were participants in the control condition (who had not been exposed to the misleading information). These findings suggest that these original memories are not necessarily deleted and that there are alternative explanations to the misinformation effect.

The extensiveness of the misinformation effect has been shown to be quite broad, with research showing that people can have false childhood memories implanted of them having been hospitalized (Hyman, Husband, & Billings, 1995), that they had been lost in a mall (Loftus, Coan, & Pickrell, 1996), that they had been attacked by an animal (Porter, 1998), or even that they had seen a demonic possession (Guiliana, Mazzoni, Loftus,& Kirsch, 2001). However, these real-world applications are not only limited to events from long ago, where memory could be poor. Morgan et al. (2007; 2011) demonstrated that misinformation effects occur for members of the military who attended "survival schools" that train them to deal with the stresses that they would face if being captured as a prisoner of war. In these studies, participants were susceptible to various contaminants of memory for the event, which led to them mistakenly identifying the person who assaulted them earlier.

Taken as a whole, this literature further cements the notion that memories are not infallible and that post-event information can lead witnesses astray. These studies document how memory for an event can be altered or even created and show how a particular word or leading question can alter memory. Another way that witnesses can be exposed to suggestive statements can pertain to indicators of accuracy given *after* they make an identification (e.g., "You correctly identified the suspect"). This is known as the Post-identification feedback (PIF) effect.

Post-identification feedback

Post-identification feedback is a well-studied phenomenon within the legal system, with research on the topic spanning over 15 years (Steblay, Wells, & Douglass, 2014). PIF occurs when a witness to a crime is given some sort of feedback after making an identification decision pertaining to the accuracy of their choice (Wells & Bradfield, 1998; 1999). Although post-identification feedback does not contribute to wrongful identifications, it makes the witness

considerably more confident in their identification accuracy. This effect is not only limited to laboratory participants, but has also been observed for actual witnesses and victims in real crimes (Wright & Skagerberg, 2007).

The danger posed by PIF has recently been acknowledged by the Oregon Supreme Court, which has now switched the burden of proof from the defense to prove that an eyewitness identification is inaccurate, to the prosecution to prove that it is admissible. Oregon now notes PIF as one factor that can negatively impact eyewitness reliability. "Confirming feedback, by definition, takes place after an identification and thus does not affect the result of the identification itself. It can, however, falsely inflate witness confidence in the reports they tender regarding many of the factors commonly used by courts and jurors to gauge eyewitness reliability. As a result, the danger of confirming feedback lies in its potential to increase the appearance of reliability without increasing reliability itself" (Oregon v. Lawson, 2012, p. 21).

In the seminal study examining PIF, Wells and Bradfield (1998) presented mock-witnesses with either confirming ("Good, you identified the actual suspect") or disconfirming ("Actually, the suspect is number _") feedback prior to asking them to provide confidence judgments regarding their identification choice. It is important to note that this study utilized a target-absent lineup, so any identification made by participants was inaccurate. The results revealed that participants who received confirming PIF were significantly more confident in their decision than were participants who did not receive feedback. The inverse was seen for participants who received the disconfirming PIF, as they reported lower confidence ratings. Not only did this feedback impact ratings of confidence, but these effects also extended into other judgments participants made regarding their witnessing experience. Specifically, confirming and disconfirming PIF increased and decreased, respectively, participants' judgments of how much

attention they paid to the event, how willing to testify they would be, how easy or difficult it was to make their identification, and how much information they had to base their identification decision on. Confirming PIF (but not disconfirming) also impacted additional judgments. Participants felt that they had a better view of the crime, took less time to make an identification, should be trusted, and were able to make out more facial details. These findings demonstrate two important facts. First, judgments that individuals make retrospectively about their memories can be influenced by information obtained *after* the event occurs, and second, the effect of this information can extend broadly into other retrospective judgments pertaining to the event. An area in the basic literature that is similar to this phenomenon is the hindsight bias or the "knew it all along" effect (Fischhoff, 1975; Fischhoff & Beyth, 1975; Wood, 1978).

The hindsight bias is typically evaluated by providing outcome information to people who are then asked if they would have been able to predict the outcome if they were not initially given that information (Fischhoff, 1977). The finding is that largely, people are prone to overestimate the likelihood that they could have predicted the outcome. However, the hindsight bias is also observed when individuals' *memory* for their initial predictions are altered to match the new outcome information (Fischhoff, 1977). In one of the earlier studies on the hindsight bias, Fischhoff (1977) presented general knowledge questions to participants. Some of these participants were in the memory condition, where they were asked to answer the questions, were then provided the correct answers, and then asked to recall the answers they had given earlier. Alternatively, other participants were assigned to the hypothetical group who were given the correct answers and then asked to indicate if they would have answered the question correctly. Participants in the hypothetical group were more likely to overestimate their accuracy compared to a control condition that actually answered the questions. Furthermore, participants in the

memory group were more likely to misremember their original judgments as having been more accurate than they truly were. These findings demonstrate that the hindsight bias not only impacts retrospective judgments of performance, but also distorts individuals' memory for how they *actually* performed on a task.

The hindsight bias has been a topic of considerable investigation and has been shown to be a robust effect that is not easily negated (see Hawkins & Hastie, 1990 for a review). One explanation for hindsight bias effects is that the hindsight bias produces a narrow top-down processing approach, where individuals spend more time reasoning why one option is correct rather than considering all of the potential options (Slovic & Fischhoff, 1977). This explanation has also been incorporated into the overconfidence literature by Koriat, Lichtenstein, and Fischhoff (1980), who argued that overconfidence is a product of individuals trying to justify a single choice option while failing to do so for the alternative options. This notion was supported in their study where participants were asked to answer general knowledge questions using one of two methods. Participants were first given "control" instructions, which were to choose the correct answer and rate the probability that their answer was correct. Participants were then given a second set of questions and instructed to provide reasons for and against each answer option. Participants using the latter set of instructions provided the highest calibration of accuracy in their judgments compared to when they were not asked to consider reasons favoring and opposing all answer options. Furthermore, the hindsight bias has been shown to occur in a multitude of applied decision-making domains, such as gambling behavior (Baboushkin, Hardoon, Derevensky, & Gupta, 2001), medical diagnoses (Arkes, Wortmann, Saville, & Harkness, 1981), economics (Holzl, Kirchler, & Rodler, 2002), and jury decision-making (Hastie, Schkade, & Payne, 1999). Bradfield and Wells (2005) provided an important extension

to this literature by demonstrating that the effects of the hindsight bias are broad and not only impact judgments of how certain participants were that they were accurate, but also judgments pertaining to factors such as how long it took them to provide a decision, how good of a basis they had for their decision, etc.

While confirmatory PIF may harm eyewitnesses by giving them hindsight bias, its deleterious effects are magnified when considered in conjunction with the *Biggers'* criteria (*Neil v. Biggers*, 1972). In this case, the U.S. Supreme court listed a set of criteria that jurors should utilize when evaluating eyewitness testimony: 1) jurors should consider the witness' view of the culprit, 2) jurors should consider the amount of attention that the witness paid to both the crime and culprit, 3), how accurate the description that the witness provided was, 4) how certain the eyewitness is in their identification, 5) how much time had passed between the crime and the identification. Unfortunately, confirming feedback alters judgments for 3 of the 5 mentioned criteria (see Table 1). Adding an additional complication to jury decision-making, research conducted on how jurors assess witnesses has revealed that more confident eyewitnesses are judged to be much more compelling and accurate by mock-jurors (Cutler, Penrod, & Dexter, 1990; Fox & Walters, 1986; Wells, Ferguson, & Lindsay, 1981). Although high confidence is usually indicative of accuracy when judgments are collected immediately and in optimal witnessing conditions (Wixted, Mickes, Clark, Gronlund, & Roediger, 2015; Wixted & Wells, 2017), the confidence estimates assessed by jurors are given at trial, which is far removed from the immediate assessments that have been shown to be accurate. The implications of these findings are that confirming PIF will result in the witness *appearing* to be more accurate in the absence of any true increase in the likelihood of an accurate identification. Indeed, research has found that witnesses who receive confirming feedback are assessed as more credible compared

to witnesses who do not receive PIF (Douglass, Neuschatz, Imrich, & Wilkinson, 2009; Smalarz & Wells, 2014).

Table 1. Effect of PIF on Biggers' criteria items.

Criterion	**Effect of PIF**
View of culprit	Increased
Degree of witness's attention	Increased
Accuracy of culprit's description	No effect
Level of certainty	Increased
Time between crime and ID	No effect

Additional research on these statements has afforded further insight into the parameters involved in altering witnesses retrospective judgments. For example, it is not necessary that the feedback be given immediately after an identification. Studies providing the feedback after a 48 hour delay still demonstrate the feedback effect (Wells, Charman, Olson, 2003). Additionally, the issue of PIF is persistent, such that the feedback effect is seen even when participants are asked to provide retrospective judgments a week after receiving the PIF (Neuschatz et al., 2005). Even more innocuous or ambiguous statements such as "You have been a great witness" have been shown to produce the PIF effect (Dysart, Lawson, & Rainey, 2012). Furthermore, these feedback effects are seen in children (Hafstad, Memon, Logie, 2004), elderly witnesses (Neuschatz et al., 2005), for both target present and target absent lineups (Semmler, Brewer, & Wells, 2004), regardless of identification procedures (e.g., simultaneous lineup, sequential lineup, showup; Douglass, McQuiston-Surrett, 2006; Key, Wetmore, Cash, Neuschatz, & Gronlund, 2017), and regardless of lineup instructions (i.e., biased or unbiased; Semmler, Brewer, Wells, 2004). Moreover, these statements need not come from a police officer; research has shown that confirmatory feedback from jailhouse informants and co-witnesses also provide the same PIF effect (Erickson, Lampinen, Wooten, Wetmore, & Neuschatz, 2016; Mote, Neuschatz, Bornstein, Wetmore, & Key, 2018; Skagerberg & Wright, 2009). Neuschatz et al.

(2005) tested the trace strength hypotheses (Brainerd & Reyna, 1988) and the accessibility hypothesis (Wells, Charman, & Olson, 2003) by examining how PIF would impact elderly eyewitnesses. These two hypotheses make differential predictions. The trace strength hypothesis predicts that older adult witnesses should be more susceptible to PIF than younger witnesses because older adults have weaker memory traces (Karpel, Hoyer, & Toglia, 2001). Alternatively, the accessibility hypothesis does not predict age differences because this account predicts that after time has passed, people cannot remember information pertaining to the quality of their memory at the time they made an identification. The results from this study were in line with the accessibility hypothesis, as both younger and older adults were similarly influenced by the PIF, which the authors view as a testament to how powerful the PIF effect is. Specifically, the diagnostic cues that usually are present and benefit younger adults compared to older adults are not available to avoid susceptibility to PIF.

The robustness of PIF effects has been documented by two meta-analyses (Douglass & Steblay, 2006; Steblay, Wells, & Douglass; 2014). In the most recent meta-analysis, 23 PIF studies were evaluated and results revealed how heavily the presence of PIF impacts retrospective judgments. Eleven of the 13 factors (see Table 2) assessed were rated as being significantly higher in conditions where inaccurate participants received PIF as opposed to those who did not, suggesting that these mock-witnesses felt that they had a better witnessing experience than those who did not receive the statement. Additionally, the Cohen's D values for these significant factors ranged from .48 to .98, demonstrating that this is not a small effect. An equally important question is whether PIF also impacts accurate witnesses and the analyses revealed that the answer is yes, but to a smaller degree. Specifically, PIF given to accurate witnesses resulted in witnesses providing higher ratings for 10 of the 13 dependent measures, but

to a lesser extent than that of inaccurate witnesses. Between both accurate and inaccurate witnesses, assessments pertaining to duration of viewing time and distance measures were revealed to not be impacted by PIF. Although it might be initially surprising, it is possible that the reason for their departure from the traditional PIF effect might be due to the nature of the judgment being made. All affected judgments pertain to subjective experiences of the witness. However, viewing time and distance could be argued as being more "objective" experiences that may make them immune to the PIF statements. Additionally, while the more subjective judgments may be difficult for participants to access when they provide their ratings, witnesses may have a more concrete memorial representation of duration and distance.

Although meta-analyses can reveal whether effects are reliable across many studies, there are important limitations to consider. One issue that meta-analyses face is the type of stimuli that are utilized. Specifically, when a meta-analysis is comprised of studies with a restricted range of materials, it is difficult to draw strong conclusions regarding whether the effect is truly there or an artifact of the materials used. This issue is one that is prevalent within the PIF literature. As noted by Steblay et al. (2014), about half of the published PIF studies use either the "bomber on the roof" or the "Target store security" videos. In addition to the more commonly used "bomber on the roof" and "Target store security" videos, there are eight additional videos and one live event that are also used within the literature. Although the studies using the latter, less traditional stimuli produce similarly robust effects, the limited set of stimuli is a factor that can hinder meta-analyses. Another factor that can limit inferences from meta-analyses is the consistency (or lack thereof) of manipulations across the studies. A rigorous and diverse literature can help meta-analyses provide insight into the boundary conditions of different phenomena. In the PIF literature, however, most studies included in the meta-analysis used only two types of feedback,

with lineup administrators either providing feedback to witnesses or telling them that a high number of other witnesses identified the same person as them. While this provides important insight, it remains to be determined whether other types of feedback would yield similar PIF effects. A final consideration pertains to the availability of studies that are included in meta-analyses. Publication is biased toward papers with statistically reliable results, such that null results are rarely published. As a result, the papers available to include in meta-analyses likely represent a biased sample of the total evidence, with many null findings locked in the proverbial "file drawer." Simonsohn et al. (2014) recently developed a method to determine whether the p-values in the reported literature are distributed in such a way that implies publication bias (e.g., many results close to the .05 cutoff), and this may be a better way to conduct future meta-analyses.

Table 2. Effect sizes for the impact of PIF on various factors. Table adapted from Steblay, Wells, & Douglass (2014). * denotes significant differences between the PIF and no PIF conditions.

Dependent Measure	Culprit	*k*	*D*
Certainty	Present	8	.47*
	Absent	19	.98*
Willingness	Present	7	.54*
	Absent	17	.98*
Basis	Present	7	.54*
	Absent	17	.90*
Ease	Present	7	.45*
	Absent	17	.86*
Memory Clarity	Present	6	.40*
	Absent	15	.69*
Trust	Present	1	.47*
	Absent	5	.69*
Facial Details	Present	7	.44*
	Absent	17	.65*
View	Present	7	.49*
	Absent	19	.58*
Time to ID	Present	7	0.10
	Absent	17	.54*
Memory for Faces	Present	7	.18*
	Absent	16	.52*
Attention	Present	7	.29*
	Absent	18	.48*
Viewing Times	Present	2	0.16
	Absent	7	0.04
Viewing Distance	Present	3	0.20
	Absent	10	0.00

Within the PIF literature, there have also been attempts to reduce or eliminate the harmful effect of these suggestive statements. However, to date, these attempts have produced mixed results. Some of these attempts include obtaining confidence *before* providing the feedback (also known as confidence prophylactic). For example, some suggest obtaining the witness's confidence statement before the lineup administrator provides any feedback to the witness (although ideally, no feedback should be provided at any point by the administrator; Wells &

Bradfield, 1998). The confidence prophylactic did appear to reduce the impact of PIF for some of the aforementioned retrospective judgments (i.e., view, basis for identification, willingness to testify, and the trust they would place in another witness with the same viewing conditions; Wells & Bradfield, 1998), but did not negate the impact of PIF following a one-week delay between participants providing their confidence ratings and receiving the feedback (Neuschatz et al., 2007). Additionally, asking witnesses to consider their retrospective judgments (e.g., view, attention, basis, etc.) individually reduced the effects of feedback, but only when participants considered these factors *before* receiving the feedback. When feedback was presented first, the effects of PIF were still observed (Wells & Bradfield, 1998).

Elicitation of suspicion has been another way that researchers have sought to reduce the harmful effects of PIF. Neuschatz et al. (2007) found that prompting suspicion pertaining to the investigator providing feedback did negate the impact of PIF and this pattern was observed even *after* a week delay between the PIF and retrospective judgments. In this study, participants were told that the person who provided the confirming feedback worked for a District Attorney's office to try prove the accuracy of eyewitness testimony and that this person told everyone they were accurate. Participants who received this additional information did not show the typical boost in retrospective judgments, suggesting that having witnesses wary of potential motivations may be a promising avenue in reducing the harmful effects of PIF.

Finally, a reasonable and important question is whether it is possible to warn witnesses about feedback to reduce its impact on retrospective judgments. In a series of four studies, Lampinen et al. (2007) found mixed results with regard to being able to successfully warn witnesses about relying on confirming feedback. In Experiments 1 and 2, the researchers tested this notion conceptually: Prior to providing retrospective judgments, participants were told that

all feedback was randomly generated, not an indication of their performance, and should be discarded. Participants were then asked to provide their retrospective judgments pertaining to their witnessing experience. In these studies, participants did not demonstrate the typical feedback effects, suggesting that witnesses are able to discount the unreliable feedback to some extent. However, Experiments 3 and 4 did not observe a similar pattern with regard to participants discrediting the feedback. The goal of Experiments 3 and 4 was to move this warning to a more forensically relevant format and participants were told that in a real court case they would be instructed to disregard any additional feedback they received and to rely solely on their own memory for the event. With these instructions, participants who received the PIF reported better witnessing conditions than those who did not receive PIF, the common PIF effect. Lampinen et al. interpreted these findings as demonstrating that warnings could work, in theory, but do not readily translate to more realistic scenarios. Of interest, and lacking from the literature, is the impact of warning witnesses explicitly about what PIF can do to their retrospective judgments. In sum, the current literature on PIF has remained consistent over the years demonstrating it to be a robust and harmful phenomenon when considering eyewitness credibility.

Theoretical mechanisms

The theoretical mechanisms underlying PIF have been of interest to psycholegal researchers trying to better understand the phenomenon. The nature of PIF is interesting because the feedback occurs *after* an identification is provided. Therefore, PIF statements do not impact witness *accuracy,* but rather their assessments of their identification process. In their original study, Wells and Bradfield (1998) proposed that the PIF effect occurred because, when witnesses observe a crime, they do not form online memory traces for the specific retrospective judgments

that will be evaluated later (e.g., the goodness of their view, their level of attention, etc.). So, lacking this information, when witnesses receive PIF suggesting that they were correct, the feedback is used as a suggestive cue permitting the witness to make inferences and provide a response to the retrospective judgments. Specifically, a witness who is told that they identified the correct person may reason "I identified the right person, therefore, I must have had a good view."

Others have argued for a slight variation of this idea. Rather than claim that witnesses do not form the memory traces that would allow them to make these judgments, they suggest that any relevant traces are inaccessible when they are asked to provide retrospective judgments (Neuschatz et al., 2007; Quinlivan, Neuschatz, Douglass, Wells, & Wetmore, 2012; Wells & Bradfield, 1999; Wells & Quinlivan, 2009). This notion became known as the cue-accessibility framework. The cue-accessibility framework has been likened to Bem's (1972) self-perception theory of beliefs and attitudes: When internal cues (i.e., memory for the culprit) are weak, people will draw conclusions about their beliefs and attitudes based on their behavior and contextual information. The cue accessibility account is also supported by evidence confirming the notion that accurate witnesses are less severely impacted by PIF than are inaccurate witnesses (Bradfield, Wells, & Olson, 2002; Steblay, Wells, Douglass, 2014). This is because the cue accessibility theory would predict accurate witnesses are less likely to need to rely on external factors, such as feedback, because they have strong internal cues and would therefore not be as strongly influenced.

While these accounts propose two different states (i.e., not formed at all or formed, but inaccessible) pertaining to whether these assessments are formed, they posit a similar explanation regarding how PIF impacts judgments. Specifically, they suggest that witnesses

utilize feedback as a cue because they lack the information to provide a judgment, either due to never having formed this information or by being unable to access it. An alternative account of the PIF effect is known as the self-presentation interpretation (SPI; Wells & Quinlivan, 2009). According to SPI, once the witness obtains the confirming feedback, they feel good that they made the "correct" choice and proceed to try to appear like a good witness. To that end, they will try to present themselves as having been certain all along, paid excellent attention, etc. However, the SPI has not been well supported within the literature (Wells & Quinlivan, 2009). For example, studies that have participants provide their retrospective judgments *before* receiving feedback reduced the negative impact of PIF (Wells & Bradfield, 1999). Specifically, forcing witnesses to create trace information pertaining to those retrospective judgments prior to the feedback prevented the witnesses from having to rely on the feedback to provide their responses. This finding was used to argue against the SPI on the basis that having formed these initial judgments before the feedback should not reduce the effects of PIF if participants were merely trying to present themselves positively. Specifically, because the judgments made prior to the feedback are private to the witness and not reported to the experimenter, they would still be able to present themselves as confident, attentive, etc. These findings are in contrast to what would be expected from the SPI explanation of PIF, while they support the cue-accessibility account.

The cue-accessibility account was later further expanded upon by Charman, Carlucci, Vallano, and Gregory (2010). In this updated version, termed the Selective Cue Integration Framework (SCIF), the explanation behind the PIF effect was expanded to be a three-stage process pertaining to how witnesses provide retrospective judgments (see Figure 1). The SCIF proposes that when witnesses are asked questions regarding their retrospective judgments, they first elaborate any internal cues that are available to them (e.g., strong memory for the event).

This is referred to as the assessment stage. Should there be no strong internal cues on which to rely, witnesses proceed to the second stage, known as the search stage. In this stage, witnesses will look for and attempt to incorporate any external cues (e.g., feedback from an administrator). If this search yields external cues, the witness will then assess how reliable or credible these external cues might be (evaluation stage). If the cues are perceived to be reliable, then the external cue is used to provide the retrospective judgments. This account is better prepared to explain some of the findings pertaining to how feedback can be negated through suspicion (Neuschatz et al., 2007) or through warnings (Lampinen et al., 2007; Exp. 1 & 2). The SCIF is typically viewed as an extension of the cue accessibility account.

The SCIF could be likened to the cue-utilization view of metacognition (Koriat, 1997). This view incorporates two important components: *cue validity* and *cue utilization.* The former is the notion that there are certain features that are diagnostic indicators of accuracy in memory performance (e.g., answers that come to mind more quickly are more likely to be accurate, Koriat, Ma'ayan, & Nussinson, 2006). The latter pertains to the heuristics that individuals may rely on to decide whether to incorporate the cue validity into retrospective judgments like confidence (e.g., response times are incorporated into the subjective confidence ratings regarding how accurate individuals believe that they are; Kelley & Lindsay, 1993; Koriat, 2008). This approach suggests that there is an inferential approach to metacognitive judgments like feeling of knowing (FOK), judgments of learning (JOLs), and subjective confidence (Koriat, 1997). Specifically, when participants make these judgments, they are not necessarily relying only on memory strength, but may also be relying on additional cues to produce these judgments, such as how difficult the items were to study (Arbuckle & Cuddy, 1969), the type of test that will be

used (Mazzoni & Cornoldi, 1993), or beliefs about their own memory performance (Hertzog, Dixon, & Hultsch, 1990).

The similarities between the SCIF and the cue utilization approach are that both accounts suggest that individuals have information that could be diagnostic indicators of memory (assessment stage and cue validity respectively) and that individuals make decisions regarding whether those cues will be used (evaluation stage and cue utilization). There are some contrasts between these two approaches. First, the SCIF appears to rely more on the notion that the memory (strong or weak) witnesses have are the internal cues whereas the cue utilization approach includes a broader array of information such as fluency or response times. The SCIF also treats the search and evaluation stages as two separate processes with differential predictions about what sort of information is likely to be used in each stage. The SCIF also appears to incorporate a flavor of confirmation bias, where it makes predictions that disconfirming pieces of information are more likely to be ignored during earlier stages (i.e., assessment and search stages) and incorporated in the last stage of evaluation. Alternatively, the cue accessibility approach appears to focus more on whether the cue is incorporated at all.

The SCIF can also be compared to Whittlesea's (1997) Selective Construction And Preservation of Experiences (SCAPE) framework (Whittlesea & Leboe, 2000). This account suggests that accessing memories involve two distinct stages: production and evaluation. Production refers to production of mental states, where images or ideas are brought to mind. This is done when some sort of perceptual input is presented (e.g., a retrieval cue), which then produces information. For example, seeing a face might result in producing the name (Goldinger & Hansen, 2005; Neisser, 1967). Evaluation is the process by which people monitor their production functions based on their subjective states of mind. For example, it is easy to

recognize a colleague who is encountered daily. Seeing that colleague in the hall will result in automatic recognition and the evaluation process will not be aroused. Alternatively, if the colleague has recently had a haircut that differs from their normal style, the production process will immediately recognize your colleague, but the evaluation process will acknowledge the dysfluency that something is different. This triggering of the evaluation process then results in a search to identify the discrepancy. Within SCAPE, the discordant feelings are a result of what Whittlesea et al. termed the discrepancy-attribution process (Whittlesea & Williams, 2001). Specifically, people have expectations regarding how perceptual inputs should be processed, and when those assumptions are violated (e.g., "something about my colleague looks different"), the individual tries to identify the discrepancy (e.g., the haircut).

This framework posits that the production and evaluation stages are two automatic processes that occur when individuals encounter perceptual stimuli. Specifically, these processes *both* contribute to memory for items. This account differs from what is proposed by the SCIF framework by suggesting that the subjective states (i.e., confidence) is actually a component of the memory trace (or the internal cue). Where SCIF indicates that confidence in an identification is made by making inferences about the witness's memory and any external cues, SCAPE posits that this could be information that the witness already possesses as part of their internal cue. Additionally, the SCAPE framework posits that both the production and evaluation stage are automatic whereas the search and evaluation stages in the SCIF are described as being a more effort-driven evaluation of external cues that happen in discrete stages.

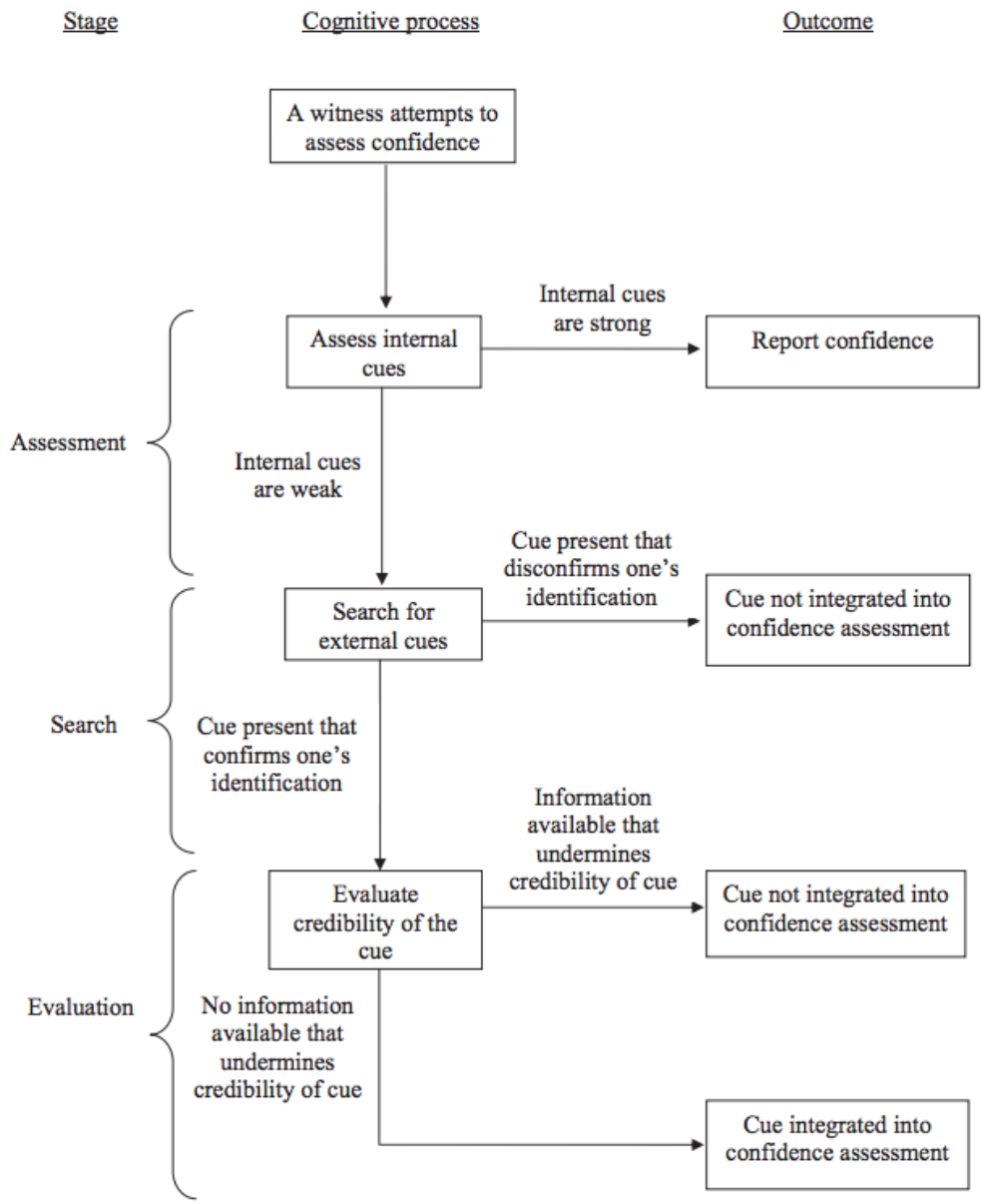

Figure 1. The Selective Cue Integration Framework. Taken from Charman, Carlucci, Vallano, and Hyman (2010).

Charman et al. (2010) argued that there are testable theoretical claims and examined factors that impact the stages differentially. Specifically, the SCIF argues that confidence can be inflated, but not deflated, in the search stage, whereas the opposite should occur in the evaluation stage. The basis for these assumptions stem from the attitude change literature where research

has shown that people are biased to initially search for evidence that supports instead of challenges their preexisting beliefs (i.e., the confirmation bias; Nickerson, 1988) and that people are more likely to initially scrutinize disconfirming information to exclude it (Anderson, Lepper, & Ross; 1980; Edwards & Smith, 1996; Lord, Ross, & Lepper, 1979). Alternatively, in the evaluation stage, any additional confirming evidence should not heavily impact confidence because these confirming cues would have already been incorporated at the search stage.

Charman et al. tested this notion in a series of three studies. In Experiment 1a and b, Charman et al. argued the data supported these claims because having disconfirming feedback presented alone did not reduce participants' confidence in their identification decision but did reduce confidence when the confirming feedback was recanted at a later time (the experimenter originally provided confirming feedback but then returned a minute later and said "Actually I made a mistake. You identified the wrong person. The suspect was someone else"). Their interpretation was that, when the disconfirming feedback was presented immediately after witnesses made their identification, they were still in the search stage, and the feedback was discounted. However, when it was presented later (via a recantation of the confirming feedback after a minute delay), that this was during participants' evaluation stage and, thus, was taken into account, which resulted in witnesses no longer feeling as confident in their identification. These findings demonstrate that information is used differently depending on what stage of the SCIF participants were in. Experiment 2 tested the predictive ability of the SCIF using a cowitness paradigm. Participants heard cowitnesses either identify the same or a different lineup member with either high or low confidence. In line with what would be predicted by the SCIF, hearing a cowitness identify the same suspect resulted in higher confidence (confirming information at the search stage) whereas hearing a cowitness identify someone else did not impact participants'

confidence (disconfirming information at the search stage). However, when the cowitness identified the same suspect, if they did so with low levels of confidence, the confidence inflation was eliminated (disconfirming information at the evaluation stage) whereas high confidence, same suspect identifications did not further inflate confidence (confirming information at the evaluation stage). In Experiment 2, the confidence level manipulation served as a way for participants to assess the credibility of the cowitness's identification, which occurs in the evaluation stage. The authors concluded that SCIF was a viable theoretical framework when examining PIF.

Jurors and PIF

The extant literature on PIF illustrates a phenomenon that appears to be very robust and one that is not easily ameliorated. However, perhaps trying to address the harmful nature of PIF at the witness level is not the most efficient way to solve this problem. Rather, it is possible that jurors may be able to recognize these suggestive statements and, when such a statement is present, incorporate that into their assessment of the eyewitness. Unfortunately, research has shown that judges, jurors, and lawyers are largely unaware of factors that can impair eyewitness identifications (Lindsay, 1994; Wise & Safer, 2004). Additionally, Kassin and Barndollar (1992) revealed that laypeople, who were jury-eligible college students, largely were not privy to factors that impact eyewitness accuracy, demonstrating that this information is not necessarily intuitive for triers of fact. This is important to note because if potential jurors cannot appreciate the more fundamental aspects of what can contribute to an inaccurate identification, it is also likely the case that they may not appreciate the more subtle factors that can influence witness testimony. The recommendations for obtaining eyewitness evidence already encourage audio or video recording of the entire identification procedure in an attempt to maintain evidence integrity

(Cory, 2001; Kassin, 1998, Technical Working Group, 1999). Furthermore, there are some police forces that are using such approaches when working with eyewitnesses (Beaudry & Lindsay, 2006; New South Wales Police Force, 2012; North Carolina General Assembly, 2007; Police & Criminal Evidence Act, 2011).

A relatively recent suggestion is that videotaped identifications may be able to aid jurors when assessing eyewitness identifications (Steblay, Wells, & Douglass, 2014; Wilford & Wells, 2013). To date, relatively little research has explored how providing this recorded information to mock-jurors impacts their evaluations of witnesses and evaluations of guilt. Reardon and Fisher (2011) showed that providing video evidence can aid in jury decision-making: The inclusion of a video helped participants more accurately discriminate between accurate and inaccurate witnesses. Additionally, Douglass and Jones (2013) found that showing participants videotaped, as opposed to written, witness evidence allowed participants to recognize when confidence varied between the time of identification and the trial. As a result, participants who saw the videotaped testimony where the witness was more confident at trial than they were at the original identification rated the witness as being less credible, less accurate, and less consistent, while also lowering their ratings of defendant guilt relative to when these statements were read. Importantly, the witnesses in this study were actors so there were no accurate and inaccurate witnesses. Although this suggests that participants in the study are gleaning additional information from the video that could be diagnostic of how reliable a witness is, the study notably did not include a 'no additional information' condition to allow for full comparisons between video, written, and a baseline control condition. An equally promising finding observed by Modjadidi and Kovera (2018) who found that participants who saw videotaped identifications and were warned about the dangers of single blind procedures were more sensitive in

recognizing when lineups were conducted in a single blind manner. Furthermore, participants were also less likely to convict a defendant under those circumstances. Similar to Douglass & Jones (2013) the witnesses were actors so there were no accurate or inaccurate identifications. The focus of both the Douglass and Jones as well as the Modjadidi and Kovera studies were to assess whether mock jurors could be sensitized to these issues and recognize them when they were present in the eyewitness identification videos.

However, not all studies examining videotaped identifications are so optimistic pertaining to jurors. An early study conducted by Kassin, Rigby, and Castillo (1991) examined whether viewing an eyewitness's videotaped identification resulted in more accurate mock-juror evaluations than showing the witness's testimony. Regardless of what type of witness evidence participants saw and witnesses' overall accuracy, participants believed the witness 42% of the time, suggesting that discriminating between accurate and inaccurate witnesses is a daunting task. Additionally, Beaudry et al. (2015) reported evidence suggesting these video recordings may be less promising than previously thought. This study tested multiple factors that pertain to eyewitness identifications. This included the type of lineup, identification accuracy, lineup administration, and the type of evidence that participants saw (the identification, the witness testimony, or both the identification and testimony). Largely, their results showed that, overall, participants were more willing than not to believe eyewitness evidence. Their results found that participants were least likely to believe the witness when they only viewed their identification in isolation (i.e., without their testimony). In contrast to Reardon and Fisher (2011), participants were not able to differentiate between accurate and inaccurate witnesses. Similarly, participants in this study were not able to differentiate between single and double blind procedures as was seen in Modjadidi and Kovera (2018), although this could have been because the latter included

an expert witness who testified about the perils of single blind studies. Most importantly, with regard to the current study, Beaudry et al. found that witnesses who received PIF were more likely to be believed than those who did not receive the PIF (a finding also shown in Quinlivan, Cash, Jenkins, & Le Grand, in prep). From this, there is evidence that not only can PIF be deleterious to a witness's retrospective judgments, but that this can also tarnish triers of facts' interpretations of witness accuracy. Seemingly, in both Beaudry et al. and Quinlivan et al., the inclusion of PIF appears to act as confirmation to the mock-juror that the witness was correct. This was highlighted further by Quinlivan, Cash, Jenkins and Le Grand (in prep), who examined mock-jurors' perceptions about how good of a view the witness had, how good of a basis they had to make an identification, etc. They found that each evaluative judgment increased when they saw the witness receive PIF. Both studies caution the recommendation of showing videotaped identification procedures to jurors without more research to further investigate the secondary transfer of PIF to mock-jurors.

Cause for concern?

Psycholegal researchers would agree that the PIF effect is a well-documented phenomenon and there is a substantial amount of evidence regarding how PIF can impact witnesses' retrospective judgments. Unfortunately, this is not true for its impact on jurors. As noted earlier, only two studies have explored the impact of PIF on jury decision-making and both studied the impact of videotaped procedures. While these provide promising initial insight into the effects of PIF on jury decision-making, there is still much that is lacking with regard to how these suggestive statements might be interpreted by jurors. For example, it is unknown whether the effects of PIF on jurors are only limited to their perceptions of the witness. Could it also be possible that the effect of PIF may extend even more broadly than currently thought?

Specifically, does the presentation of PIF also lend itself to "PIF by proxy" for jurors who see the witness receive these suggestive statements? For example, a juror who sees the witness receive confirming feedback may think the witness was paying great attention, had a great basis for their identification and then may also believe that they, as a juror, were paying great attention to the trial, had a great basis for their verdict decision, etc. In this instance, the legal system may end up with not only overconfident witnesses, but also overconfident jurors, regardless of their overall accuracy.

Current Studies

The goal of the current studies was to further examine the effects of PIF on mock-jurors. Although PIF is difficult to eliminate in witnesses, sensitizing jurors to its harmful effects on witnesses' confidence could serve as a way to encourage more accurate verdict decisions. However, Beaudry et al. (2015) and Quinlivan et al. (in prep) appear to suggest that jurors seemingly view PIF as confirmation that a witness is correct. Are there broader implications that this feedback provides to jurors, such as their overall performance as a jury member?

Across two experiments, I evaluated the effect of PIF on mock-jurors' perceptions of an eyewitness, as well as their perceptions of their own performance as a juror (Exp. 1). I also explored whether mock-jurors can recognize PIF and incorporate its potential danger to witness testimony when warned about the harmful effects of PIF (Exp. 2). Experiment 1 served as a conceptual replication to Beaudry et al. (2015) and Quinlivan et al. (in prep) by testing whether there is a transfer of PIF from witnesses to jurors that alters the jurors' perceptions about the witness's viewing experience. As an extension, participants also provided ratings about their own juror experience. These questions were modeled after those that are typically asked about eyewitnesses to assess whether the PIF effect expands even more broadly than what is currently thought. Experiment 2 explored whether mock-jurors are able to recognize and weight testimony that has been tainted by PIF less heavily than if no PIF had been provided. If it is the case that a simple warning can eliminate the effects of PIF on witness perceptions, then perhaps any dangers of PIF to jurors are not so detrimental.

I predicted that the PIF effect would not only impact perceptions of the witness, but would also extend to mock-jurors perceptions about their own juror experience. Furthermore, I

predicted that when jurors were made aware of the dangers of PIF, they would find the witness less credible than when no warning was presented.

Experiment 1

The goal of Experiment 1 was to further researchers' understanding of the parameters of PIF and its impact on jurors. To do this, Experiment 1 served as a conceptual replication and extension of both Beaudry et al. (2015) and Quinlivan et al. (in prep). I manipulated the presence of PIF in eyewitness identification videos that participants viewed. Participants rated factors assessing the witnessing experience of the witness as well as factors that assessed their own mock-juror experience.

Method

Participants

To determine the number of participants needed for Experiment 1, a power analysis was conducted. Using an alpha of .05, a beta of .8, looking for a medium effect size of $D = .5$ with an independent samples t-test, the required number of participants was 102 with 51 participants per condition. Because I had two versions of both the PIF and no PIF videos, I doubled this sample size. A total of 200 participants were recruited to participate in Experiment 1 ($n_{PIF1} = 49$, $n_{PIF2} = 51$, $n_{NoPIF1} = 50$, $n_{NoPIF2} = 50$). Participants were recruited from Amazon Mechanical Turk and were required to have an approval rating of at least 98% as well as having had completed at least 1000 HITs (Human Intelligence Tasks) prior to participating in the study. The mean age for participants in this study was 35.95 years ($SD = 11.18$). Of the participants who took part in the study, 63% identified as male and 76% identified as Caucasian. The IRB approval for this study can be seen in Appendix A.

Stimuli

Videos. The stimuli were four eyewitness identification videos. These videos were filmed with actors acting as witnesses and law enforcement. There were two videos that contained PIF,

while the other two did not. The videos depicted the law enforcement officer reading unbiased lineup instructions (i.e., the person may or may not be in the lineup), before the witness selected a suspect from the lineup. Please see Appendix B for the scripts for all four videos. The videos for the current study can be seen here: https://osf.io/fxhv4/?view_only=0879aee1a2934fa5b71a81ad6f6710a9. Witness-actors were instructed to pause for five seconds before providing their identification (e.g., "Number 4") without any additional verbal or non-verbal cues. For the PIF videos, the law enforcement officer provided confirmatory feedback such as "Great job. That is exactly who we thought it was too." The PIF absent videos ended after the witness made their identification choice and were signing their identification sheet. The purpose of having two PIF and two no PIF videos was to serve as a replication attempt. The two PIF and no PIF videos conveyed the same information, but with minor changes between the two scripts. All videos were between 35-38 seconds in length. In an attempt to keep the level of emotion consistent between the PIF and No PIF videos, both actors were instructed to speak with a neutral tone throughout the interaction, including when delivering and receiving the PIF. Participants in Experiment 1 were randomly assigned to view only one of the four videos. The videos in the current study were normed by asking a sample of 79 Amazon Mechanical Turk workers to answer three questions about the videos. They were asked to rate the quality of the video, the quality of the audio, and they were asked to guess the hypothesis of the study (i.e., *does the PIF statement impact perceptions of the eyewitness?).* Participants in the norming study only viewed one of the four videos. The audio and video quality questions were rated on a 1-6 Likert scale with 1 representing terrible quality and 6 representing terrific quality. The average ratings for all videos on these two questions were 4.03 (*SD* = 1.25) and 4.70 (*SD* = 1.02) respectively. More importantly, none of the participants were

able to guess the goal of the experiment, suggesting that the PIF statements were not obvious enough to produce demand characteristics.

Retrospective judgments. Participants provided two sets of retrospective judgment ratings per video. The first set were judgments about the witness and the second set were judgments about participants' own juror experience. The judgments regarding the witness were an adapted version of those used in Quinlivan, Cash, Jenkins, and Le Grand (in prep). Similar to Quinlivan et al. these judgments were provided on 11-point Likert scales. Additionally, these judgments were then adapted to form parallel versions in which participants could assess perceptions about their performance as a mock-juror. These questions can be seen in Appendix C.

Design

Experiment 1 was a single factor, between subjects design that manipulated the presence of PIF (present, not present). The primary factors of interest were participants' perceptions of the witness as well as their perceptions about their own juror experience.

Procedure

The survey was conducted on Qualtrics and distributed through Amazon Mechanical Turk. Participants began the study by first providing consent. After consent was obtained, they were instructed that they would act as a juror who is being asked to consider the testimony from an eyewitness. They were told to pay attention because they would be asked questions about the witness. After indicating that they understand the instructions, participants were then randomly shown one of the four witness videos. After viewing the witness video, participants were asked to provide the retrospective judgments for both themselves and the witnesses. These questions

can be seen in Appendix C. At this point, participants provided demographic information, before being thanked and debriefed.

Results

The goal of Experiment 1 was to assess whether the presence of PIF not only impacts perceptions about the witness, but also whether it impacts perceptions on mock-jurors' experiences. I conducted two sets of overall analyses: the perceptions of the witness and perceptions of the juror. It is important to note that both the perceptions of the witness and perceptions of the juror pertain to multiple judgments as seen in Appendix A. However, the prediction is that PIF (or lack of) will influence these judgments similarly. To correct for multiple comparisons, the alpha for all univariate analyses was adjusted using a Bonferroni correction and is set at .0028. A multivariate analysis was conducted to look for outliers in the data set. The analysis revealed 10 participants who differed significantly from the overall pattern of data, however, their inclusion did not impact the pattern of the results so their data are included in the following analyses. Before conducting any of the primary analyses, I tested to see if there were any differences between the two PIF videos as well as the two No PIF videos. There were no significant differences between these two sets of videos on the dependent variables of interest so I have collapsed across the versions 1 and 2 for the PIF and No PIF videos. The result of this is that the comparisons will now be between the PIF and No PIF videos.

Does PIF impact perceptions about the witness?

To examine whether the presence of PIF impacted jurors' perceptions of an eyewitness, I conducted independent samples t-tests for each of the nine factors participants provided ratings

for. With the alpha corrections for multiple comparisons, none of the analyses were significant (all *p*s > .03). Specifically, the presence or absence of PIF did not alter participants' ratings between the two conditions. The means, *t*, and *p* values can be seen in Table 3. To investigate whether these data represent true null results, I repeated the frequentist analyses using Bayesian t-tests, which provide evidence in favor of the alternative (BF10) or null (BF01) hypotheses (these values can be seen in Table 3 and Figure 2). These values were calculated using JASP (JASP Team, 2018). The Bayes Factor that is produced can be interpreted as an odds ratio, expressing whether one hypothesis (either the null or alternative) is more likely than the other. When interpreting the strength of evidence from these values, Jeffreys (1961) suggests that odds smaller than 3:1 should be viewed as anecdotal evidence, odds that fall between 3:1 and 10:1 should be viewed as strong evidence, and odds that are greater than 10:1 should be seen as substantial evidence. I have also included odds ratio markers for 2:1, 3:1, and 7:1 odds. These numbers are arbitrary, but serve to help interpret the data. As can be seen in the figure, most of the Bayes factor values appear to provide at least anecdotal (i.e., willingness to testify and how much should the witness be trusted) or substantial evidence (i.e., certainty, basis, ease, details, duration, and distance) for the null hypothesis. The single exception to this trend is that attention appears to provide anecdotal evidence in favor of the alternate hypothesis.

Table 3. The means, standard deviations, t values, p values and Bayes factor values for all dependent variables and comparisons pertaining to judgments about the witness.

	PIF Presence				
Factor	**PIF**	**No PIF**	***t***	***p***	**BF10**
Certainty	7.30 (2.32)	7.48 (2.20)	-0.56	0.57	0.18
Testify	9.02 (1.91)	8.55 (2.41)	1.53	0.13	0.46
Basis	7.19 (2.14)	7.39 (2.18)	-0.65	0.51	0.19
Ease	7.31 (2.49)	7.03 (2.40)	0.81	0.42	0.21
Trust	6.96 (2.53)	7.54 (2.35)	-1.68	0.10	0.57
Attention	7.72 (2.29)	8.32 (1.73)	-2.09	0.04	1.17
Details	6.90 (2.50)	7.15 (2.28)	-0.74	0.46	0.20
Duration	5.59 (2.28)	5.90 (2.25)	-0.97	0.34	0.24
Distance	6.99 (1.81)	7.02 (1.77)	-0.12	0.91	0.16

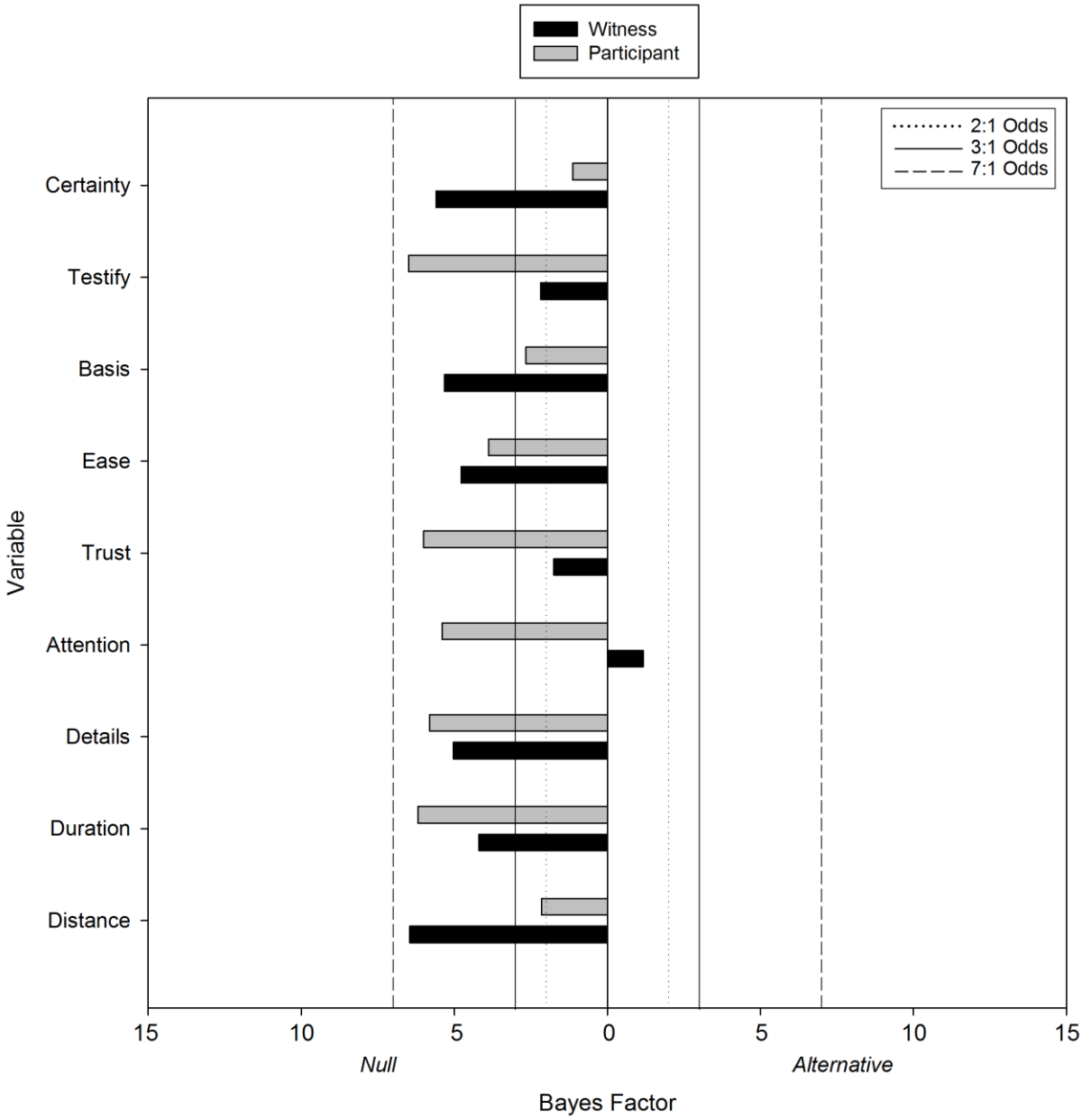

Figure 2. The Bayes factors for all dependent variables. Dark and light bars are indicative of values for the witness and participant respectively. Additionally, the three cutoff lines for 2:1, 3:1, 7:1 odds are also shown.

Does PIF impact perceptions about the jurors' experience?

To test whether PIF may be even more influential than encompassed by previous studies, I conducted independent samples t-tests on participants' ratings of their juror experience as a function of the presence of PIF. Similar to the ratings of the witness, there were no significant

differences on any of the dependent variables as a function of the presence or absence of PIF (all *ps* > .05). The means, *t*, and *p* values can be seen in Table 4. I also conducted Bayes factor values for each factor similar to perceptions of the witness, the Bayes factor values provide either anecdotal (i.e., certainty, basis, and distance) or substantial evidence (i.e., testify, ease, trust, attention, details, and duration) for the null hypothesis. These values can be seen in Figure 2.

Table 4. The means, standard deviations, t values, p values and Bayes factor values for all dependent variables and comparisons pertaining to judgments about the participants' own performance.

	PIF Presence				
Factor	**PIF**	**No PIF**	***t***	***p***	**BF10**
Certainty	7.09 (2.97)	7.85 (2.57)	-1.94	0.05	0.88
Testify	7.81 (2.95)	7.79 (2.96)	0.05	0.96	0.15
Basis	6.45 (2.79)	7.00 (2.84)	-1.38	0.17	0.38
Ease	7.25 (2.71)	7.65 (2.67)	-1.05	0.30	0.26
Trust	7.26 (3.03)	7.43 (2.79)	-0.41	0.68	0.17
Attention	10.33 (1.44)	10.45 (1.25)	-0.63	0.53	0.19
Details	9.07 (1.81)	9.19 (1.65)	-0.49	0.63	0.17
Duration	3.29 (1.91)	3.38 (2.07)	-0.32	0.75	0.16
Distance	8.72 (1.82)	8.31 (1.95)	1.54	0.13	0.46

Discussion

The results from Experiment 1 failed to support the proposed hypotheses that predicted a PIF effect on both jurors' perceptions of the witness as well as of their own performance. These data differ from Beaudry et al. (2015) and Quinlivan et al. (in prep) who observed participants' viewing the PIF as confirmatory evidence that the witness was correct in their identification. In fact, the only analysis that achieved traditional significance was that of participants' assessments of how much attention the witness had paid and this was seen in the opposite direction as what would be predicted by the PIF literature (Quinlivan et al., in prep; Steblay, Wells, & Douglass,

2014). Specifically, participants in the no PIF condition rated the witness as having paid more attention than participants in the PIF condition.

Despite not finding the traditional PIF effect, the Bayes factor analyses provide helpful insight into interpreting the null findings. Specifically, many of the analyses revealed substantial support for the null hypothesis. Furthermore, this null effect is somewhat impressive as it is fairly consistent across the multitude of dependent variables and consistent across versions 1 and 2 of the PIF and no PIF videos; demonstrating that these findings are replicable within the study. The benefit to knowing whether these findings are truly indicative of a lack of effect is that it suggests there may not be a negative effect of showing jurors recorded eyewitness procedures as there does not appear to be a difference in perceptions of the witness as a function of the presence of PIF.

Experiment 2

The goal of Experiment 1 was to enhance understanding of the impact of PIF, both for perceptions of witnesses as well as jurors' metacognitive assessments of their own performance. Although I failed to find the typical PIF effect, it is still possible to assess whether warning jurors about the suggestive nature of PIF impacts perceptions of an eyewitness. To this end, Experiment 2 furthered this examination by exploring whether warning jurors about PIF can negate its effect on witnesses.

To date, no study has attempted to warn jurors about the harmful effect of PIF and only one study has attempted to do so for witnesses. Lampinen et al. (2007) found mixed results with their attempt to warn participants that they should not incorporate feedback. In Experiments 1 and 2, this warning was presented in the format of faulty feedback, wherein participants were told that any feedback received was randomly generated and, therefore, could not be trusted. Under these circumstances, the researchers were able to eliminate the PIF effect. However, their more applied approach was not so promising. In Experiments 3 and 4, participants were told that, were this a true trial, they would be asked to discount any additional feedback and to discard their previously heard feedback. In these studies, participants still appeared to incorporate the PIF, because their judgments about their witnessing experience were significantly higher when the PIF was present than when it was not. A potential argument to these findings could be that the nature of the warning did not compel witnesses to discount that feedback. In other words, although the witness was told to ignore the information, there was no reason to suspect that the feedback was inaccurate, so they may have ignored this warning. Evidence supporting this assumption is seen in Neuschatz et al. (2007) where suspicion regarding the experimenter's motives eliminated the PIF effect.

Potentially, a more compelling forensic warning that suggests these statements may make witnesses appear more credible than they truly are might eliminate the PIF effect. One way that is commonly used in court cases to provide information to members of the jury is to enlist an expert witness. Research has shown that jurors have difficulty understanding scientific evidence (Thompson & Schumann, 1987) and that having an expert witness for eyewitness evidence makes both real and mock-jurors more sensitive to issues regarding eyewitness testimony (Cutler, Dexter, & Penrod, 1989; Hosch, Beck, & McIntyre, 1980; Loftus, 1980). It is possible that having a manipulation that provides similar information to an expert witness may result in jurors being more wary of the witnesses who receive the PIF. To this end, the goal of Experiment 2 is to examine whether warning participants about PIF can negate the harmful impact of PIF in jurors' assessments.

Method

Participants

To determine the number of participants needed for Experiment 2, a power analysis was conducted. Using an alpha of .05, a beta of .8, looking for a medium effect size of $D = .5$ with a between factors ANOVA, the required number of participants was 104. I elected to nearly double this again to account for using the two PIF and no PIF videos. A total of 200 participants were recruited to participate in Experiment 2 and randomly assigned to one of the eight video x warning conditions. For the warning conditions the means were $n_{PIF1} = 25$, $n_{PIF2} = 25$, $n_{NoPIF1} = 25$, $n_{NoPIF2} = 26$. For the no warning conditions the means were $n_{PIF1} = 24$, $n_{PIF2} = 24$, $n_{NoPIF1} = 26$, $n_{NoPIF2} = 25$. Participants were again recruited from Amazon Mechanical Turk and were required to have the same qualification criteria as Experiment 1 to participate (i.e., have an approval rating of at least 98% and having at least 1000 HITs completed). Participants who had

completed Experiment 1 were ineligible to participate in Experiment 2. The mean age for participants in this study was 39.06 years (*SD* = 12.50). Of the participants who took part in the study, 57.5% identified as male and 69.5% identified as being Caucasian.

Stimuli

Videos. The stimuli for the Experiment 2 were the same four videos that were used in Experiment 1.

Warning. The warning participants received was presented to only participants in the warning condition after they viewed the video and prior to making their retrospective judgments. The warning can be seen in Appendix D.

Retrospective judgments. The retrospective judgments that participants were asked were the same as those used in Experiment 1.

Design

Experiment 2 conformed to a 2 (PIF: present, not present) x 2 (Warning: present, not present) between subjects design. The primary factors of interest were participants' perceptions of the witness and their perceptions about their own juror experience.

Procedure

The study was similar to Experiment 1. The survey was conducted on Qualtrics and distributed through Amazon Mechanical Turk. Participants provided informed consent and received the same instructions as participants in Experiment 1. Participants were randomly assigned at the start of the survey to either the warning present or warning absent condition. Additionally, they were also assigned to view one of the four videos. Participants then viewed their respective eyewitness video. Following this, participants in the warning condition read the warning statement while participants in the warning absent condition simply continued on to the

next stage in the study. All participants provided their retrospective judgments for the video and then completed the demographic information before being debriefed.

Results

The goal of Experiment 2 was to examine whether including a forensically relevant warning made mock-jurors more aware of PIF and how it might impact an eyewitness. Similar to Experiment 1, I focused on participants' assessments of the witness and assessments of themselves. Although there were no differences in Experiment 1 as a result of PIF, it is possible that warning participants about its deleterious effect on eyewitnesses may make participants more skeptical of eyewitnesses who received the PIF statement. I conducted a multivariate analysis to look for outliers in the data and the analysis revealed there were 7 participants who differed significantly from the group. However, similar to Experiment 1, the inclusion of these participants did not alter the overall pattern of the data so all participants are included in the analyses. I again tested for differences between the sets of videos (i.e., PIF 1 and 2 without a warning, PIF 1 and 2 with a warning, No PIF 1 and 2 without a warning, and No PIF 1 and 2 with a warning). There were no significant differences between the sets of videos so I collapsed the data across the two versions of videos for each of the 2 (PIF: present, absent) x 2 (Warning: present, absent) videos. Similar to Experiment 1, the alpha level for the univariate analyses was set at .0028 to correct for multiple comparisons.

Does warning mock-jurors about PIF impact ratings of the witness?

To examine whether warning participants about the harmful effect of PIF on eyewitness testimony, I conducted 2 (PIF: present, absent) x 2 (Warning: present, absent) between subjects ANOVA for each of the judgments pertaining to the eyewitness. Of these analyses, the only comparison to achieve statistical significance was the main effect of warning on participants'

ratings of how long they believed the crime event lasted for the witness (duration), $F(1,196) = 9.40$, $p = .002$, $n^2_p = .046$. Specifically, participants who did not receive the warning rated the encounter as having lasted longer ($M = 6.82$, $SE = .23$) than participants who did receive the warning ($M = 5.30$, $SE = .23$). All other comparisons were not significant. Please see Table 5 for means and Table 6 for *p* and Bayes factor values. The Bayes factor values supported the notion that the main effect of warning on duration provides strong evidence for the alternate hypothesis while also providing moderate evidence for the alternate hypothesis for the main effect of warning on details remembered about the face.

With regard to evidence for the null, when looking at the main effect of PIF, there was substantial evidence for the basis, ease, trust, attention, details and duration variables. All other variables provided moderate support for the null. For the main effect of warning, of the remaining variables (excluding details and duration), there was substantial evidence for the ease, trust, and distance variables while all other factors provided anecdotal evidence. Finally, for the interaction, there was anecdotal evidence for the null in the testify and details variables. All other remaining variables provided substantial evidence for the null hypothesis. The Bayes factor values for the interaction of all dependent variables are shown in Figure 3.

Table 5. The means and standard deviations for all dependent variables about the witness.

	PIF, Warning		PIF, No Warning		No PIF, Warning		No PIF, No Warning	
Factor	***M***	***SD***	***M***	***SD***	***M***	***SD***	***M***	***SD***
Certainty	7.38	2.10	7.73	2.75	6.47	2.63	7.37	2.21
Testify	9.14	1.48	9.17	2.09	8.33	1.79	9.14	1.80
Basis	7.48	1.79	7.75	2.55	7.02	1.97	7.76	2.09
Ease	7.04	1.99	7.40	2.46	6.80	2.11	7.10	2.01
Trust	7.30	1.96	7.46	2.70	6.96	2.30	7.57	2.01
Attention	7.76	1.97	8.19	2.49	7.43	2.23	7.96	2.10
Details	7.08	1.82	7.27	2.54	6.63	2.17	7.71	1.91
Duration	5.46	2.10	6.27	2.62	5.14	2.17	6.29	2.17
Distance	6.72	1.43	7.13	2.25	6.37	1.50	6.51	1.82

Table 6. The p values and Bayes factor values for all comparisons made pertaining to judgments about the witness. *denotes statistical significance for p values.

	Certainty	**Testify**	**Basis**	**Ease**	**Trust**	**Attention**	**Details**	**Duration**	**Distance**
PIF (*p*)	0.07	0.10	0.46	0.38	0.72	0.37	0.98	0.64	0.06
Warning (*p*)	0.07	0.10	0.09	0.29	0.23	0.13	0.04	0.00*	0.28
PIF*Warning (*p*)	0.42	0.13	0.43	0.92	0.48	0.87	0.14	0.59	0.59
PIF (BF)	0.72	0.54	0.20	0.22	0.16	0.22	0.15	0.17	0.84
Warning (BF)	0.70	0.54	0.60	0.26	0.31	0.46	1.30	12.32	0.26
PIF*Warning (BF)	0.28	0.60	0.27	0.20	0.27	0.22	0.51	0.25	0.23

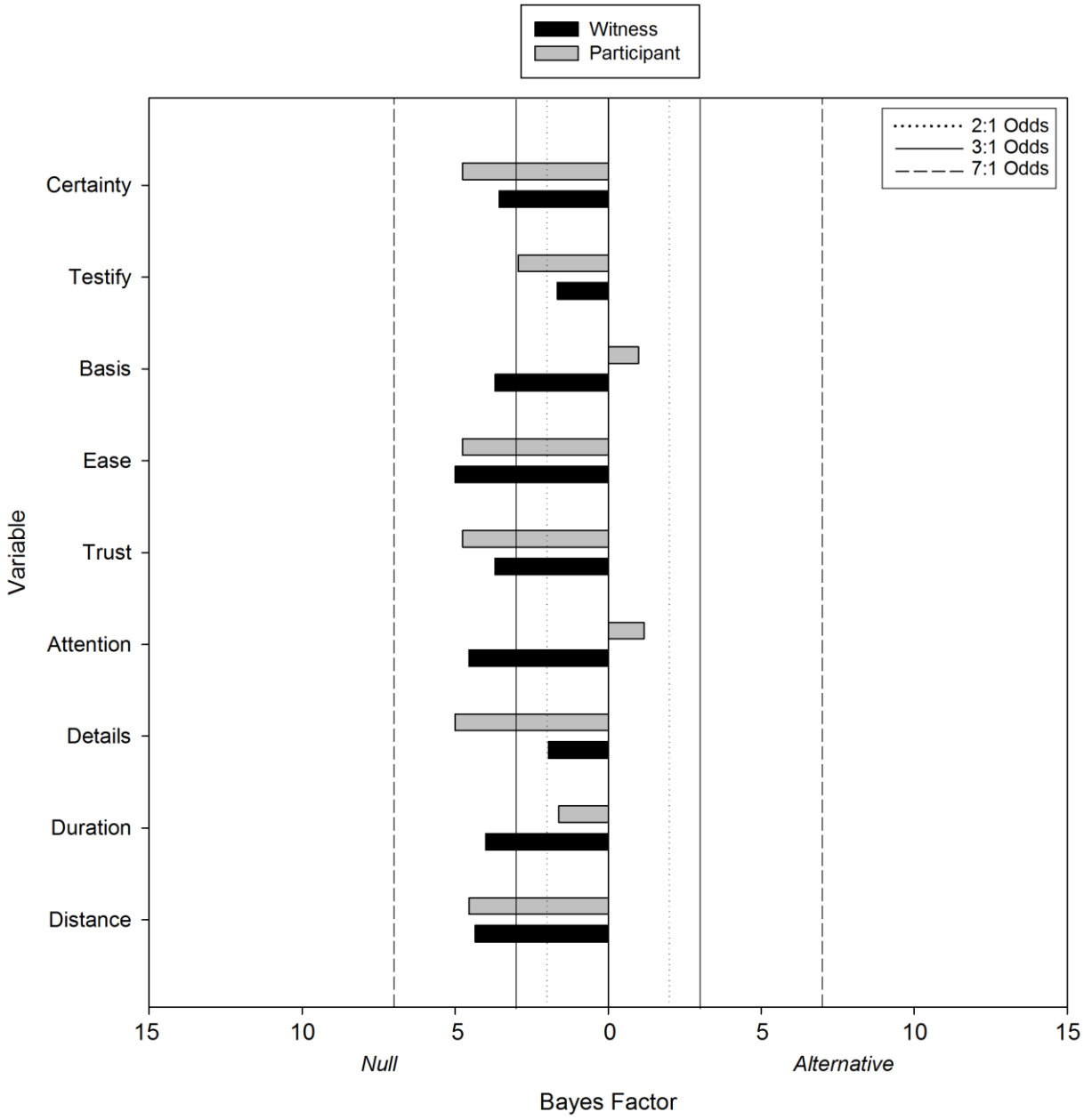

Figure 3. The Bayes factors for the interaction for all dependent variables. Dark and light bars are indicative of values for the witness and participant respectively. Additionally, the three cutoff lines for 2:1, 3:1, and 7:1 odds are also shown.

Does warning mock-jurors about PIF impact their perceptions of their own performance?

To test whether there is an effect of warning on participants' perceptions of their own performance, I conducted 2 (PIF: present, absent) x 2 (Warning: present, absent) between subjects ANOVA for each of the mock-juror judgments. None of the conducted analyses reached significance. See Table 7 for means and Table 8 for *p* and Bayes factor values. With regard to the Bayes factor values, only the main effect of PIF on certainty produced anecdotal evidence for the alternate hypotheses. All other values provided evidence toward the null.

The Bayes factor values for the main effect of PIF revealed anecdotal evidence for the null in the testify, ease, and detail variables while providing substantial evidence for the basis, trust, attention, and distance variables. For the main effect of warning, there was anecdotal evidence for the null in the testify variable, substantial evidence for the certainty, ease, trust, attention, details, duration, and distance variables, and strong evidence for the basis variable. Finally, with regard to the interaction, there was anecdotal evidence for the null in the testify, and duration variables. All other variables provided substantial evidence for the null hypothesis. The Bayes factor values for the interaction of all dependent variables are presented in Figure 3.

Table 7. *The means and standard deviations for all dependent variables pertaining to the mock-jurors.*

	PIF, Warning		PIF, No Warning		No PIF, Warning		No PIF, No Warning	
Factor	***M***	***SD***	***M***	***SD***	***M***	***SD***	***M***	***SD***
Certainty	7.58	2.38	8.13	2.63	6.88	2.96	7.22	2.67
Testify	7.80	2.48	7.98	2.88	6.88	3.12	7.86	2.64
Basis	7.16	2.43	6.92	3.19	6.12	2.88	6.75	2.78
Ease	7.52	2.43	7.79	2.49	6.88	2.97	7.25	2.83
Trust	7.26	2.62	7.40	2.90	7.04	3.02	7.41	2.71
Attention	10.34	1.29	10.63	0.98	10.63	0.96	10.29	1.19
Details	9.40	1.44	9.60	1.61	9.10	1.78	9.22	1.86
Duration	3.56	1.70	3.33	2.45	3.25	1.64	3.94	2.28
Distance	8.42	2.05	8.83	1.88	8.29	2.04	8.51	2.13

Table 8. The *p values and Bayes factor values for all comparisons made pertaining to judgments about the mock-jurors.*

	Certainty	**Testify**	**Basis**	**Ease**	**Trust**	**Attention**	**Details**	**Duration**	**Distance**
PIF (p)	0.04	0.19	0.13	0.13	0.80	0.89	0.15	0.60	0.44
Warning (p)	0.25	0.14	0.63	0.40	0.53	0.88	0.50	0.43	0.28
PIF*Warning (p)	0.78	0.31	0.28	0.90	0.77	0.05	0.86	0.12	0.73
PIF (BF)	1.23	0.34	0.17	0.47	0.16	0.16	0.41	0.17	0.20
Warning (BF)	0.28	0.42	0.08	0.21	0.19	0.16	0.19	0.21	0.27
PIF*Warning (BF)	0.21	0.34	1.00	0.21	0.21	1.17	0.20	0.62	0.22

Does warning mock-jurors make them more likely to recognize PIF?

Although not one of my planned research questions, an interesting question is whether participants who had been warned about PIF were more likely to recognize the suggestive statement when the statement was in fact presented. To examine this question, I conducted a binary logistic regression looking only at participants who received the PIF with warning being the only independent variable. For the purpose of this analysis, participants who did not hear the PIF statement were excluded. Although an examination of the nominal data might suggest there would be a statistically significant difference, this analysis revealed that there was no difference in the rates of which participants in either the warning or no warning condition reported recognizing the PIF statement, $b = .786$, Wald $\chi^2 (1) = 2.86$, $p = .09$. The lack of significant difference between the two warning groups is most likely a result of a ceiling effect in both conditions where participants generally were likely to report having heard the officer say something that would impact the witness's perceptions of their performance. Of the 50 participants in the warning condition, 40 of those reported having heard a suggestive statement (80%). Similarly, of the 48 participants in the no warning condition, 31 also reported having heard a suggestive statement (65%).

Discussion

Experiment 2 produced a very similar pattern to Experiment 1 in that I was not able to replicate the traditional PIF effect. These findings do replicate Experiment 1, however, in that the effect of PIF may only be limited to impacting witnesses' perceptions about their accuracy and potentially do not extend to jurors who are asked to evaluate the witness identification. The Bayes Factor analyses again provide evidence that this may be a true null effect of the presence

of PIF, suggesting that there may not necessarily be a detriment to showing jurors eyewitness videos.

Interestingly, the presence or absence of a warning did not appear to impact participants' perceptions of the eyewitness, even when the witness received PIF. Given that ratings of the witness were not impacted, this seems to provide support to the notion suggested by Lampinen et al.'s (2007) work that warnings are not readily utilized.

General Discussion

The goal of the current investigation was to evaluate the impact of PIF on mock-jurors. The aim of the studies were three-fold: 1) to replicate prior literature that found PIF also impacts mock-jurors, 2) to test whether the PIF extends beyond perceptions of the witness to also impact perceptions of mock-jurors' own performance, and 3) to reveal whether warning participants about PIF influences perceptions of the witness and mock-juror. In contrast to what was observed in Beaudry et al. (2015) and Quinlivan et al. (in prep), the current studies failed to detect an impact of the suggestive PIF on participants' perceptions of the witness. Furthermore, I did not observe any effect of PIF on participants' perceptions of their own performance. Although the PIF effect was not observed in Experiment 1, warning participants about its harmful effect on an eyewitness's retrospective judgments could have still produced differential ratings where participants in the PIF with warning condition perceived the witness as being less credible as indexed by the relevant PIF questions. However, this pattern of data was not observed. Similar to Experiment 1, there were no significant differences as a function of PIF presence. The only significant evidence for the alternative hypothesis was that participants who had been provided a warning rated the encounter as having lasted for a shorter duration than those who had not received a warning: a finding that does not lend itself to providing insight on how PIF might impact jurors who view an eyewitness identification procedure at trial. However, the Bayes Factor values provide important insight into the PIF effect as well. Specifically, that there was either substantial or strong evidence for the null hypothesis, which suggests that perhaps the secondary transfer of PIF is not as prevalent of a problem as previous research might suggest.

Taken in isolation and at face value, these data seem to suggest that showing real jurors a videotaped identification procedure that contains these suggestive statements will not produce any differences in perceptions made about the witness. Additionally, given that the PIF relevant questions assess judgments that indicate how credible the witness may appear to jurors, these data suggest that there is no difference regarding how credible a witness appears to be as a function of the PIF statement. Despite the dangers that PIF can have on eyewitnesses (Steblay, Wells, & Douglass, 2014), these findings are optimistic in that showing eyewitnesses' identification procedure recordings will not bias jurors if PIF is present. It is still important to note the effect of PIF on the witness testimony as witnesses who have received the PIF will still be overconfident in their identification at the time of trial, however, previous research has shown that jurors are able to recognize these discrepancies in confidence (Douglass & Jones, 2013). Furthermore, the fact that the current studies did not detect differences in perceptions of the witness as a function of PIF presence posits that perhaps showing jurors eyewitness identification videos could be beneficial.

Science is never interpreted in isolation, and the data from the current studies are in direct contrast to data from Beaudry et al. (2015) and Quinlivan et al. (in prep). This raises questions about why the current study failed to replicate these results. Additionally, these data pose a secondary (and more forensically relevant) question regarding whether mock-jurors also view PIF as confirmation that the witness was accurate. One potential reason for the discrepancy between the current study and prior literature may lie in the materials that were used. Specifically, the difference may stem from the videotaped eyewitness procedure. The actors in the current study were instructed to remain devoid of emotion when providing and receiving the PIF statement. This was done to try keep the PIF and no PIF videos as consistent as possible by

specifically trying to avoid an extra factor of emotion compounded with the PIF statement and this was a contrast to the videos used by Quinlivan et al. (in prep). This may be why our results failed to replicate the prior research.

The lack of emotion in the videos may have affected the current study's ability to find the secondary PIF effect through one of two ways. First, it might be the case that the emotional component of the PIF statement may make the statement more believable and seem to be a reliable cue when assessing the eyewitness. One of the functions of emotion in language is to provide emphasis to important points (Hamilton, Hunter, & Burgoon, 1990). Emotion has been shown to lend itself to overall believability. For example, Vrij and Fischer (1997) suggested that credibility is enhanced when emotional responses fit with the expected emotional reaction within a situation (e.g., despair after a traumatic event). In the case of the current study, maybe the lack of an emotion component when delivering and receiving the PIF statement made this less believable and therefore, less likely to be used as a cue when assessing the perceptions of the eyewitness. This suggestion could be in line if the SCIF model (Charman et al., 2010) is seen to extend from eyewitnesses to mock-jurors. Specifically, the second stage of the model is the search phase, where individuals look for external cues (e.g., the PIF) that could confirm the witness's identification. However, this evidence is then assessed in the evaluation stage, where potentially the lack of believability undermined the credibility of the PIF cue, which caused participants to disregard the PIF when making their assessments. Adding additional video conditions that use the same script as the ones used in the current study, but have the actors incorporate the more natural emotion and emphasis for both the PIF and no PIF videos could assess this explanation.

A second and alternate explanation may be more in line with demand characteristics. Demand characteristics occur when participants create an interpretation regarding the goals of a research study and unconsciously alter their behavior accordingly (Orne, 1996; 2009). With regard to the PIF literature, demand characteristics would be observed in that the PIF statements may stand out enough that participants in prior studies were able to guess the purpose of the study, and provided higher ratings to witnesses who received the suggestive PIF statements. Although the prior studies were between subjects, this remains an alternate explanation. This suggestion could easily be examined by using the same norming procedure on prior PIF videos to assess whether viewers are able to guess the hypothesis of the study.

One explanation that I believe is safe to rule out as an explanation for why I did not obtain the typical PIF effect is that the PIF statements were forgotten or simply not memorable. Evidence for this suggestion comes from the question that asked whether the officer said anything that could make the witness feel more accurate in their identification. This question was asked after participants provided their retrospective judgments and of the participants who heard the PIF statement, 65% and 72% of participants in Experiments 1 and 2 reported hearing the statement. These values might represent a lower percentage of participants who remembered hearing the statement because these values do not include participants who remembered hearing the statement, but did not believe it would bias the witness.

Given that the videos in the current study failed to produce the traditional PIF effect, it is difficult to truly provide an answer to whether PIF extends beyond perceptions of the witness. Based on the data in the current study, the evidence would suggest no, however, examining this using the PIF videos from prior literature would be an interesting avenue. Should those videos produce the PIF by proxy effect, this would warrant the need for further exploration into why the

current studies were unable to detect this effect. Therefore, this still remains an interesting and important question. The applied implications for why it matters whether jurors are over confident in their verdict decisions are the same as the implications for witness: just because they are *confident*, does not mean that they are *accurate*.

Finally, the question of whether jurors can be warned about the effects of PIF remains open, and important. Despite not observing the typical PIF effect, it was still possible that participants who were warned about the feedback could have provided lower ratings pertaining to how credible they viewed the witness to be. This did not happen. Despite the difference between the current study warning the mock-juror and Lampinen et al. (2007) warning the witness, the data from both studies support the notion that PIF warnings are not readily utilized. The contrast between the warning used in the current study compared to Lampinen et al. (2007) was that the former explained exactly *how* PIF might impact how a witness perceives their performance when given a suggestive statement. Alternatively, Lampinen et al. (2007) told witnesses to disregard the feedback they had received because that is what they would be instructed to do if they were a real witness. It is possible that using a warning that takes an approach from Neuschatz et al. (2007), where suspicion is induced, would encourage mock-jurors to discredit the eyewitness, but this is an avenue that should be addressed through future research.

The inability to replicate prior studies give rise to the need for further studies to assess how (if at all) viewing PIF impacts jurors' perceptions of witnesses. The inability to replicate research findings within psychology has been highlighted and observed broadly across multiple psychological domains. For example, the notions that priming students to perform better on tasks (Shanks et al., 2013) or to walk slower when exposed to the stereotype of age (Doyen, Klein,

Pichon, & Cleeremans, 2012), that humans have a limited amount of will power that can be depleted (Hagger et al., 2016), and the idea that smiling can make people happier (Wagenmakers et al., 2016) are examples of findings that were once accepted as scientific facts, but have failed to be replicated in more recent studies. There is now a much warranted push for studies to replicate their findings before they are accepted as truth and this is arguably even more important when the findings contribute to decisions that will either convict or exonerate individuals in the justice system.

The research questions asked in the current study are important and timely given the current environment within the legal system. As technology advances, so do the practices, tools, and techniques used within the legal system. Where it was once viewed as unpractical to record witness identifications, it is now something that can be done nationwide in all police departments. The notion that eyewitness procedures should be recorded is not a particularly novel concept (Kassin, 1998, Technical Working Group, 1999), but is beneficial in encouraging best practice procedures as well as potentially acting as a safeguard for when best practices are not used. However, before researchers can recommend showing these videos to jurors, it is imperative that the effects of these videos are fully understood. Based on the data from the current study, there appear to be no ill effects of showing suggestive PIF statements to jurors, but this is an area of the literature that merits more exploration before researchers can safely recommend this.

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Appendix A. IRB Approval

ACTION ON EXEMPTION APPROVAL REQUEST

Institutional Review Board
Dr. Dennis Landin, Chair
130 David Boyd Hall
Baton Rouge, LA 70803
P: 225.578.8692
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irb@lsu.edu
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TO: Daniella Cash
Psychology

FROM: Dennis Landin
Chair, Institutional Review Board

DATE: May 7, 2019

RE: IRB# E11711

TITLE: Evaluating Behavior

New Protocol/Modification/Continuation: New Protocol

Review Date: 5/7/2019

Approved X **Disapproved**

Approval Date: 5/7/2019 **Approval Expiration Date:** 5/6/2022

Exemption Category/Paragraph: 2a

Signed Consent Waived?: Yes

Re-review frequency: (three years unless otherwise stated)

LSU Proposal Number (if applicable):

By: Dennis Landin, Chairman

PRINCIPAL INVESTIGATOR: PLEASE READ THE FOLLOWING –
Continuing approval is CONDITIONAL on:

1. Adherence to the approved protocol, familiarity with, and adherence to the ethical standards of the Belmont Report, and LSU's Assurance of Compliance with DHHS regulations for the protection of human subjects*
2. Prior approval of a change in protocol, including revision of the consent documents or an increase in the number of subjects over that approved.
3. Obtaining renewed approval (or submittal of a termination report), prior to the approval expiration date, upon request by the IRB office (irrespective of when the project actually begins); notification of project termination.
4. Retention of documentation of informed consent and study records for at least 3 years after the study ends.
5. Continuing attention to the physical and psychological well-being and informed consent of the individual participants, including notification of new information that might affect consent.
6. A prompt report to the IRB of any adverse event affecting a participant potentially arising from the study.
7. Notification of the IRB of a serious compliance failure.
8. **SPECIAL NOTE: When emailing more than one recipient, make sure you use bcc. Approvals will automatically be closed by the IRB on the expiration date unless the PI requests a continuation.**

* *All investigators and support staff have access to copies of the Belmont Report, LSU's Assurance with DHHS, DHHS (45 CFR 46) and FDA regulations governing use of human subjects, and other relevant documents in print in this office or on our World Wide Web site at http://www.lsu.edu/irb*

Appendix B. Video Scripts

PIF 1

Officer: Hello Ms. Reynolds. Thank you for coming. So I am going to show you a lineup containing some photographs and I just want you to let me know if you think any of the people shown is the person who committed the crime. Keep in mind that the culprit might not be in the lineup. Understand?

Witness: *Nods***** Yes.

*Officer slides the folder to the witness. The witness opens the folder and looks at the faces for 5 seconds. The witness keeps a straight/emotionless face during this time. Witness: *After 5 seconds, the witness turns the folder with the lineup toward the police officer and verbally says Number 3 while pointing to the picture.

Officer: Number 3? Great that's who we thought it was also. *Officer should sound neutral*

Witness: Great. *Witness should sound neutral*

No PIF 1

Officer: Hello Ms. Reynolds. Thank you for coming. So I am going to show you a lineup containing some photographs and I just want you to let me know if you think any of the people shown is the person who committed the crime. Keep in mind that the culprit might not be in the lineup. Understand?

Witness: *Nods***** Yes.

*Officer slides the folder to the witness. The witness opens the folder and looks at the faces for 5 seconds. The witness keeps a straight/emotionless face during this time. Witness: *After 5 seconds, the witness turns the folder with the lineup toward the police officer and verbally says Number 3 while pointing to the picture.

Officer: *neutral tone * Number 3? Please sign and date next to that photo.

PIF 2

Officer: Hello (Mr or Ms.) Reynolds. Thank you for coming. What I have here are a couple of photographs of some potential suspects and I want you to let me know if any of these people are the person who committed the crime. Just be aware that the culprit may or may not be in the lineup. Does that make sense?

Witness: *Nods***** Yes.

*Officer slides the folder to the witness. The witness opens the folder and looks at the faces for 5 seconds. The witness keeps a straight/emotionless face during this time. Witness: *After 5 seconds, the witness turns the folder with the lineup toward the police officer and verbally says Number 4 while pointing to the picture.

Officer: Number 4? Great job. That is who we thought too. *Officer should sound neutral*

Witness: Great. *Witness should sound neutral*

No PIF 2

Officer: Hello Ms. Reynolds. Thank you for coming. What I have here are a couple of photographs of some potential suspects and I want you to let me know if any of these people are the person who committed the crime. Just be aware that the culprit may or may not be in the lineup. Does that make sense?

Witness: *Nods***** Yes.

*Officer slides the folder to the witness. The witness opens the folder and looks at the faces for 5 seconds. The witness keeps a straight/emotionless face during this time. Witness: *After 5 seconds, the witness turns the folder with the lineup toward the police officer and verbally says Number 4 while pointing to the picture.

Officer: *neutral tone * Number 4? Just put the date by the photo.

Appendix C. Witness and Participant Perception Questions

Perceptions of the witness

- How certain are you that the witness identified the person who actually committed the crime?
- How willing do you think the witness would be to testify in court the person they identified was the actual culprit?
- To what extent do you think the witness had a good basis (enough information) to make an identification?
- How easy or difficult do you think it was for the witness to figure out which person in the photo spread was the culprit?
- How much do you think that the identification from this witness should be trusted?
- How much attention do you think the witness paid to the culprit's face?
- How well do you think the witness remembers specific details about the culprit's face?
- How long do you think the witness viewed the culprit?
- How far away was the culprit from the witness during the crime?

Manipulation check questions

- What color was the shirt the witness was wearing?
- What was the number of the person who the witness identified?
- What was the witness's last name?
- Which of the following items were shown on the desk?

Perceptions of the participant

- How certain are you that the answers to the questions about the video are accurate?
- How willing would you be to provide your assessments about the video if this were a real witness and a real court case?
- To what extent do you believe that you had a good basis (enough information) to provide your answers to the questions about the video?
- How easy or difficult was it for you to answer the questions about the video?
- How much do you think your assessments for the video that you provided should be trusted as accurate?
- How much attention did you pay to the video information?
- How well do you feel that you remembered specific details from this video?
- How long did the video last?
- How far away was the witness from the law enforcement officer?

PIF check question

- Did the police officer say anything that could potentially bias the witness, or make her feel more accurate than she was?

Appendix D. PIF Warning for Warning Condition

As you are answering the upcoming questions, please be wary of witnesses who received **confirming feedback.** Confirming feedback takes place after an identification and does not affect the result of the identification itself. It can, however, falsely inflate witness confidence in the reports they provide regarding many of the factors commonly used by courts and jurors to gauge eyewitness reliability. The danger of confirming feedback lies in its potential to increase the appearance of reliability without increasing reliability itself.

Please keep this in mind as you answer the upcoming questions.

Vita

Daniella Cash received her bachelor's degree in Psychology from Florida Southern College in 2012. She then completed her M.A. in 2014 at The University of Alabama in Huntsville. She will receive her Ph.D. from the LSU Cognitive and Brain Sciences doctoral program in August 2019. Her research interests broadly include memory and decision-making, particularly pertaining to the legal system. She will start an Assistant Professor position in the upcoming Fall semester.