Undermining Witnesses' Perceptions of Police Belief Improves Eyewitness Identification Accuracy in Showup Procedures

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UNDERMINING WITNESSES’ PERCEPTIONS OF POLICE BELIEFS IMPROVES EYEWITNESS IDENTIFICATION ACCURACY IN SHOWUP PROCEDURES

A dissertation submitted in partial fulfillment of the requirements for the degree of
DOCTOR OF PHILOSOPHY in
PSYCHOLOGY

by
Alexis Taylor Mook

2021
To: Dean Michael R. Heithaus  
College of Arts, Sciences and Education

This dissertation, written by Alexis Taylor Mook, and entitled Undermining Witnesses’ Perceptions of Police Beliefs Improves Eyewitness Identifications Accuracy in Showup Procedures, having been approved in respect to style and intellectual content, is referred to you for judgment.

We have read this dissertation and recommend that it be approved.

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Date of Defense: March 24, 2021

The dissertation of Alexis Taylor Mook is approved.

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College of Arts, Sciences and Education

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Andrés G. Gil  
Vice President for Research and Economic Development and Dean of the University Graduate School

Florida International University, 2021
DEDICATION

I dedicate this dissertation and the subsequent PhD to Joseph Epstein. Without his blatantly misogynistic Wall Street Journal article: Is There a Doctor in the White House? Not if You Need an M.D, hundreds of female PhDs might still struggle to embrace their hard-earned titles. This one’s for you kiddo.
ACKNOWLEDGMENTS

I must first thank my mentor, Dr. Steve Charman for his guidance and support throughout the entirety of my graduate career. I’ve learned so much from working with you and appreciate all the time and effort you’ve spent developing me into the researcher I am today. I would also like to thank my committee members- Dr. Jacqueline Evans, Dr. Deborah Goldfarb and Dr. Amy Hyman-Gregory- for all their input and support throughout this final step of my doctoral program. Your critiques and feedback has helped me complete a project I am proud of.

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And most importantly, I would like to thank my family and friends for their love and support at all times but especially through this journey. Special shout out to my mother, who answered nearly every neurotic phone call I have made to her over the past five years. Without her love, patience and encouragement I might not have made it to this final stage of my doctoral career.
ABSTRACT OF THE DISSERTATION

UNDERMINING WITNESSES’ PERCEPTIONS OF POLICE BELIEFS IMPROVES EYEWITNESS IDENTIFICATION ACCURACY IN SHOWUP PROCEDURES

by

Alexis Taylor Mook

Florida International University, 2021

Miami, Florida

Professor Stephen Charman Major Professor

Showups—an identification procedure in which a single suspect is presented to an eyewitness—are considered to be highly suggestive by the U.S. Supreme Court and eyewitness researchers. Despite the suggestive nature of the procedure, showups are often utilized by law enforcement as a fast and easy means of either quickly arresting a suspect (if the witness makes an identification) or setting a suspect free (if the witness does not make an identification). Researchers have therefore begun examining procedural safeguards that might mitigate the suggestiveness of showups. To date, only the additional-opportunities instruction (AOI) has been developed as an easily implemented safeguard to address the suggestiveness of being presented a single suspect. This instruction has proved beneficial in the lab but has yet to replicate using more ecologically valid paradigms. It is argued that this failure to replicate is due to the instruction’s failure to undermine what we consider to be the leading factor behind the procedure’s suggestiveness in the real-world: the witness’s belief that police believe the suspect is guilty. Therefore, the present study had two research goals: 1) To test whether a new belief instruction aimed at undermining this belief improves showup identification
outcomes; and 2) to test whether the AOI improves identification outcomes using a novel ecologically valid paradigm in which this belief exists. Using a 3 (belief instruction: control/suspect/bystander) X 2 (additional-opportunities instruction: given vs. not given) X 2 (target status: target-present vs. target-absent) between subjects design, participants (N = 1814) viewed a mock-crime they were led to believe was actual footage of a real crime and were presented with a showup identification procedure they believed to be part of an actual criminal investigation. As hypothesized, the AOI and suspect belief instructions had no impact on eyewitness decision-making; however, our novel bystander belief instruction led to a beneficial tradeoff between culprit and innocent suspect identifications as analyzed by logistic regression and receiver operator characteristic (ROC) analyses. Confidence-accuracy characteristic (CAC) analysis found no confidence-accuracy relationship.
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CHAPTER I

INTRODUCTION

On April 5th, 1997, Tommy Vanhoose, a convenience storeowner in Bridge City, Louisiana, was held at gunpoint by a man in a ski mask who demanded he hand over all the money from the store’s register. Vanhoose refused and was shot four times by the perpetrator before he fled the scene of the crime. Vanhoose did not survive. Several eyewitnesses in a nearby car saw the perpetrator take off his mask, gloves, and shirt and dive through the window of a getaway car prior to fleeing the scene. One of these witnesses claimed to have seen the perpetrator’s face in his rear-view mirror as he was being shot at by the man making his escape. A few hours later, police stopped 17-year-olds Ryan Matthews and Travis Hayes near the scene of the crime, who were found parked in a car that matched the description of the one seen fleeing Vanhoose’s convenience store. Police brought the witness (who claimed to have gotten a look at the perpetrator’s face in his rear-view mirror) to where Ryan Matthews and Travis Hayes were being detained. While there, police conducted a showup identification procedure (an identification procedure in which a single suspect is presented to an eyewitness), in which the witness was asked whether these men were the murderer and getaway driver that he witnessed committing the crime earlier. The witness identified Ryan Matthews as the murderer, an identification that led to his immediate arrest (Innocence Project, 2020b). Travis Hayes, the alleged getaway driver, was also arrested for second-degree murder after this initial identification. Hayes was interrogated regarding his role in the crime. Although Hayes was initially adamant that both he and Matthews were not in the area
during the time of the crime, six hours into the interrogation Hayes confessed to being the getaway driver. Defense teams later argued that both Hayes and Matthews were borderline mentally retarded (Innocence Project, 2020c).

During their trial, defense teams found evidence to suggest the car in which the suspects had been parked could not have been the getaway car. Although the witnesses claimed the perpetrator jumped through the passenger window of the getaway car, the defendants’ car’s passenger window was inoperable and could not be rolled down. Furthermore, no other witnesses were able to identify either man during lineup procedures conducted after the initial arrest. Unfortunately, the single showup identification of Matthews was enough to have him convicted of murder and sentenced to death row. Hayes was also convicted of second-degree murder and sentenced to life in prison, based solely on his eventual confession. Both men maintained their innocence since their arrest. Years later, DNA that was being tested as evidence in another murder in the area indicated a local man Rondell Love’s DNA to be a match to the mask left behind at Vanhoose’s convenient store. It was not until 2004, after spending five years on death row, that Ryan Matthews was granted a new trial and was later exonerated for his crimes. Matthews became the 14th death row inmate in the United States to be exonerated based on DNA testing (Innocence Project, 2020b). Due in part to his confession, Hayes was not granted a re-trial until December 2006 at which point he was also exonerated for his alleged role in Vanhoose’s death (Innocence Project, 2020c).

If it were not for the witness’s initial showup identification, it is unlikely either Matthews or Hayes would have been interrogated or arrested for this crime. Unfortunately, Ryan Matthews’ story is not an uncommon one. Mistaken eyewitness
identifications are the leading cause of wrongful conviction cases, playing a role in nearly 70% of DNA exoneration cases (Innocence Project, 2020a). Of these cases, showup identifications, such as that of Matthews, make up between 15 to 34% of these misidentifications (Garrett, 2011; West & Meterko, 2016). A showup identification procedure is one in which an eyewitness is shown only one person (the suspect) without any known-innocent fillers. Showup identification procedures tend to be used in situations in which an individual fitting the description of the perpetrator is detained in the general vicinity of the crime, usually shortly after the crime has occurred. In Ryan Matthews’ and Travis Hayes’ cases, they did not fit witnesses’ descriptions; in fact, Matthews was significantly shorter than the reported perpetrator. Rather, the car the men were driving was similar to that of the assailants’ car. Driving a vehicle that fits a description of one that fled a crime scene is not grounds for arrest on its own. This is an excellent example of why police may opt for a quick showup instead of waiting to conduct a lineup. A showup can help to quickly get a potentially dangerous person off the streets. Additionally, law enforcement might also use showups to quickly exonerate an innocent person, allowing investigators to spend more time searching for the actual perpetrator.

For these reasons, showups are extremely common in real world investigations. Field research by Gonzalez, Ellsworth and Pembroke (1993) found that 77% of identification procedures being conducted in Illinois were showup procedures. Additionally, McQuistion and Malpass (2001) found that showups were used in over 30% of all identification procedures in El Paso, Texas in the 90s. Flowe, Ebbesen, Burke and Chivabunditt (2001) found showups to be used in 55% of identification tasks in a
non-disclosed U.S. metropolitan area between 1991 and 1995. A more recent examination of eyewitness identification evidence from a sample of felony cases from a District Attorney’s office in a large south-western U.S city between 1991-2000 found similar results, with nearly 60% of felony suspects having been presented for identification using a showup procedure (Flowe et al., 2011). Taken together, these numbers suggest showups are a prominent identification procedure throughout the United States.

Despite the frequent use of showups in real-world investigations, there has been relatively little attention given to the procedure by eyewitness researchers compared to the much more voluminous literature on lineups. Seminal research such as Wells’ (1978) characterization of system and estimator variables did not directly address showups as a potential system variable (i.e., a variable that is under the control of the criminal justice system), nor were showups addressed in the first “white paper” on eyewitness identification research (Wells et al., 1998). This original “white paper” was the first set of science-based recommendations on eyewitness identification procedures and was pivotal in the development of the U.S. Department of Justice’s guide (hereafter referred to as the Guide) for obtaining eyewitness evidence. Upon its release, the Guide was disseminated to all police departments in the United States and has been considered to be best practice procedures for procuring eyewitness evidence (Technical Working Group, 1999).

Although showups are addressed in the Guide, there are few recommendations provided regarding their administration beyond separating co-witnesses before they attempt a showup identification and using pre-showup instructions that the suspect may or may not be the perpetrator, a recommendation that at that point in time had not yet been
researched in the context of showup identifications. The absence of showups in the original white paper and their minimal consideration in the Guide are likely in part a reflection of researchers’ beliefs that the procedure not be used in the first place.

Researchers have generally discouraged the use of showup for two main reasons: (1) lineups are a superior means of eliciting accurate identification decisions (Clark, 2012; Gronlund et al., 2012; Steblay et al., 2003; Wetmore et al., 2015); and (2) showups are considered to be an inherently suggestive procedure (Neuschatz et al., 2016; Wells et al., 2020).

**Lineup Superiority**

Decades of research have found the use of showups to be an unreliable means of procuring an accurate identification (Clark, 2012; Lee & Penrod, 2019; Steblay et al., 2003). In fact, the most recent version of the updated “white paper” for eyewitness identifications states “There is no debate among eyewitness scientists about the fact that lineups produce better outcomes than do showups…” (Wells et al., 2020, p. 7). A variety of studies have supported this conclusion over the years. Specifically, Steblay et al. (2003) conducted the first meta-analytic comparison of showups and lineups. The researchers examined eight published articles consisting of 12 tests of identification performance between showups and lineups. The meta-analysis found hit rates (correctly identifying the suspect in target-present conditions) to be approximately equal between showups and lineups, with showups also leading to more correct rejections (correctly identifying the suspect as “not there” in target-absent conditions) than lineups. However, when excluding filler identifications (known errors) from data analyses, false identifications of the innocent suspect were greater in showups (15%) than in lineups.
This false identification rate from showups increases dramatically in studies in which the innocent suspect strongly resembles the perpetrator: nearly a quarter (23%) of witnesses made false identifications of highly similar innocent suspects from showups whereas only 10% of witnesses made false identifications of highly similar innocent suspects from lineups. Since this original meta-analysis on the topic, these trends have persisted, with a more recent meta-analysis of 51 studies by Clark (2012) finding remarkably similar results: although lineups and showups produced approximately equal correct identification rates (43% and 41% respectively), lineups produced a lower false identification rate (11%) than did showups (18%), closely replicating Steblay et al.’s (2003) findings.

**Bayesian analyses.** Not only have these trends persisted over time, but they have also persisted across new and more advanced statistical analyses. The field of eyewitness psychology has evolved greatly in terms of how identification data is approached and analyzed. For instance, some eyewitness researchers have advocated for a Bayesian approach for examining eyewitness identification data (Wells & Olson, 2002). A Bayesian analysis of eyewitness identification data demonstrates how the information gained from an identification (i.e., the extent to which an investigator can update his/her belief in the guilt of the suspect as a function of the witness’s identification decision) varies dramatically as a function of the base rate of suspect guilt (i.e., the probability, prior to the identification attempt, that the suspect is guilty). In contrast, traditional eyewitness data, which is generally presented as frequencies of various identification outcomes (hits, false alarms, correct rejections, and filler identifications) as a function of some manipulated variable, is unable to account for the effect of base rates.
The importance of base rates is demonstrated by Wells, Yang, and Smalarz (2015), who adopted a Bayesian approach to re-analyze Clark’s (2012) data on showups and lineups. Specifically, they developed base-rate effect-equivalency (BREE) curves, which demonstrate how the beneficial effect of changing from a worse to a better identification procedure can similarly be accomplished by simply increasing the base rate of a suspect’s guilt before conducting an identification procedure. Their analysis showed that (a) changing from a showup to a lineup procedure produces a more beneficial overall effect compared to any other procedural improvement, and (b) the benefit of changing from a showup to a lineup procedure was equivalent to increasing the base rate of guilt by up to 13%.

**Multi $d'$ analyses.** Even more recently, Lee and Penrod (2019) introduced the multi $d'$ model, an extension of signal detection theory. Signal detection theory (SDT) is a framework for analyzing memory performance data that allows eyewitness researchers to distinguish between whether an identification procedure caused changes in witnesses’ decision criteria (i.e., their willingness to make an identification) versus changes in witnesses’ abilities to discriminate between innocent and guilty suspects. For instance, instructing witnesses to only make an identification if they are 100% sure should raise their decision criteria, thus tending to make them less likely to attempt an identification and reducing both correct and false identifications; however, it should not affect their ability to discriminate between innocent and guilty suspects. In contrast, providing witnesses with a better view of the criminal should improve witnesses’ abilities to discriminate between innocent and guilty suspects, but may not affect their overall willingness to make an identification (i.e., their decision criterion).
However, SDT was developed to apply to situations in which people must make an old/new recognition decision for a single stimulus, and consequently their responses can be categorized into a 2 (witness response: old vs. new) x 2 (stimulus: old vs. new) structure. However, witnesses who view a lineup are shown multiple stimuli at once (i.e., multiple lineup members), and their responses are consequently categorized into a 3 (response: suspect ID, filler ID, not there) x 2 (stimulus: target-absent, target-present) structure. Lee and Penrod (2019) have addressed this shortcoming of applying SDT to a lineup task with their multi $d'$ model, in which witnesses can be thought of as making two decisions: (a) whether the guilty person is in the lineup, and (b) if so, which lineup member is the guilty person. Their data using this multi $d'$ model demonstrates that lineups better allow witnesses to discriminate guilty suspects from innocent suspects compared to showups.

**ROC analyses.** Finally, many eyewitness researchers argue that receiver operator characteristic (ROC) analyses are most appropriate for analyzing eyewitness data, as these analyses provide a way of differentiating between changes in decision criteria and changes in discriminability without the underlying theoretical assumptions of SDT (Wixted & Mickes, 2015). ROC analyses involve constructing ROC curves by plotting culprit identification rates as a function of innocent suspect identification rates across varying levels of decision criteria (for which witness confidence serves as a proxy). Comparing ROC curves allows researchers to see which procedure produces a better tradeoff between culprit identifications and innocent suspect identifications. The identification procedure that produces the best discriminability between the innocent and guilty suspect results in the highest ROC curve, closest to the upper left-hand corner of
the graph. ROC analysis can better determine if an identification procedure results in better discriminability (i.e., the ability to distinguish between guilty and innocent suspects), changes in decision criteria, or both (Neuschatz et al., 2016; Wetmore et al., 2015).

Gronlund et al. (2012) were the first to compare showups and lineups using ROC analyses. The researchers chose to reanalyze data from a prior study, Gronlund et al. (2009), which compared eyewitness performance between simultaneous lineups (a lineup in which the suspect and fillers are shown at the same time) and sequential lineups (a lineup in which the suspect and fillers are shown one at a time). Although early research provided evidence for a sequential lineup advantage, whereby sequential lineups reduced false identifications without a concomitant reduction in the correct identification rate (Steblay et al., 2003; Steblay et al., 2011), subsequent research has shown that this advantage may only be found when the suspect is placed late, but not early, in a sequential lineup (Gronlund et al., 2009). Furthermore, recent ROC analyses suggest a simultaneous lineup advantage (Mickes, Flowe, & Wixted, 2012). Clark (2012) concluded that the main effect of a sequential lineup is to produce a conservative criterion shift, the effect of which is to reduce both false identifications and correct identifications.

Gronlund et al. (2012) compared this old data from Gronlund et al. (2009) to new data collected using the same stimuli and procedures but using a showup procedure. ROC analyses found simultaneous lineups to be diagnostically superior to showups, supporting the general consensus among eyewitness researchers that showup procedures are an inferior procedure. However, sequential lineups were sometimes no more diagnostic than showups contradicting Steblay et al.’s (2011) findings of a sequential lineup advantage.
Specifically Gronlund et al. (2012) found positions effects, in which suspects that were placed early in a sequential lineup (position 2) resulted in lineup performance that was no better than showups.

Although Gronlund et al.’s (2012) research supported the notion that simultaneous lineups are superior to showups, there are two potential limitations to this research. First, they did not manipulate retention interval, which could play a pivotal role in the efficiency of showup procedures. As noted earlier, showups are commonly conducted shortly after the crime has occurred with an eyewitness and suspect still near the scene of the crime. This has potential to provide an advantage for showup procedures, as it is well documented that memory degrades over time (Light, 1996; Deffenbacher et al., 2008). Therefore it is possible an immediate showup performs better than a delayed lineup. To test this assertion, Wetmore et al. (2015) examined eyewitness performance, again using ROC analysis, to determine whether retention interval moderated the apparent benefits of simultaneous lineups. They found no evidence for such an effect and concluded that lineups are more diagnostic than showups regardless of retention interval. Neuschatz et al. (2016) further supported these findings through their meta-analysis, which also re-analyzed showup data with ROC curves. They found eyewitness discriminability in immediate showups to be worse than lineups after a 2-day delay.

Second, Gronlund et al.’s (2012) lineups were fair—in other words, all fillers matched a description of the perpetrator. Wells and Quinlivan (2009) have suggested, however, that biased lineups, in which the fillers are a poor match to the suspect, could produce worse outcomes than showups. This effect could occur if dissimilar fillers make the suspect stand out of a biased lineup, inflating the false identification rate. To test this
assertion, Wetmore et al. (2015) and Neuschatz et al. (2016) also compared showups to biased lineups. However, neither study supported Wells and Quinlivan’s (2009) assertion: Both studies found both fair and biased lineups to be more diagnostic of eyewitness accuracy compared to showups, leading Neuschatz et al. to conclude “ROC analyses paint a grim picture of showups as an identification procedure” (p. 56). Taken together, decades of research and various methods of analyzing identification data paint a very clear picture of lineups being a more reliable measure of a suspect’s guilt compared to showups. While researchers tend to agree on this point, there is still some debate regarding the theoretical explanation for the poor performance observed from showups.

**Theoretical Perspectives on Lineup Superiority**

**Diagnostic Feature Detection Theory.** There are two camps of research regarding the theoretical reasoning behind lineup superiority. Wixted and Mickes (2014) proposed the diagnostic feature-detection (DFD) hypothesis to explain lineup superiority. DFD argues that because fillers are selected based on common underlying characteristics, the suspect and fillers share these characteristics. However, there will nonetheless be some features that are unique to the suspect. These diagnostic features allow witnesses to differentiate guilty from innocent suspects in a lineup. DFD therefore posits that lineups produce better discriminability than showups because lineups allow eyewitnesses to compare multiple lineup members, effectively teaching them which features are non-diagnostic (and can thus be ignored) and which features are diagnostic (and should thus be attended to). For example, if all lineup members are young Hispanic men with short dark hair, an eyewitness learns to see past these shared features and instead to focus on more diagnostic features such as eye shape, etc. In contrast, showups do not allow a
witness to identify these diagnostic facial features, as there are no fillers available with which to compare the suspect’s features. Therefore, Wixted and Mickes (2014) argue the benefits found for lineups as opposed to showups is due to witnesses in showups making less diagnostic identification decisions due to the absence of filler features that could be used to better discriminate guilty and innocent suspects.

**Filler Siphoning Theory.** Wells, Smalarz, and Smith (2015) argue that DFD is problematic in that it was based on conclusions derived from ROC analyses, which, due to its inherent 2 (stimulus: present, absent) x 2 (response: present, absent) structure, is forced to collapse filler identifications and lineup rejections into a single ‘non-identification’ category. They argue, however, that filler identifications and lineup rejections are qualitatively different, and that lumping them into a single non-identification category can lead to erroneous conclusions regarding eyewitness performance. For this reason, they argue that ROC differences cannot be interpreted as differences in witnesses’ underlying ability to discriminate between guilty and innocent suspects, making DFD inherently flawed (Smith et al., 2017). Instead, they argue that lineups produce better outcomes than showups not because lineup fillers improve eyewitnesses’ abilities to distinguish between guilty and innocent suspects, but because all incorrect identifications from showups fall on the innocent suspect, whereas in a simultaneous lineup, incorrect identifications can be siphoned off to fillers. Because filler identifications are known errors, these identification decisions do not impact the likelihood an innocent suspect is investigated or convicted of a crime they did not commit.
These researchers argue the reason for the reduction in false identifications observed within lineups compared to showups is entirely due to increased filler identifications. The logic behind this claim comes from the idea that if lineups truly allowed witnesses to better discriminate features diagnostic of a suspect’s guilt or innocence, they would also inflate correct rejections. However, the data contradict this assertion, as correct rejections are higher in showups than in lineups (Smith et al., 2017). Therefore, according to Wells et al. (2015), the issue with showups is not that they hinder a witness’s ability to discriminate between guilty and innocent suspects, but rather that all incorrect choices are false identifications. In other words, the inflated false identification rate seen from showup identifications is an artifact of the highly suggestive nature of police presenting a witness with only one person.

**Suggestiveness of Showups**

Showups are often criticized as being a suggestive procedure by researchers and lawyers alike. Courts as high as the U.S. Supreme Court have addressed the suggestiveness of the procedure, stating “It is hard to imagine a situation more clearly conveying the suggestion to the witness that the one presented is believed to be guilty by police” (*United States v. Wade*, 1967, p. 226). The most obvious factor that leads a showup to be suggestive, as addressed through Wells et al.’s (2015) filler siphoning theory, is the absence of fillers and a presumption of guilt associated with being presented a single suspect. As mentioned above, showups are typically carried out by a witness or victim being brought to view a suspect who is (a) near the scene of the crime, (b) matches the description of the perpetrator, and (c) is being detained by law enforcement. Researchers and lawmakers alike have argued that simply being detained by
police officers could be suggestive enough for witnesses to misidentify an innocent suspect (Agricola, 2009; Eisen et al., 2017a; Eisen et al., 2017b; Wells et al., 2020).

Steblay (2013) has suggested that even during lineup identification tasks it is likely a witness will assume the police have caught the actual culprit. However, during a lineup procedure this suggestive presumption should not affect one lineup member any more than other lineup members, as the fillers act as a procedural safeguard. The assumption that law enforcement only presents a suspect because they believe they are the culprit likely stems from a general trust in police (Behram & Davey, 2001; Wagenaar & Veefkind, 1992). Unfortunately, even the most well-intentioned and detailed investigators could unintentionally detain an innocent suspect. In these cases, the absence of fillers puts an innocent suspect at risk to be falsely identified because witnesses generally trust police. There are however numerous additional reasons why showups tend to be more suggestive than lineups.

**Pressure to Choose.** There are several factors that have been found to exacerbate the inherent suggestiveness of showup procedures. For example, it has been proposed that showups, especially those conducted in the field, may result in witnesses feeling an increased pressure to choose the suspect. In three experiments, Eisen and colleagues (2017a) compared identification performance from showups using either a lab-based paradigm (in which participants knew they were part of a study) or a field-simulation paradigm (in which participants believed they were making a real-world identification). The researchers found that nearly 50% of eyewitness in field-simulated conditions reported feeling pressured to choose compared to only 28% of eyewitnesses in lab conditions (Eisen et al., 2017a). The increased pressure to choose is particularly
dangerous when conducting a showup being that all incorrect identification decisions made as a result of this added pressure land on the innocent suspect rather than some incorrect identifications landing on fillers (as they would if conducting a lineup).

**Suspect Similarity.** Additionally, the similarity between a culprit and an innocent suspect has been found to exacerbate the suggestiveness of showup procedures. For instance, Eisen et al. (2017a) showed that an innocent suspect that highly resembles the culprit resulted in a higher showup false identification rate (20.5%) than an innocent suspect that was less similar to the culprit (5.3%). This effect also existed when using a field-simulated paradigm in which participants believed their identifications had real-world consequences (48.3% vs 37%, respectively), putting similar looking suspects at an increased risk of being falsely identified. These results are concerning given that the main reason an innocent suspect would be presented in a showup in the first place is that they match to the description of the perpetrator, and thus are expected to be similar looking to the actual culprit. Additionally, it is important to note that in field-simulated conditions dissimilar suspects were 9 times more likely to be identified (37%) compared to those in lab conditions (5.3%). Taken together, these results suggest that while suspect similarity exacerbates real-world showups’ suggestiveness, the procedure is suggestive enough to even put dissimilar innocent suspects at risk of being falsely identified.

**Clothing Bias.** An additional factor that can increase the suggestiveness of showups is clothing bias, in which the suspect is wearing clothing similar to that worn by the perpetrator. Nearly 50% of all descriptors provided by eyewitnesses involve clothing information and this information is often more descriptive than person descriptors (Lindsay et al., 1987; 1994). In the first known study examining effects of clothing bias
in showups, Yarmey et al. (1996) found false identifications to be significantly higher when a similar looking innocent suspect wore the same clothes as the perpetrator. However, clothing did not increase false identifications when the suspect was an “implausible” innocent suspect. Additionally, it may be unlikely for an innocent suspect to be wearing identical clothing to a perpetrator unless that clothing is common clothing such as a plain black t-shirt, etc. Therefore, Dysart et al. (2006) tested the effects of clothing bias in showups further by manipulating clothing type (common vs. distinct) and target similarity (same target, high similarity innocent suspect, low similarity innocent suspect). They found a significant difference in overall identification accuracy between common and distinct clothing. Specifically, when the target was presented in “common” clothing, an effect of clothing bias was virtually non-existent. However, distinct clothing (a Harley-Davidson t-shirt) led to a significant increase in false identifications regardless of how similar the suspect resembled the perpetrator. False alarms were over twice as prevalent when a highly similar suspect (50%) was wearing distinct clothes compared to those who were wearing common clothes (23%). Additionally, low-similarity innocent suspects were only identified while wearing distinct clothes (37%) compared to common clothes (0%). Clothing bias did not influence correct identifications, whether the clothing was common or distinct, resulting in only detrimental effects associated with clothing bias in showup procedures even for low-similarity suspects.

**Blind Administration.** Another suggestive feature of showup procedures is the inability to conduct one in a double-blind manner. Double-blind lineup administration refers to a situation where neither the eyewitness nor lineup administrator (usually a police officer) knows which lineup member is the suspect and which are fillers. Double-
blind administration is recommended for lineup procedures as a way to ensure a witness is not influenced, either intentionally or not, from choosing one lineup member over the others (Wells et al., 1998; 2020). Double-blind procedures also eliminate the opportunity for postidentification feedback (an administrator confirming the witness picked the suspect) to unduly inflate witness confidence (Wells & Bradfield, 1998). While the recommendation for double-blind procedures were made in the original 1998 “white paper” of eyewitness procedures, at the time there was no empirical research on its impact on lineup procedures specifically. Rather the recommendation was based on expectancy effects—when an individual’s expectations of another person’s behavior cause that behavior to occur (Rosenthal, 1994). More recently, these effects have been more directly applied to eyewitness identification research.

As expected, non-blind lineup administration has been associated with a variety of unwelcome outcomes (Kovera & Evelo, 2020). Most basically, witnesses who were administered a lineup by non-blind administrators are more likely to identify the suspect, regardless of suspect guilt (Greathouse & Kovera, 2009). Additionally, non-blind lineup administrators have been found to exhibit suggestive behaviors towards the witness, such as smiling at, pointing out, and asking questions about the suspect (Charman & Quiroz, 2016; Zimmerman et al., 2017). Non-blind administrators are also more likely to interpret ambiguous statements by witnesses directed towards the suspect (e.g., “It could be #3 but I’m not sure”) as identifications compared to blind administrators (Charman, Matuku & Mook, 2019). It is difficult, if not impossible, to conduct a double-blind showup procedure. Even if police officers made sure only officers who are not privy to case details administered showups, both the administrator and witness are still aware the
person being presented to them is the suspect. Not only does this make a showup inherently prone to these negative outcomes associated with non-blind administration, but the absence of fillers also creates a situation where all inaccurate identifications land on the innocent suspect, creating a very dangerous situation for any innocent person that finds himself or herself in a showup.

**White Paper Recommendations**

While research seems to paint a clear image of showup procedures being highly suggestive and inferior to lineup procedures, showups nonetheless persist in real world investigations, given their advantages in terms of quickly incriminating or exonerating a suspect. Therefore, researchers have shifted their focus away from testing the reliability of showups towards examining ways in which the procedure can become more reliable. For instance, the updated “white paper” provides the following recommendation for collecting and preserving eyewitness evidence from showups:

Showups should be avoided whenever it is possible to conduct a lineup. Cases in which it is necessary to conduct a showup should use the procedural safeguards that are used for lineups, including the elimination of suggestive cues, a warning that the detained person might not be the culprit, video-recording the procedure, and securing a confidence statement. (Wells et al., 2020, p. 8)

Although the authors acknowledge that showups should, whenever possible, be avoided, they also address a ramification of this recommendation: showups will tend to be used specifically when there is a low base rate of the suspect’s guilt. Specifically, if showups are to be used when lineups cannot be conducted, they will tend to be used when an officer has an individual detained but currently has no other evidence linking them to the
crime to warrant an arrest other than a description match. In other words, the likelihood of the suspect being guilty is relatively low. Given the inherent suggestiveness of a showup procedure—the fact that the witness knows who the suspect is, might trust the police to have arrested the correct person (Behrman & Davey, 2001), and can often see cues suggesting the police believe the suspect is guilty (e.g., they are often presented in handcuffs, a police car, or simply held by an officer)—it is possible a witness misconstrues detainment as an arrest. A low base rate of guilt combined with a suggestive procedure therefore results in a high likelihood of a witness making a false identification. Consequently, the various procedural safeguards recommended by the updated white paper—using unbiased instructions, video recording the procedure, and collecting an immediate confidence statement—are particularly important for showup identifications. Nonetheless, there are only a handful of studies that have examined whether the benefits of such safeguards extend to showup procedures.

Unbiased Instructions as Procedural Safeguards for Showups

The recommended procedural safeguard that has received the most attention is the use of unbiased preidentification instructions (also referred to as an “admonition”). A standard admonition informs witnesses that the actual culprit may-or-may not be present in the identification procedure. This instruction is universally part of best practice recommendations regarding lineup administration (e.g., Wells et al., 2020; Technical Working Group, 1999). Additionally, it is one of the few recommendations that has been widely adopted amongst law enforcement agencies. Most jurisdictions within the United States use some form of the instruction and such an instruction is a required procedure in England and Wales when administering lineups (Steblay, 2013). There is a plethora of
research that has examined the effect of this admonition for lineup identifications. Most notably, two meta-analyses have indicated that admonishing witness prior to an identification procedure reduces the likelihood of a false identification but at the cost of also reducing the likelihood of a correct identification (Clark, 2005; Steblay, 1997). Additionally, subsequent research using very large sample sizes depicted this same pattern of results with admonitions producing fewer incorrect identifications and fewer correct identifications among both adult and child witnesses (Brewer & Wells, 2006; Keast, Brewer, & Wells, 2007; Wells et al., 2020).

Additional research examined whether admonitions would continue to act as a procedural safeguard in the presence of other suggestive information regarding the suspect’s guilt. For instance, Quinlivan et al. (2012) tested whether pre-admonition suggestions made prior to reading unbiased lineup instructions would mitigate the prophylactic effect that is typically seen with admonitions of lineup identifications. Before being shown a lineup, witnesses to a mock crime were either told “surely, you can pick the perpetrator” (pre-admonition suggestion) or not (no suggestion), and were subsequently given either biased or unbiased lineup instructions. As predicted, the researchers found that the pre-admonition suggestion, which suggested to the witness the actual culprit must be in the lineup, increased the rate of false identifications in target-absent conditions, even among witnesses given otherwise unbiased instructions. Additionally, the false identifications made after hearing the pre-admonition suggestion were made with more confidence, and these participants rated themselves as more willing to testify that the person they picked was guilty, compared to those who did not hear the
suggestion. These results suggest unbiased instructions alone do not mitigate other, more suggestive, influences during lineup procedures.

Despite the decades of research on the effect of unbiased instructions on lineup identifications, only a handful of studies have examined their effect on showup identifications. Eisen et al. (2017a) examined the efficacy of admonishing witnesses in culprit-absent showups in both laboratory and field-simulated conditions. The admonition in this study was not a simple “may or may not be there” but was more detailed, with witnesses being told:

You are under no obligation to identify this person as the suspect. We want to have guilty persons identified, but we also want to make sure that innocent persons are cleared of any suspicion in this matter. You should not draw any conclusions about a person just because he is in our custody, or handcuffed (Eisen et al., 2017a, p. 5).

The admonition in the laboratory condition was nearly identical but with the last sentence instead stating “You should not draw any conclusions about the person just because he is being presented to you.” The researchers found that admonishing witnesses led to a dramatic reduction in false identifications from showups in both field-simulated (21%) and laboratory conditions (11%). However, the benefits of admonition were much more prevalent when the suspect was dissimilar to the culprit in height and weight. Nonsimilar suspect identifications were reduced by 31% in field-simulated conditions and were eliminated entirely in laboratory conditions. However, when the suspect was a good match to the culprit the use of a preidentification admonition did not significantly reduce false identification rates. Therefore, admonishing witnesses only acted as a
safeguard against false identifications from showups when innocent suspects differed from the actual culprit in height and weight, which is often a rarity in real world showups because most are conducted when a suspect matches the description of the perpetrator.

The limitations of unbiased instructions in controlling the false identification rate from showups are also demonstrated by Eisen et al. (2017b), who examined the effects of a subtle pre-showup suggestion on false identifications from highly realistic field-simulated showup procedures. Researchers staged a realistic laptop robbery during an ostensibly unrelated experiment. During the experiment, a confederate-participant walked out of the study with the researcher’s laptop. Shortly after, two research assistants walked in and frantically announced that the participant who left stole their laptop. They then discussed aloud that they must call the head researcher. Participants then overheard a phone call with the researcher in which an experimenter described what occurred. All participants were then told that the head researcher had called the police and that they would be required to chat with police when they arrived. Shortly after, a research assistant dressed as a campus police officer arrived to the lab and interviewed the experimenters regarding the crime. As the ‘officer’ was interviewing the experimenters, participant groups overheard a call come through the officer’s radio stating either “The L.A. County Sheriff has detained an individual who matches the description of the thief…” (no pre-admonition suggestion) or “The L.A. Sheriff caught the guy…” (pre-admonition suggestion). The campus police then escorted the participants individually to view the suspect, who was being detained in handcuffs and standing next to a sheriff’s patrol car. Prior to making an identification decision, all participants were provided with unbiased instructions. As with Eisen et al.’s (2017a) previous research, the similarity of
the perpetrator was manipulated in terms of height and weight. As predicted, the researchers found that participants in culprit-absent conditions who overheard the pre-admonition suggestion were over twice as likely to make a false identification than those in the control condition (52% vs. 24%). Additionally, the false identification rate was even more pronounced when the suspect was similar (48%) compared to not similar (13%) to the actual culprit. Even simply overhearing a pre-admonition suggestion was found to diminish the protective nature of unbiased identification instructions. Taken together, these studies suggest the protective effects of admonitions are limited during suggestive procedures.

**Present-Absent Criteria Discrepancy Theory**

Even in situations where admonitions are protective, admonishing witnesses prior to identification procedures typically reduces the likelihood of a false identification at the cost of also reducing the likelihood of a correct identification (Clark, 2005; Steblay, 1997; 2013). Clark argues from a signal-detection theory perspective that this trade-off is a result of pre-identification admonitions leading witnesses to set lower decision criteria during the identification task, regardless of whether the witness is presented with the culprit or not. As it pertains to showups, a signal detection task involves a yes/no judgment as to whether the person the witness is presented with as the suspect matches their memory of the perpetrator. If the suspect’s resemblance to the perpetrator surpasses the decision criterion set by the witness then they will respond by identifying the suspect as the perpetrator. If this match to memory instead falls below this set decision criteria, then they will fail to make an identification. Therefore, setting a lower decision criterion results in greater choosing rates from both target-present and target-absent showups.
Clark (2005) argues that this pattern shows that instructions only influence a witness’s tendency to make an identification (response bias) but they do not influence their ability to distinguish between guilty and innocent suspects (discriminability).

Smith et al. (2018) argue, however, that instructions can affect witnesses’ decision criteria differently, depending on whether the witness is shown the perpetrator or not. They point out that Clark’s (2005) logic is based on recognition paradigms from basic cognitive psychology, whereby participants are shown many trials of “new” items to match to memory, some where the signal is present, others where it is not. This repeated-measures design allows participants to settle on a set decision criterion after several trials, which they then apply to both target-present and target-absent tests. This assumption of a single criterion—regardless of whether the witness is shown a target-present or target-absent procedure—is the crux of Clark’s argument. Smith et al. argue that we cannot make this single criterion assumption because eyewitness research paradigms are not repeated-measures tests but instead involve only a single test per participant. Therefore, we cannot assume participants in eyewitness paradigms are making their decisions using the same set criterion placement in both target-present and target-absent conditions.

Rather, Smith et al. (2018) propose the present-absent discrepancy theory, which argues that eyewitnesses who view a target-absent showup will tend to have a weak recognition experience and will consequently drop their decision criterion in order to avoid missing making a correct identification. This lowered decision criterion will then only require the suspect to be a relatively weak match their memory of their perpetrator in order to make an identification. In contrast, eyewitnesses who encounter a target-present showup, and who thus tend to have a relatively strong recognition experience,
have no need to drop their decision criterion. This theory is based around the assumption that participants in target-absent conditions have an inherently more difficult recognition task because they are attempting to detect a signal (identify the perpetrator) when there is no signal to detect (shown an innocent suspect).

Prior research looking at eyewitness performance in suboptimal encoding conditions (e.g., short exposure duration, greater distance at encoding, disguise) support the notion that weaker recognition experiences, and therefore more difficult recognition tasks, result in eyewitnesses lowering their decision criterion (Lampinen et al., 2014; Mansour et al., 2012; Memon et al., 2003). Additional research has shown that eyewitnesses given target-absent lineups report having a worse view or paying less attention than participants given target-present lineups, despite having had an identical viewing experience and paying on average the same amount of attention (Bradfield et al., 2002). This finding has been reasoned to be a result of target-absent eyewitnesses misattributing the difficulty of their identification decision to factors other the possibility they are presented with a target-absent lineup, as a precaution to not miss their opportunity to identify the perpetrator. The present-absent discrepancy theory builds on these findings by proposing that eyewitnesses assigned to target-absent conditions set a lower decision criteria than those in target-present conditions because they inherently have a weak recognition experience and believe this will be their only opportunity to identify the perpetrator.

This is far from the first theoretical model of eyewitness decision-making that has suggested eyewitnesses engage in different cognitive processes when they encounter a target-present or target-absent lineup. For instance, seminal research by Wells (1984,
1993) examined relative and absolute eyewitness judgments as a means to address differences between eyewitnesses in target-present and target-absent conditions. According to Wells (1984), an absolute judgment is one where a witness selects a lineup member because they believe that person matches their memory of the perpetrator. A relative witness judgment is made when a lineup member is selected because the suspect is a better match to the witnesses’ memory relative to the other lineup members. These different judgment strategies result in little difference among eyewitness decision-making when the target is present; however when the target is absent, a relative judgment puts an innocent suspects at a greater risk of being identified compared to absolute judgments (Wells, 1993).

Based on the present-absent discrepancy theory, Smith et al. (2018) reasoned that if eyewitnesses in target-absent conditions are simply setting a lower decision-criterion than those in target-present conditions, then it should be reasonable to assume an instruction aimed at mitigating target-absent witnesses’ propensity to set a low decision criterion should reduce rates of false identifications in these conditions, without also reducing correct identifications among witnesses shown target present showups. This reasoning led to the development of a novel instruction, specifically aimed at improving identification outcomes from showup procedures, known as the additional-opportunities instructions (AOI).

**Additional-Opportunities Instruction**

Because Smith et al. (2018) reasoned that witnesses shown a target-absent showup lower their decision criterion in order to avoid missing out an identifying the actual perpetrator, the AOI was developed to assuage these concerns. Specifically, the
AOI involves instructing witnesses that if they do not think the culprit is present in a showup, they will have an additional opportunity to view someone else later. To test their novel instruction, Smith et al. manipulated the AOI and the typical may-or-may-not admonition across witnesses who were shown either a target-present or target-absent showup procedure. Using a typical lab-based mock crime paradigm, participants watched a mock crime, completed a filler task, and were then randomly assigned to an experimental condition before being presented with a photograph of either the culprit or an innocent suspect.

Results found the traditional may-or-may-not admonition to have no effect on suspect identifications for either target-absent or target-present conditions. More importantly, however, results supported the authors’ main hypothesis: the AOI resulted in a significant decrease in false identifications (33% to 15%) but no significant decrease in correct identifications (56% to 50%), supporting their present-absent criteria discrepancy hypothesis. These beneficial outcomes were further supported by ROC analyses that indicated that that AOI increased witnesses’ ability to discriminate between guilty and innocent suspects. Therefore, initial research depicts promise for the AOI as a potential policy recommendation for showup procedures.

Eisen et al. (2017a) tested whether the benefits of the AOI extended to field-simulated showups. In one of their studies, mock witnesses were randomly assigned to a laboratory paradigm (in which participants were aware their identifications were part of a study) or a field-simulated paradigm (in which participants were not aware their identifications were part of a study). Participants viewed a crime and were presented with
either a culprit-absent or culprit-present showup. Half of participants were given the following pre-showup admonition:

You are under no obligation to identify this person as the suspect. We want to have guilty persons identified, but we also want to make sure that innocent persons are cleared of any suspicion in this matter. You should not draw any conclusions about a person just because he is in our custody, or handcuffed.

(Eisen et al., 2017a, p. 5)

The other half of participants were given the admonition plus the additional-opportunities instruction: “If you do not believe that this is the person you saw, we will continue to investigate and others may be shown to you later to attempt an identification.” Although the field-simulation paradigm resulted in increased choosing compared to the lab paradigm (suggesting that showups are especially likely to produce false identifications under realistic scenarios), the AOI did not significantly decrease false identifications in the field simulation paradigm, in contrast to results of Smith et al. (2018).

There are at least three possible explanations for this failure to replicate. First, the researchers might have experienced floor effects: suspect identifications were only at 12.5% in lab conditions without the AOI, leaving little room for the AOI to significantly reduce the false identification rate. Due to the logistic difficulty of running participants in a field-simulated paradigm, the researchers had a limited sample size and might have simply lacked the power to detect the decrease in the false identification rate. Second, the authors suggest the language variation used across their studies may have impacted results. In particular, participants in Smith et al.’s (2018) study were told they would have additional opportunities to make an identification later, whereas participants in Eisen et
al. (2017a) were told they *may* have additional opportunities for identifying the culprit. The weaker language used in Eisen et al.’s study might have diminished the effects of the AOI. Third, the methodologies differed across studies; although Smith et al. used a lab-based paradigm, Eisen et al. used a more realistic field simulation paradigm. It is therefore possible that some aspect of the realistic field simulation paradigm was responsible for the observed null effects. Why might the AOI fail to improve identification decisions using a more realistic paradigm?

**Witnesses’ Beliefs that Police Believe the Suspect is Guilty**

Taken together, the use of the AOI shows promise but its effectiveness has yet to generalize past laboratory studies. Additionally, although the AOI is believed to reduce false identifications by leading witnesses to avoid lowering their decision criterion in culprit-absent conditions by making them believe they will see additional suspects, it does not address one of the key sources of suggestiveness of showups: the witnesses’ beliefs that the police believe the suspect is indeed the culprit—a belief that should lead witnesses to identify the suspect presented to them. In fact, Eisen et al. (2017b) effectively manipulated this belief in their manipulation of the pre-admonition suggestion, in which some witnesses overheard that the suspect in the showup was "the guy" and, as mentioned above, they found this suggestion to inflate false identifications despite witnesses being admonished. If the main source of suggestiveness of showups is the witness’s belief that the police believe the suspect is guilty, then it would not be surprising that the AOI failed to reduce false identifications in the presence of suggestive cues such as Eisen et al.’s (2017b) “caught the guy” suggestion, as the AOI does not address a witness’s belief in the police’s belief of suspect guilt. In other words,
suggestiveness stemming from what witnesses believe the police believe may override any protective benefits of the AOI. Specifically, it is expected that the beneficial effects of the AOI will be minimized in conditions in which the witnesses believe the police believe the initial suspect is the perpetrator—which is common in real-world showup procedures but is not a factor in laboratory paradigms. This reason, rather than the small sample size, could be why Eisen et al. (2017a) was unable to replicate the beneficial effects of the AOI in their highly realistic field-simulated showups. To improve showup performance under real-world conditions, eyewitnesses may need additional instructions specifically aimed at undermining their belief that the police believe the suspect is the culprit. In fact, preliminary data from research by Charman and Kavetski (2016) has found promising effects in reducing false identifications in showups through an instruction that does just that.

**Preliminary Data for a Belief Instruction**

Charman and Kavetski (2016) examined effects of suggestiveness between showups and lineups by manipulating witnesses’ beliefs regarding police officers’ beliefs using a more ecologically valid single lineup paradigm (see Oriet & Fitzgerald, 2018). A single lineup paradigm involves the use of two identical mock crimes, filmed using two different perpetrators. Rather than a standard dual lineup paradigm, in which witnesses are shown a single perpetrator and then either a lineup that contains the perpetrator or one that contains an innocent suspect, a single lineup paradigm involves showing witnesses one of two perpetrators and then a lineup that contains one of the two perpetrators. Thus, each lineup can be either target-present (if the perpetrator in the video was the same as
the suspect in the lineup) or target-absent (if the perpetrator in the video was different from the suspect in the lineup).

Participants in Charman and Kavetski (2016) began by reading a cover story developed to ensure they thought they were watching an actual crime. Participants were then shown one of two mock crimes and were randomly assigned to view either a lineup or a showup that was either target-present or target-absent. Suggestiveness was manipulated by describing the person in the photo as either a suspect (suggestive) or simply a bystander (not suggestive). Specifically, witnesses in the suspect condition were told “This photo is one of several people that were in the area shortly after the incident. Police found this person behaving suspiciously and believed the evidence pointed to him.” In the bystander condition, witnesses were told “This photo is one of several people that were in the area shortly after the incident. At this point, there was no evidence pointing to any suspect.” For those assigned to view lineups, the instructions were nearly identical with the exception of “This photo” being changed to “These photos.”

As predicted, and consistent with past research, the researchers found showups to produce significantly more false identifications (19%) than lineups (7%). More relevant to the present study, however, the data also showed a dramatic reduction in showup false identifications from those who were told the person was a suspect (30%) to those who believed the person was merely a bystander (7%), without a concomitant reduction in correct identifications (74% vs 79% respectively). In fact, this belief manipulation decreased showup false identifications to a rate similar to that of lineups, suggesting the suggestiveness of showup procedures is due to an inherent belief that police believe the suspect is guilty.
The Present Study

Although Smith et al.’s (2018) AOI study demonstrated the effectiveness of an instruction in mitigating the inherent suggestiveness of showup procedures, it was conducted under laboratory settings in which witnesses presumably did not believe the crime was real, and results were not replicated using a more ecologically valid field-simulated paradigm in which witnesses did believe a real crime was committed (Eisen et al., 2017a). One possible reason the AOI failed to replicate under more ecologically valid conditions could be that simply telling witnesses they will have the opportunity to see additional people does not address the leading factor behind the suggestiveness of showups—that witnesses believe the police believe the suspect is guilty. As seen in Charman and Kavetski’s (2016) preliminary data, showups result in higher false identification rates when this police belief is present but is reduced to the level of a more protective lineup when they are instructed that there is no reason to believe the person in the photo is a suspect. Therefore, whereas the AOI might be expected to prove beneficial in laboratory paradigms in which participants know there was no actual crime (and thus there are no police beliefs regarding the suspect), we might expect it to fail under more ecologically valid conditions in which participants believe there was an actual crime and real police procured a suspect. However, an instruction aimed at undermining witnesses’ beliefs that the police believe the suspect is guilty should show promise within more ecologically valid showup paradigms that better generalize to real world showup procedures.

Although Charman and Kavetski’s (2016) preliminary data have shown promise when manipulating witnesses’ perceptions of police belief, their study has two important
shortcomings. First, they lacked a control group of witnesses who were not given any statement regarding police beliefs. Consequently, it is unknown whether the bystander instruction reduced false identifications or whether the suspect instruction increased false identifications. Second, they lacked the power necessary to perform ROC analyses, and were thus unable to determine whether their bystander instruction increased discriminability. The primary goal of the proposed study, therefore, is to test whether a bystander instruction increases witnesses’ ability to discriminate between guilty and innocent suspects in a showup procedure by (a) including a control condition of witnesses who receive no instruction regarding police beliefs, and (b) including a large enough sample size to conduct ROC analyses.

The proposed study helps to elucidate the theoretical reason behind the suggestiveness of showups. For example, if showups are suggestive due to eyewitnesses’ expectations that they will only be shown one person, then the AOI, which informs witnesses they will be shown other suspects, should increase discriminability regardless of witnesses’ perceptions of police guilt. However, if showups are suggestive due to witnesses’ beliefs that the police believe the suspect is guilty, as we are arguing, the AOI should fail to reduce false identification in circumstances where witnesses hold such a belief. Instead, a bystander instruction, which undermines this belief, should increase discriminability.

A standard lab-based paradigm is an inadequate way to examine these goals because witnesses in lab studies are aware there is no actual criminal, and thus lack the inherent perceptions of police guilt that are common to a real-world showup procedure. However, a field-simulated paradigm, which leads participants to believe they are
attempting an actual showup identification of a thief can be problematic due to (a) the
difficulty due to its extremely time-consuming nature, and (b) its difficulty in maintaining
experimental control across nested participant groups. Therefore, the proposed study also
introduces a novel experimental paradigm to test our hypotheses, which maintains high
levels of experimental control while maintaining ecological validity. Specifically, this
paradigm uses an involved cover story to lead witnesses to believe they are watching
actual surveillance video of a crime and that they are being presented with a showup that
was shown to the actual witness in the case, which, unlike current showup paradigms in
which witnesses know the crime is staged, should allow participants in the current study
to believe that the police believe the suspect is guilty—a prerequisite for examining the
effectiveness of our manipulated belief instruction. If results support our hypotheses, this
novel paradigm may become a standard methodology in showup research.

It is hypothesized, based on Charman and Kavetski’s (2016) results suggesting
that witnesses’ beliefs of police’s belief in suspect guilt is largely responsible for
showups’ suggestiveness, that false identification rates will be high among participants
given no instructions regarding police beliefs (controls) and those instructed that police
believe the suspect is guilty, due to a lowering of their decision criterion specifically in
target-absent conditions (i.e., the present/absent discrepancy hypothesis). However, we
expect participants led to believe that the police do not believe the suspect to be guilty
(because s/he is simply a bystander) to avoid lowering their decision criteria in target-
absent showups, which will be reflected in a reduced rate of false identifications.
However, because witnesses are not expected to lower their decision criteria from target-
present showups, we hypothesize that hit rates will remain relatively stable regardless of
belief instruction. As a result, we expect that the bystander instruction will result in significantly better discriminability as analyzed via ROC analyses than either the control condition or the suspect instruction condition. Furthermore, we expect the AOI to be less effective at increasing discriminability within our paradigm, due to its failure to undermine witnesses’ beliefs about police beliefs.

CHAPTER II

METHODS

Design

This study conforms to a 3 (belief instruction: no instruction/suspect instruction/bystander instruction) X 2 (additional-opportunities instruction: given vs. not given) X 2 (target status: target-present vs. target-absent) between-subjects design. Participants were randomly assigned to all conditions. The present study was pre-registered on the Open Science Framework\textsuperscript{1}.

Participants

To examine differences in discriminability across police belief conditions, we planned to conduct receiver-operator characteristic (ROC) analyses, which tend to require upwards of 300 participants per experimental condition (e.g., Wetmore, 2015). Given target presence is a variable used while calculating ROC curves, we were left with our 3 (belief instruction: control/suspect/bystander) x 2 (AOI: given or not) design, which resulted in six manipulated conditions. Therefore we aimed to collect 1800 participants total. Funding to collect this large of a sample was provided by Florida International

\textsuperscript{1}https://osf.io/q49bn
University’s psychology department’s SEED grant. Utilizing these funds, participants were paid $0.50 for their successful completion of the Qualtrics survey. Understanding not all collected participants would pass our manipulation check, we collected as many participants as our funding would allow.

Two thousand nine hundred and thirty-one (2,931) participants were recruited through Amazon Mechanical Turk (MTurk). Of those recruited, 684 were missing data for our dependent measures and an additional 433 failed our manipulation check, described below. Therefore, one thousand seven hundred and seventeen (1117) were excluded from data analysis after not meeting our exclusion criteria, leaving us with 1,814 participants included in our final analyses. Participants were required to be at least 18 years of age and to reside in the United States.

**Exclusion criteria.** Participants were excluded from data analysis if they either (a) did not complete the survey up to the dependent measures, or (b) failed the manipulation check. The manipulation check consisted of three multiple-choice questions asking participants to identify which pre-showup instructions they had read. The first manipulation check question asked participants whether they were given a standard admonition (the person may or may not be the perpetrator), the second question asked participants what belief manipulation question they had received, and the final question asked participants whether they received the AOI manipulation (see Appendix H for exact questions). Participants who could not correctly recall the instructions they were given were excluded from final data analysis.

**Demographics.** A small number of participants were missing demographic data ($n = 59$). Of those who completed their demographics, mean participant age was 39.85
37. Participant gender consisted of 1,208 (66.6%) identifying as female, 538 (29.7%) identifying as male, and 9 (0.5%) self-identifying as non-binary. The ethnic breakdown was as follows; 1,324 White/Non-Hispanics (73%), 144 Black/African Americans (7.9%), 121 Hispanic/Latinos (6.7%), 114 Asian/Pacific Islander (6.3%), 7 Native American (0.4%), and 45 responded “Other” (2.5%). In response to the highest level of education completed, 510 (28.1%) reported high school, 282 (15.5%) reported associate’s degree, 570 (31.4%) reported bachelor’s degree, 277 (15.3%) reported master’s degree, and 116 (6.4%) reported “Other.”

**Criminal Justice Experience.** Several questions regarding our sample’s experience with the criminal justice system were asked. In response to the question “Are you or a member of your immediate family a law enforcement officer?” 1,621 (89.4%) participants reported “No.” One hundred and fifteen (6.3%) individuals reported having a member of law enforcement in their family and 19 (1%) of participants reported being a member of law enforcement. In response to the questions “How familiar are you with the United States criminal justice system?” 72 (4%) reported being “extremely familiar”, 276 (15.2%) reported being “very familiar”, 846 (46.75%) reported being “moderately familiar”, 498 (27.5%) reported being “slightly familiar”, and 62 reported being “not familiar at all.”

For the remainder of the criminal justice questions two additional participants were missing data ($n = 61$). In response to the question “Do you have first-hand experience with the criminal justice system?” 1,192 (65.7%) participants had no first-hand experience with the criminal justice system, 66 (3.6%) preferred not to answer and the remaining 435 (27.3%) participants reported, “Yes.” Of those who reported “yes”, 50
(2.8%) reported they have been a suspect, 72 (4%) reported they had been a suspect and later a defendant, 187 (10.3%) reported they have been a victim, 89 (4.9%) reported they had been a witness, and the remaining 97 (5.3%) reported to be involved in some professional capacity (law enforcement, attorney, etc.).

When asked if they have ever been asked to make a real-world criminal identification, the majority of participants (1,540; 85%) reported “No”, 213 (11.7%) reported they had (“Yes”) and 61 (3.4%) failed to respond. The majority of our sample (1455; 80.2%), had never been arrested, 265 (14.6%) participants had been arrested, and 33 (1.8%) preferred not to answer. In response to the question “How trustworthy do you find police officers to be?” 149 (8.2%) reported police as being “not trustworthy at all”, 332 (18.3%) reported police as “slightly trustworthy”, 649 (35.8%) reported police as being “moderately trustworthy”, 512 (28.2%) reported police as being “very trustworthy”, and 111 (6.1%) reported police as being “extremely trustworthy.”

Materials

**Targets videos.** We adapted Oriet and Fitzgerald’s (2018) single lineup paradigm to showup procedures. Each participant was presented with one of two target videos and then a showup of one of the two targets; each target acted as the other’s innocent suspect for the target-absent showups. Both targets matched the same general description of the perpetrator, with both targets being male, Hispanic, in their early 20s, clean-shaven with short, dark hair. Each target video was shot from the perspective of a witness filming the crime from a cell phone in a nearby car. Each video depicted the targets walking through an overflow parking lot on Florida International University’s MMC campus, where they were shown looking through the windows of several cars. When the targets reach the
third car they are shown looking around, opening the car door and stealing a laptop out of the passenger side door. The videos were identical other than the different targets and each lasted approximately 40s.

**Target Photos.** Each showup procedure involved participants being presented with one of the two targets. Participants in target-present conditions viewed a photo of the target that appeared in the video they had seen, whereas participants in target-absent conditions viewed a photo of the target that appeared in the video they had not seen. Participants were shown the target/innocent suspect standing next to police officers, in front of a police car, with the police officers facing the target and looking sternly at him. The target was looking directly at the camera, with their full body in view, approximately 10 feet away from the photographer. Both men wore black t-shirts and jeans—the same clothes as depicted in the mock crime videos. Each photograph was approximately 10cm x 7cm. See Appendix A for the photo of target 1 and Appendix B for the photo of target 2. These photos were selected because they are representative of a traditional showup: a suspect being detained by police but not yet arrested.

**Filler Task.** After the encoding task, participants completed a visual search task presented to them through Qualtrics. Participants were shown two complex scenes and asked three open-ended questions regarding the image (Appendix C). These questions were developed to act as filler tasks, as well as a general attention check for online participants. Each open-ended question was timed to last 1 minute and would automatically progress to the next question after time elapsed; therefore this part of the filler task lasted 6 minutes total (3 questions for 2 complex scenes). After the visual search tasks, participants were automatically progressed to six survey questions regarding
the mock crime and the perpetrator that they were required to answer (Appendix D). The entirety of the filler task lasted approximately 10 minutes.

**Procedure**

The entirety of the present study was conducted using a Qualtrics link shared with Amazon Mechanical Turk (MTurk) participants. The link was shared using the platform CloudResearch, which vetted MTurk workers for quality and charged less in fees than recruiting directly from MTurk. All participants were first presented with an online version of an informed consent form. Only participants who selected “Yes- I consent to participate” continued on with the survey; those who responded “No- I DO NOT consent” were immediately taken to the end of the survey.

Participants then proceeded to a page containing our cover story (Appendix E). The cover story was designed to ensure some level of ecological validity through an online format by convincing participants that they were aiding a real police investigation in which actual police were involved. This was a pivotal point of our design, as we required witnesses to believe that the police held some belief in the suspect’s guilt.

Participants were told that the video they were about to be shown was that of a real crime, captured by a concerned citizen who submitted the video to Miami-Dade Crime Stoppers anonymously. Participants were then told they would be testing a new identification policy created in collaboration with the FIU Psychology Department and Miami-Dade Police. The policy read as follows:

The psychology department at FIU is collaborating with the Miami-Dade Police to test a new identification policy. In accordance with this new policy, police must take photos of the area and all people in the area. Shortly after photos are taken,
they then must crowd source identifications made from Crime Stoppers footage. As part of the present study, you will be a part of the crowd sourced identification procedure. You are about to be shown the Crime Stoppers footage mentioned above. Please pay attention to the video, as you will be asked to make a crowd sourced identification later (Appendix E).

Participants then watched one of two mock crimes and were then immediately directed to the filler tasks. Following the filler tasks, participants proceeded to the identification task. Participants were instructed “You are about to be shown a photograph of someone who may or may not be the thief from the video you witnessed earlier. In accordance with the new crowd sourcing policy, this person’s photo was taken because he was one of a few people near the scene of the crime, shortly after the crime had occurred.” According to our belief instruction condition, those in the control condition were given no further information. Those in the suspect condition were also instructed “However, police believed he was behaving suspiciously and believed the evidence pointed to him.” Those in the bystander condition were instead instructed “However, police noted he was not behaving suspiciously and do not have evidence pointing towards him.” Those who were also randomly assigned to be given the AOI were also instructed “If you do not believe the man in the photo is the man you saw, other photos may be shown later for you to attempt to identify the man from the video.” For the exact phrasing for each condition, see Appendix F.

Following their instructions, participants were randomly assigned to see one of two targets and were asked “Is this man in the photo the same man from the video who stole the laptop from a car?” and were given a “Yes”/“No” multiple choice response
option. Participants then rated their confidence using a 0-100% confident snap-grid (Appendix G). After their identification decision and confidence rating, participants were presented with our three manipulation check questions, a question regarding any suspicions, and a final questionnaire consisting of follow-up questions regarding their identification decisions as well as their demographics (Appendix I). Participants were then debriefed (Appendix J), given their MTurk ID number and thanked for their participation. On average, the entire procedure took 21 minutes to complete.

CHAPTER IV
RESULTS

Manipulation check

Participants’ beliefs that police believe the suspect is guilty. To examine whether belief instruction affected participants’ perceptions of police’s beliefs of suspect guilt in the intended way, a one-way ANOVA was conducted to compare mean guilt perception ratings on a Likert scale ranging from 1 (police believe they are not guilty at all) to 7 (police believe they are extremely guilty) for control, suspect, and bystander conditions. There was a significant effect found for belief instruction on perceptions of police’s beliefs of suspect guilt, $F(2, 1753) = 310.9, p < .001, \eta^2 = .26$. Post hoc comparisons using Tukey’s LSD test indicated that perceptions of police’s beliefs of suspect guilt were significantly lower for those given the bystander instruction ($M = 3.31, SD = 1.62$) compared to those in the control condition ($M = 4.57, SD = 1.40$), $p < .001$ and the suspect condition ($M = 5.36, SD = 1.63$), $p < .001$. Additionally, perceptions of guilt were significantly higher in and the suspect condition compared to the control condition, $p < .001$. There was no significant effect for participants’ perceptions of
police’s belief of suspect guilt between those given the AOI \( M = 4.48, SD = 1.62 \) and those who were not \( M = 4.35, SD = 1.65 \), \( t(1754) = -1.59, p = .110 \).

**Participants beliefs that the suspect is guilty.** To examine whether belief instruction affected participants’ own beliefs of suspect guilt, a one-way ANOVA was conducted to compare mean guilt ratings on a Likert scale ranging from 1 (they are not guilty at all) to 7 (they are extremely guilty) for control, suspect, and bystander conditions. There was a significant effect found for belief instruction on participants’ beliefs of suspect guilt, \( F(2, 1753) = 8.25, p < .001, \eta^2 = .01 \). Post hoc comparisons using Tukey’s LSD test indicated that participants’ beliefs of suspect guilt were significantly lower for those given the bystander instruction \( M = 3.33, SD = 1.76 \) compared to those given the suspect instruction \( M = 3.75, SD = 1.74 \), \( p < .001 \), but not compared to the control condition \( M = 3.51, SD = 1.74 \), \( p = .08 \). Participants’ belief of suspect guilt also did not significantly differ between those given the suspect instruction and those in the control condition, \( p = .21 \). Additionally, there was no significant effect on participants’ belief of suspect guilt between those given the AOI \( M = 3.61, SD = 1.76 \) and those who were not \( M = 3.45, SD = 1.76 \), \( t(1754) = -1.84, p = .07 \).

**Descriptives**

Overall identification rates, as well as identification rates broken down by target, are depicted in Table 1. Participants made an identification 66.3% of the time. There was a significant difference in participants’ choosing behavior, such that participants shown target-present showups made correct identifications 55.1% of the time, whereas participants shown target-absent showups made false identifications 24.7% of the time, \( \chi^2 (1) = 174.61, p < .001, \phi = .31 \).
**Target differences.** We compared the identification rates between the two targets. Witnesses were significantly less likely to correct identify Target 1 (48.3%) compared to Target 2 (62.2%), $\chi^2 (1) = 17.86, p < .001, \varphi = .13$. False identification rates did not differ significantly between Target 1 (26.9%) and Target 2 (22.6%), $\chi^2 (1) = 2.26, p = .13, \varphi = .05$. All subsequent analyses collapse across target.

**Comparing Correct and False Identifications with Logistic Regression Analysis**

**Suspect Belief Instruction.** To examine the effect of witness belief on choosing, two hierarchical binary logistic regression analyses were conducted. The first analysis, which examined the effect of the suspect instruction manipulation, regressed choosing on the AOI (given vs. not given), suspect belief condition (control vs. suspect instruction), target presence (target-present vs. target-absent), and the various two-way and three-way interactions. Main effects were entered into the first block of the analysis, all two-way interactions were entered into the second block of the analysis, and the three-way interaction was entered into the third and final block of the analysis (see Table 3). There was no significant three-way interaction between the AOI, suspect belief, and target presence. Similarly, there were no significant two-way interactions between any of the variables. Additionally, our model did not significantly improve between block 1 and block 2, $\chi^2 (3) = 1.15, p = .76$, nor between block 2 and block 3, $\chi^2 (1) = .19, p = .67$. Therefore, our final model contained only main effects.

There was no main effect of the AOI, indicating that the AOI did not significantly impact the frequency with which eyewitnesses identified the suspect. However, there was a significant main effect of target presence on choosing, $B = 1.07, SE = .25$, Wald = 18.57, $p < .01$, OR = 2.91, 95% CI [1.78, 4.72]. Participants who were shown target-
present showups were more likely to make an identification decision (56%) than those shown target-absent showups (28%). There was also a significant main effect of our suspect belief instruction on choosing, $B = .310, SE = .12, \text{Wald} = 6.56, p = .01, OR = 1.36, 95\% \text{ CI} [1.08, 1.73]$. Participants who were given the suspect belief instruction (46%) were more likely to identify the suspect than those who did not receive the instruction (39%). These results indicated that our suspect instruction led to a more suggestive showup procedure, which inflated both correct and false identifications.

**Bystander Belief Instruction.** The second analysis, which examined the effect of the bystander instruction manipulation, regressed choosing on the AOI (given vs. not given), bystander belief condition (control vs. bystander instruction), target presence (target-present vs. target-absent), and the various two-way and three-way interactions. Again, all main effects were entered into the first block of the analysis, all two-way interactions into the second block of the analysis, and the three-way interaction into the third and final block of the analysis (see Table 4). There was no significant improvement between block 1 and block 2, $\chi^2 (3) = 4.91, p = .18$, or between block 2 and block 3, $\chi^2 (1) = .43, p = .51$. The highest-order significant interaction was a two-way interaction; therefore, our final model included main effects and two-way interactions. There was no significant three-way interaction between the AOI, bystander belief instruction, and target presence. Additionally, there was no significant two-way interaction between AOI and target presence. Nor was there a two-way interaction between AOI and bystander belief instruction.

As hypothesized, there was a significant two-way interaction found between target presence and our bystander belief instruction, $B = .559, SE = .26, \text{Wald} = 4.68 p$
Follow-up analyses found that participants that were presented with target-absent showups were significantly less likely to make a false identification when given our bystander belief instruction (17.3%) compared to control instructions (26.1%), $\chi^2 (1) = 6.70, p = .01, \phi = .11$. There was no significant difference in correct identifications from target-present showups between those in the bystander (51.7%) and control (52.9%) conditions, $\chi^2 (1) = .084, p = .77, \phi = .03$. These results depict a promising picture for the bystander belief instruction, as it shows our bystander instruction decreases the number of false identifications without also decreasing the number of correct identifications.

Additionally, there was no main effect of the AOI on choosing, $B = -.098, SE = .13, Wald = .61, p = .44, OR = .907, 95\% \text{ CI}[.71, 1.16]$. These results support our hypotheses that the AOI would prove ineffective in our paradigm where witnesses’ beliefs in police belief of guilt are present.

**Receiver Operator Characteristic (ROC) Analysis**

ROC curves were constructed to examine which of our manipulated conditions produced optimal discriminability. ROC curves involve plotting the proportion of correct identifications as a function of the proportion of false identifications at various levels of decision criteria (for which witnesses’ confidence reports serve as a proxy). This analysis produces a family of correct identification/false identification pairs that can be described with a curve for each manipulated condition. The leftmost point on the ROC curve represents the proportion of correct and false identifications made with 100% confidence. The second-to-leftmost point represents the proportion of correct and false identifications made with 100% or 90% confidence. This pattern continues until a point for each level of
confidence has been plotted. ROC curves can then be compared to one another and the ROC curve that produces the highest ROC curve is the procedure that produces the best tradeoff between culprit and innocent-suspect identifications (i.e., the procedure that results in the greatest discriminability) and which is therefore the preferred procedure.

We compared ROCs by examining partial area under the curve (pAUC) as measures of each procedure’s utility. However, when comparing two procedures that produce very different false identification rates (i.e., the rightmost point on the x-axis), pAUCs are misleading because the areas under the curves of the various procedures cover very different areas in the ROC space. Therefore, to compare pAUCs for ROC curves when false identification rates differ, one can either (a) compare only the portion of the range covered by the less extensive of the two ROCs (truncated pAUC), or (b) compare the portion of the range covered by the more extensive of the ROCs by extrapolating the ROC curve of the truncated procedure (extrapolated pAUC).

To date, there has been no clear advantage to using one of these pAUC measures over the other and both have been criticized as poor measures of identification procedures utility (Smith et al., 2019). These criticisms led to the development of deviation from perfect performance (DPP) as a measure of utility. DPP is recommended when two procedures have large discrepancies in false identification rates, as is true with some of our data. As the name describes, DPP measures a procedure’s deviation from perfect performance, which would be represented in ROC space as a curve that touches the upper leftmost point on the ROC graph. To calculate DPP, a DPP (c) point is calculated for each point (representing each level of confidence) on the ROC curve using the following equation:
\[ DPP (c) = [1-\text{Culprit ID rate}(c)] + [\text{Innocent Suspect ID rate}(c)] \]

Average DPP, which is simply the average of all \( DPP (c) \) values, is the preferred way to measure DPP, although other calculations such as high-confidence DPP, terminal-point DPP, and minimum DPP have been suggested as alternative measures (Smith et al., 2019). For the purposes of our research, average DPP was used to compare our constructed ROCs. The lower the DPP value is, the closer it is to perfect performance, making the procedure with the lower DPP value the preferred procedure. DPP is conceptually similar to pAUC without the caveat of being yoked to any specific area of ROC space. Average DPP was calculated using code developed by Smith and colleagues (2019) provided through Open Science Framework\(^2\). Partial area under the curve (pAUC) was analyzed using the “pROC” package in R (Robin et al., 2011).

We first examined the effect of the AOI (collapsed across belief condition) by constructing an ROC for all participants given the AOI \((n = 933)\) and one for all participants not given the AOI \((n = 881)\). These ROC curves are depicted in Figure 1. Because false identification rates were similar between these two groups, we only examined truncated pAUC and DPP. Table 5 displays all measures and inferential statistics for comparisons. As depicted by the overlapping ROC curves, witnesses given the AOI were no better able to discriminate guilty from innocent suspects than witnesses not given the AOI. Because there was no significant difference found between these two ROCs, our remaining analyses collapsed across our AOI manipulation.

\(^2\) https://osf.io/9ytxw/?view_only=ffcc9a0e5bce48c2ba2bbefd5a07a66b
We examined the effect of our belief manipulation by constructing three ROC curves, one for each of our manipulated belief instruction conditions. We first compared our control belief condition (n = 623) to the suspect belief condition (n = 593). Figure 2 depicts the ROC curves. As shown in Table 6, witnesses’ abilities to discriminate between guilty and innocent suspects did not significantly differ between controls and those given our suspect belief instruction, whether calculated via truncated pAUC, extrapolated pAUC, or average DPP.

Next, we compared our control belief condition ROC curve to the ROC for our bystander belief condition (n = 589). Figure 3 depicts the ROC curves. As shown in Table 7, witnesses in our bystander condition were significantly better able to discriminate between guilty and innocent suspects than controls, whether calculated via truncated pAUC or extrapolated pAUC measures. Average DPP values were marginally significantly different between our control ROC and bystander ROC (p = .051). These results are a promising sign our bystander instruction may act as a potential intervention that may be used towards the goal of reducing the suggestiveness of showup identification procedures.

Confidence-Accuracy Characteristic (CAC) Analysis

CAC analyses, which include only witnesses who made suspect identifications, examine the likelihood that a suspect identification is accurate at varying levels of confidence. Suspect identifier (n = 727) accuracy per confidence bin (j) was calculated as

$$a_j = \frac{\text{# accurate suspect IDs}}{\text{# accurate suspect IDs} + \text{# inaccurate suspect IDs}}.$$  

A CAC curve was then constructed examining the accuracy of suspect identifications at low (0-60%), medium (70-80%), and high (90-100%) confidence level for each cell of our 3 (belief instruction:
control/suspect/bystander) x 2 (AOI: given vs. not given) design (see Figure 5). In general, confidence for suspect identifiers was not strongly related to accuracy. For five of our six CAC curves, accuracy was not significantly different as a function of witness confidence (Table 8). Confidence was only significantly related to accuracy among suspect identifiers in the suspect belief instruction/AOI given condition. Here we found a significant difference between the proportion of accurate suspect identifiers for those in the low confidence bin (.48) compared to those in the medium confidence bin (.75), $\chi^2(1) = 7.83, p = .01, \phi_c = 0.27$, but not between those in the medium confidence bin to those in the high confidence bin (.77), $\chi^2(1) = 0.05, p = 0.84, \phi_c = 0.02$.

Because high-confidence suspect identifiers are the witnesses most likely to testify at trial (Mickes, 2015), we examined the accuracy rate of the high-confidence suspect identifiers across our six manipulated conditions to determine whether either belief instruction or AOI affected the trustworthiness of these identifications. Again, there was no significant difference found between the proportions of these high confidence participants, $\chi^2(5) = 6.43 p = 0.27, \phi_c = 0.20$.

CHAPTER IV
DISCUSSION

The present study had two primary goals: (1) to test whether witnesses’ beliefs of police belief of guilt contributes to the suggestiveness of showups by examining the effect of a pre-showup instruction aimed at undermining that belief, and (2) to examine whether the beneficial effects of the AOI generalize past laboratory studies to instances in which witnesses believe police believe the suspect is guilty. In relation to the first goal, a bystander belief instruction increased eyewitnesses’ discriminability, whereas a suspect
belief instruction did not significantly affect discriminability. In relation to the second goal, the AOI did not significantly increase eyewitness discriminability. Both results support our hypotheses. We also found no evidence for a confidence-accuracy relationship among our suspect identifiers.

**Suspect and Bystander Belief Instructions**

Our first research goal was to further examine whether a witness’s belief that police believe the suspect is guilty contributes to the suggestiveness of showup identification procedures, as we predicted. We examined this by manipulating witness belief via our suspect and bystander belief instructions. Our suspect and bystander instructions successfully manipulated witnesses’ beliefs, such that the suspect belief instruction increased witnesses’ beliefs that the police believed the suspect was guilty and our bystander belief instruction decreased this belief compared to control participants. This pattern persisted when witnesses were asked their own perceptions of suspect guilt, suggesting a strong police belief impacts a witness’s own perceptions of guilt as well.

We predicted that our bystander instruction would result in a beneficial tradeoff between the rates of culprit and innocent suspect identifications. These hypotheses were supported by our data such that our bystander belief instruction led to a significant reduction in false identifications compared to our control showup procedure, without also reducing correct identification rates. These findings were again reflected in our ROC analysis, which showed that our bystander belief instruction improved discriminability among eyewitnesses compared to controls. These results suggest that undermining the suggestive influence of a witness’s belief that police believe the suspect is guilty leads to more diagnostic eyewitness decisions from showup procedures.
Related to these findings, our suspect belief instruction bolstered both correct identification decisions and false identification rates compared to our control condition. Although we made no formal hypotheses regarding our suspect instruction, the increase in choosing overall found amongst those given our suspect belief instruction is consistent with this instruction increasing perceptions of police guilt. Our data show that the more a witness believes the police believe the suspect is guilty, the more liberal their decision criterion becomes. These results are similar to those of Eisen et al. (2017a), who found witnesses who overheard a police officer stating that he “caught the guy” prior to being administered a showup were nearly twice as likely to make an identification compared to those who did not overhear this suggestion. Eisen et al. also found that in field simulated showups nearly 50% of eyewitnesses felt pressured to choose a suspect. Broken down by condition, this pressure was reported significantly more often by eyewitnesses who heard the “caught the guy suggestion” (43.4%) compared to controls (27.8%). Although our hybrid paradigm did not involve witnesses believing they were making real identifications (as Eisen et al.’s participants did using a field simulation paradigm), our witnesses were nonetheless led to believe the crime and suspects were real, which, we argue, created a similar pressure to choose, which explains the liberal criterion shift found among our eyewitnesses given the suspect instruction. Taken together, the detrimental effects of any added suggestion to witnesses that police believe the suspect is guilty are clear. These results support the updated white paper recommendation on showups, which calls for the elimination of suggestive cues during showup procedures (Wells et al., 2020).
Additional Opportunities Instruction

Our second research goal was to examine whether the beneficial effects of the AOI generalize past laboratory studies to instances in which witnesses believe police believe the suspect is guilty. We found no significant effect of the AOI on hit rates or false alarm rates in any of our analyses, supporting our hypothesis that the AOI instruction would fail to undermine witnesses’ beliefs that the police believe in suspect guilt and thus fail to improve identification outcomes from real-world showup procedures. This notion is supported through our initial manipulation check analyses in which we directly measured this belief across AOI conditions: The AOI failed to significantly affect participants’ ratings of both their belief that the police believe the suspect is guilty as well as their own perceptions of suspect guilt. Therefore, the AOI had no bearing on witnesses’ perceptions of guilt regarding the suspect.

These results support our hypothesis that the AOI would have minimal effect when presented outside a laboratory setting, where witnesses experience other suggestive cues beyond just seeing a single suspect. Within a lab study, witnesses are aware that there are no actual police, and they therefore have no beliefs regarding police beliefs. Under these conditions, showups might be suggestive due to the witness only being presented with a single suspect, and the AOI could therefore reduce that suggestiveness. But real-world witnesses are aware that the police might believe the suspect is guilty. The AOI, which only instructs witnesses that they may have additional opportunities to identify other suspects, does not address these suggestive influences, and, we contend, therefore fails to undermine the suggestiveness of real-world showup procedures. However, our bystander instruction does undermine this belief. Therefore, as we
demonstrated in the present study using a paradigm in which participants believed there were actual police, the AOI did not significantly affect eyewitnesses’ ability to discriminate between guilty and innocent suspects, but our bystander belief instruction did.

Our findings are also consistent with Eisen et al.’s (2017) failure to replicate, using a field-simulated study, Smith et al.’s (2018) finding that the AOI improved discriminability. Although the authors argued their null effects of the AOI were a consequence of their small sample size ($N = 365$), we argue their null findings were likely an artifact of the fact the AOI was unable to override witnesses’ beliefs in police beliefs in the suspect’s guilt what was inherent to their field-simulated paradigm. Our results, which found null effects of the AOI using a much larger sample size ($N = 1814$), argue against the AOI’s failure being solely the result of a lack of power. Taken together, our results suggest the AOI, which has been recommended for use during showup procedures via the updated white paper on eyewitness identification (Wells et al., 2020) may be an inefficient means of protecting innocent suspects from being incorrectly identified in real world investigations in which witnesses hold this highly suggestive belief that the police believe the suspect is guilty.

**Theoretical Implications**

To date, there are few research studies that have examined the underlying mechanisms behind the suggestiveness inherent to showups. What has been studied has primarily focused on the single suspect presentation being the driving force behind the procedure’s suggestiveness. However, preliminary data by Charman and Kavetski (2016) found evidence that showups are largely suggestive due to witnesses’ beliefs of police
belief, which inflates choosing from target-absent showups. However, the researchers failed to include a control group of witnesses who did not hold beliefs regarding the police. In the current study, we added a control group, which allowed us to parse apart whether the bystander instruction mitigated false identification in showups or whether the suspect instruction simply inflated false identifications by creating an even more suggestive showup procedure than what is typically conducted in real world investigations. We found that our bystander instruction was clearly successful in improving the accuracy and diagnosticity of identifications from showup procedures compared to a control condition. Our suspect instruction manipulation, on the other hand, resulted in higher choosing overall without any change to discriminability. These data suggest the impressive reduction in false alarms between Charman and Kavetski’s (2016) initial suspect (30%) and bystander (7%) manipulation was likely an effect of both the bystander instruction undermining this suggestive witness belief and the suspect instruction bolstering this suggestive witness belief. Additionally, the large sample size we were able to achieve through the use of our novel hybrid paradigm allowed us to conduct ROC analyses, which was not accomplished in Charman and Kavetski’s initial study. It was through our ROC analyses that we were able to show that the bystander instruction not only reduces false identifications, but also improves discriminability in showups. These results provide strong support for the bystander instruction as a potential intervention for showup procedures and also supports our notion that it is the witnesses’ beliefs that police believe the suspect is guilty that drives the suggestiveness of a showup procedure.
**Lineup Superiority.** While this witness belief appears to be the main factor impacting a showup’s suggestiveness, this is not to say that having only a single suspect does not impact witnesses’ decision making at all. In fact, it is having only one suspect and the inability to conduct the procedure blind that allow witnesses’ beliefs about police beliefs to negatively affect their decision-making. Because our research goals were specific to understanding the impact of witnesses’ beliefs about police beliefs among showup procedures, we did not also examine the impact of our belief instruction on lineup identifications. In fact, if a lineup procedure is conducted properly, a witness should not know which lineup member is the suspect; therefore, neither the suspect nor the bystander instruction, which inform witnesses whether police believe the suspect is guilty or not, should lead witnesses to selectively identify a particular lineup member. Consequently, witnesses’ beliefs that the police believe the suspect is guilty should be much less likely to affect lineup identifications, as, in contrast to a showup, witnesses do not know who the suspect is. This is exactly the pattern of results found in Charman and Kavetski’s (2016) preliminary study, in which they directly compared the effect of a bystander instruction on showup and lineup identifications. Whereas they found the bystander instruction to reduce false identifications in showups without also reducing correct identifications (as we replicated in the present study), these benefits did not extend towards lineups procedures, as no significant differences in suspect identification rates were found between witnesses given the suspect or bystander instructions among witnesses shown lineups. In other words, the presence of fillers protected innocent suspects from the suggestive influence of the suspect instruction, consistent with Wells et al.’s (2015) filler siphoning theory. These findings fit with decades of research showing
lineups are a superior identification procedure compared to showups due to the protective effects fillers have on innocent suspects (Clark, 2012; Steblay et al., 2003).

**Present-Absent Discrepancy Theory.** The beneficial effect of our bystander instruction manipulation on eyewitness discriminability is consistent with Smith et al.’s (2018) present-absent discrepancy theory. This theory argues that eyewitnesses who encounter an innocent suspect experience a relatively weak match to memory and therefore drop their decision criterion to avoid missing out on identifying the perpetrator, resulting in witnesses using a higher decision criterion for target-present showups than target-absent showups. Their demonstration that the AOI reduced false identifications without reducing correct identifications (thereby increasing discriminability) is consistent with this hypothesis.

Similarly, our bystander instruction also resulted in lower false identification rates without affecting correct identification rates, resulting in significantly improved discriminability. This pattern of results reflects a discrepant-criteria model, and suggests that the bystander instruction improved decision-making by specifically leading witnesses to avoid dropping their decision criterion when viewing a target-absent showup. These results were expected, as although the exact mechanism of action is different between the two instructions, the ultimate utility of the bystander instruction is similar to that of the AOI. Specifically, the AOI was developed as a tool for mitigating the propensity of target-absent witnesses to misattribute their poor recognition experience to factors other than target absence. Our bystander instruction does just that by providing witnesses with a reason other than suspect guilt as to why the individual is being presented to them—because it is part of a new policy.
It could also be the case that the bystander instruction works in part by alluding to the possibility of future additional opportunities to view the perpetrator as well. For example, if witnesses believe they are being shown this person only because he was one of a few people near the scene of the crime shortly after the crime had occurred, there is potential for a witness to assume that they could be shown other individuals found around the crime scene as well. Future research could test this idea. Nonetheless, regardless of the exact mechanism behind the bystander instruction’s beneficial outcomes, these findings provide further support for Smith et al.’s (2018) present-absent discrepancy theory.

**Methodological Implications**

Being that both research goals were focused on eyewitness decision-making when witnesses believe the police believe the suspect is guilty, it was imperative that our experimental paradigm reflected these witness beliefs as they exist in real-world showup procedures. For this reason, we argue that laboratory studies fail to completely capture real-world witness decision-making from showup procedures because typical laboratory studies do not allow witnesses to form these beliefs. In fact, it is often the case that participants in laboratory studies are aware the crime they are watching has been staged and that they are making the identification as part of a research study. Although field simulations would also allow witnesses to form these beliefs, giving us optimal ecological validity, the amount of time and resources needed to achieve appropriate statistical power for ROC analyses was unrealistic given our project’s timeline and would also have resulted in nested participant data, as participants would be run in groups.
Therefore, we devised a novel experimental paradigm, which maintains high levels of experimental control while also maintaining ecological validity. This hybrid paradigm aimed to convince participants they were watching footage of a real crime that was being investigated by Miami-Dade Police and that their identification was part of a crowd sourcing procedure that could lead to the suspect’s investigation and/or arrest. This crowd sourcing cover story allowed us to instill in online participants a belief that police believe the suspect is guilty. We hypothesized the increased ecological validity of our procedure would result in the AOI being less effective in reducing false identification rates and our data supported this notion. Additionally, we were successful in manipulating the impact of this police belief through our suspect and bystander instructions, suggesting our paradigm was effective in instilling a belief that the police believe the suspect is guilty that we presume to be common in real world showup procedures. These results support the use of our hybrid paradigm in future research examining showups, as it allows researchers to test eyewitnesses’ decision-making in the presence of a realistic suggestion the police hold a belief the suspect is guilty, while also allowing for the collection of large sample sizes required by ROC analyses and maintaining experimental control.

**Practical Implications**

Decades of research examining showup identification procedures have found the procedure to be highly suggestive and its use has been criticized by researchers and lawyers alike (Agricola, 2009; Eisen et al., 2017a; Eisen et al., 2017b; United States v. Wade). The suggestiveness of the procedure, along with the low base rate of guilt typically associated with it, creates a high likelihood of a witness making a false
identification (Wells et al., 2020). Despite these issues, showups continue to be used by law enforcement as a means of quickly incriminating or exonerating a suspect. Because showups will continue to be used by law enforcement for practical purposes, government agencies have urged for the development of an investigative instruction that can be added to admonitions used in real world showups (Department of Justice, 1999; National Institute of Justice, 2013; National Research Council, 2014). Researchers have responded by beginning to examine potential interventions to mitigate the suggestiveness of the procedure. Wells et al. (2020) recommended that procedural safeguards typically associated with lineups (video-recording the procedure, collecting confidence judgments, reading of the admonition) be applied to showups. However, to date, very little research has examined how these safeguards impact identifications from showups, and only Smith et al.’s (2018) AOI has been developed as a potential intervention specifically for use with showup procedures. The present study makes clear the limitations of using the AOI alone in real world showup procedures, as it fails to undermine a witness’s belief that the police believe the suspect is guilty. Our belief instruction, however, has proved beneficial by increasing eyewitnesses’ discriminability in situations where witnesses hold a belief that the police believe the suspect is guilty.

In fact, our bystander instruction is arguably the closest version of a blind showup procedure that is possible. As previously mentioned, it would be extremely difficult, if not impossible, to conduct a double-blind showup because both the administrator and witness are aware the person being presented to them is the suspect. However, our bystander belief instruction directly challenges that notion by giving the witness an alternative reason this person is being presented to them—that they were a bystander.
Although no known research has examined the extent to which the suggestive administrator behaviors commonly seen in non-blind lineup procedures (e.g., smiling at, pointing out, and asking questions about the suspect; Charman & Quiroz, 2016; Zimmerman et al., 2017) extend to showups, it is reasonable to assume these suggestive cues occur during showups as well. While the bystander instruction may not impact showup administrators’ suggestive behaviors, since they will know the person they are detaining is the suspect regardless of reading the instruction, it is possible the benefits of our instruction would influence how witnesses interpret these behaviors. Future research could examine whether the bystander instruction leads witnesses to ignore or discount suggestive administrator behaviors during showup procedures.

While more research on the bystander instruction is needed before making firm policy recommendations, our current data suggest the instruction has potential for widespread use among law enforcement. The belief instruction provides law enforcement with a useful tool in improving identification accuracy in showups that requires little effort and essentially no resources to implement by investigators. In fact, implementing the instruction during the standard admonition, which is already standard procedure in most U.S. identification procedures (Steblay, 2013; Wells et al. 2020), would be as simple as reading an additional sentence or two to eyewitnesses.

**Limitations and Future Directions**

While our bystander belief instruction shows promise as an instruction that law enforcement might incorporate into their investigations to improve showup procedures, our paradigm utilized an involved cover story that gave witnesses a reason as to why they might be presented an innocent person during their identification procedure. We included
this as part of our hybrid paradigm that sought to bring ecological validity to an online study, but it might be unrealistic to incorporate into a real-world investigation. For instance, police might be hesitant to instruct witnesses that the suspect “was not behaving suspiciously and do not have evidence pointing towards him,” which might not be true. This issue could be addressed with a slight rewording of the bystander instruction that might be more appropriate for use in real world procedures. Perhaps an instruction telling witnesses they know more about the crime and the perpetrator than investigators do would efficiently undermine suggestiveness as well. This type of instruction has been found to be a useful tool within the context of the cognitive interview where it has been associated with improved witness recall for event details (Fisher & Geiselman, 1992). Future research could examine various adaptations of the bystander instruction to test whether they can similarly undermine witnesses’ beliefs that the police believe the suspect is guilty.

Another limitation of our present study is the use of our hybrid methodology. Although our paradigm was able to successfully influence witnesses’ beliefs that the police believe the suspect is guilty, allowing for greater ecological validity than traditional laboratory paradigms, participant-witnesses did not make their identification decisions with the knowledge their choice could lead to the suspect being sent to prison. This knowledge is, however, an inherent aspect of real-world showups and the present paradigm was unable to replicate this through our cover story or belief instructions. It is nonetheless expected that our belief instruction would prove beneficial in the presence of this witness knowledge as well, because the witnesses’ beliefs that the police believe the suspect is guilty is likely even more prevalent in real-world showups that have serious
consequences. However, this assumption could be tested by replicating current findings using field-simulated paradigms in which witnesses are led to believe their identification will lead to the suspect’s arrest.

It is also important to note we were unable to achieve significant confidence-accuracy curves among five of six of our manipulated conditions, as evidenced by the lack of a slope in our CAC curves. These results suggest that confidence ratings were not related to eyewitness accuracy in any meaningful way in the present study. There has been a plethora of eyewitness research that has been conducted to determine the extent to which eyewitness confidence is related to accuracy (Wixted & Wells, 2017). Although research has shown a promising relationship of higher confidence predicting accuracy from witnesses, recent DNA exonerations have depicted real world scenarios in which innocent individuals have been wrongfully convicted by highly confident eyewitnesses (Garrett, 2011; Wells et al., 2019). Taken together, the relationship between these variables is not as clear-cut as researchers originally thought. However, the majority of research has examined the confidence-accuracy relationship as it pertains to lineup procedures (Garrett, 2011; Wells et al., 2019), with very few studies examining the relationship within showup procedures (Neuschatz et al. (2016). Interestingly, some research that has examined the confidence-accuracy relationship for showup identifications has also failed to find a significant relationship between witnesses’ confidence ratings and their accuracy. For example, Eisen et al. (2017a) constructed calibration curves to examine the confidence-accuracy relationship within their field-simulated and laboratory conditions. They found that eyewitnesses in their lab conditions failed to exhibit a significant confidence-accuracy relationship. The researchers argued
the lack of a relationship in lab conditions was due to the lab identifications being highly accurate no matter the witness confidence. However, other research has also failed to find a significant confidence-accuracy relationship from showup identifications. For instance, Neuschatz et al. (2016) analyzed Wetmore et al.’s (2015) showup data using both point-biserial correlations and calibration techniques and also found only a weak confidence-accuracy relationship. The researchers found that although there was a pattern of higher confidence showup identifications being more accurate, the proportion of correct showup identifications only ranged from .64 among low-confidence witnesses to .75 among high-confidence witnesses. These accuracy rates are similar to those calculated in the present study. Taken together, these data suggest confidence might not be strongly related to accuracy within showup procedures.

No known research has explored reasons for the inconsistent confidence-accuracy relationship seen in research on showups. One possible theoretical reason for the lack of a confidence-accuracy relationship in showups could be related to the absence of fillers in showup procedures. Perhaps confidence is better calibrated with accuracy in circumstances in which witnesses are able to compare the recognition experience evoked by various lineup members. Specifically, good fillers, who produce a relatively strong recognition experience, might lead eyewitnesses to correctly assign low confidence to their suspect identifications, as they appropriately recognize that the identification task is difficult. Showup procedures, which do not have fillers, are unable to provide information to the witness about the difficulty of the identification task, resulting in inaccurate eyewitnesses reporting inappropriately high confidence in their identifications.
Future research should further examine the eyewitness confidence judgments in showups compared to lineups to help elucidate the theoretical reasons for the mixed findings regarding the confidence-accuracy relationship.

**Final Remarks**

Although more research is needed before creating firm policy for law enforcement, the bystander belief instruction shows promise as a means of improving identification outcomes from showup procedures. By mitigating the underlying suggestion to witnesses that the police believe the suspect is guilty, our bystander instruction can reduce innocent suspect identifications without impacting guilty suspect identifications with little to no additional resources needed by police precincts. While the benefits of the bystander instruction are quite promising, it is important to note we do not argue that the benefits seen here outweigh those that would be seen by conducting a lineup. In fact, we stand by the recommendations made in the white paper that showups only be conducted if no probable cause exists to arrest the suspect and create a lineup instead (Wells et al., 2020). However, in circumstances in which a showup is deemed necessary, the use of our bystander instruction may act as a procedural safeguard to protect innocent suspects from being incorrectly identified and later wrongfully convicted.
Table 1
Proportion of Identification Decisions as a Function of Lineup Type and Target ($N = 1814$)

<table>
<thead>
<tr>
<th>Target-Present</th>
<th>Target-Absent</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Correct</td>
</tr>
<tr>
<td></td>
<td>Identifications</td>
</tr>
<tr>
<td>Target 1</td>
<td>.48 (224/464)</td>
</tr>
<tr>
<td>Target 2</td>
<td>.62 (281/452)</td>
</tr>
<tr>
<td>Overall</td>
<td>.55 (505/916)</td>
</tr>
</tbody>
</table>
Table 2

*Proportion of Correct Identifications, False Identifications and Overall Choosing by Condition and Target*

<table>
<thead>
<tr>
<th>Instruction</th>
<th>No AOI</th>
<th></th>
<th>AOI</th>
<th></th>
</tr>
</thead>
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<tr>
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<td>Correct</td>
<td>False</td>
<td>All</td>
<td>Correct</td>
</tr>
<tr>
<td></td>
<td>Identifications</td>
<td>Identifications</td>
<td>All</td>
<td>Identifications</td>
</tr>
<tr>
<td>Target 1</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Control</td>
<td>.45 (32/71)</td>
<td>.29 (23/80)</td>
<td>.36 (55/151)</td>
<td>.42 (42/99)</td>
</tr>
<tr>
<td>Suspect</td>
<td>.53 (45/85)</td>
<td>.43 (29/68)</td>
<td>.45 (74/153)</td>
<td>.64 (44/69)</td>
</tr>
<tr>
<td>Bystander</td>
<td>.44 (30/69)</td>
<td>.18 (13/73)</td>
<td>.30 (43/142)</td>
<td>.44 (31/71)</td>
</tr>
<tr>
<td>Target 2</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Control</td>
<td>.61 (43/71)</td>
<td>.27 (19/71)</td>
<td>.44 (62/142)</td>
<td>.61 (49/80)</td>
</tr>
<tr>
<td>Suspect</td>
<td>.68 (50/74)</td>
<td>.23 (14/62)</td>
<td>.47 (64/136)</td>
<td>.61 (44/72)</td>
</tr>
<tr>
<td>Bystander</td>
<td>.65 (49/75)</td>
<td>.16 (13/82)</td>
<td>.40 (62/157)</td>
<td>.57 (46/80)</td>
</tr>
</tbody>
</table>
Table 3

**Binary Logistic Regression Predicting Choosing for Control and Suspect Belief Instruction Comparisons**

<table>
<thead>
<tr>
<th>Block 1</th>
<th>B</th>
<th>S.E.</th>
<th>Wald</th>
<th>df</th>
<th>p</th>
<th>Odds Ratio</th>
<th>95% C.I. for Odds Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Lower</td>
<td>Upper</td>
</tr>
<tr>
<td>Belief Instruction (Control/Suspect)</td>
<td>.31</td>
<td>.12</td>
<td>6.56</td>
<td>1</td>
<td>.01</td>
<td>1.36</td>
<td>1.08 - 1.73</td>
</tr>
<tr>
<td>AOI Presence</td>
<td>-.08</td>
<td>.12</td>
<td>.47</td>
<td>1</td>
<td>.49</td>
<td>.92</td>
<td>.73 - 1.17</td>
</tr>
<tr>
<td>Target Presence</td>
<td>1.18</td>
<td>.12</td>
<td>94.70</td>
<td>1</td>
<td>&lt;.01</td>
<td>3.27</td>
<td>2.58 - 4.15</td>
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<tr>
<td>Block 2</td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Belief Instruction (Control/Suspect)</td>
<td>.19</td>
<td>.22</td>
<td>.74</td>
<td>1</td>
<td>.39</td>
<td>1.21</td>
<td>.79 - 1.86</td>
</tr>
<tr>
<td>AOI Presence</td>
<td>-.24</td>
<td>.22</td>
<td>1.18</td>
<td>1</td>
<td>.28</td>
<td>.79</td>
<td>.51 - 1.21</td>
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<tr>
<td>Target Presence</td>
<td>1.01</td>
<td>.21</td>
<td>22.62</td>
<td>1</td>
<td>&lt;.01</td>
<td>2.75</td>
<td>1.81 - 4.17</td>
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<td>Belief Instruction* AOI</td>
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<td>.24</td>
<td>.16</td>
<td>1</td>
<td>.69</td>
<td>1.10</td>
<td>.68 - 1.77</td>
</tr>
<tr>
<td>Belief Instruction* Target Presence</td>
<td>.14</td>
<td>.24</td>
<td>.34</td>
<td>1</td>
<td>.56</td>
<td>1.15</td>
<td>.71 - 1.86</td>
</tr>
<tr>
<td>AOI* Target Presence</td>
<td>.20</td>
<td>.17</td>
<td>.69</td>
<td>1</td>
<td>.41</td>
<td>1.23</td>
<td>.76 - 1.98</td>
</tr>
<tr>
<td>Block 3</td>
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<td></td>
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<td></td>
<td></td>
<td></td>
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<tr>
<td>Belief Instruction (Control/Suspect)</td>
<td>.25</td>
<td>.26</td>
<td>.92</td>
<td>1</td>
<td>.34</td>
<td>1.28</td>
<td>.77 - 2.14</td>
</tr>
<tr>
<td>AOI Presence</td>
<td>-.179</td>
<td>.26</td>
<td>.47</td>
<td>1</td>
<td>.49</td>
<td>.84</td>
<td>.50 - 1.39</td>
</tr>
<tr>
<td>Target Presence</td>
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<td>.25</td>
<td>18.57</td>
<td>1</td>
<td>&lt;.01</td>
<td>2.91</td>
<td>1.79 - 4.72</td>
</tr>
<tr>
<td>Belief Instruction* AOI</td>
<td>-.02</td>
<td>.36</td>
<td>.01</td>
<td>1</td>
<td>.95</td>
<td>.98</td>
<td>.48 - 1.99</td>
</tr>
<tr>
<td>Belief Instruction* Target Presence</td>
<td>.03</td>
<td>.35</td>
<td>.01</td>
<td>1</td>
<td>.92</td>
<td>1.03</td>
<td>.52 - 2.05</td>
</tr>
<tr>
<td>AOI* Target Presence</td>
<td>.09</td>
<td>.34</td>
<td>.08</td>
<td>1</td>
<td>.77</td>
<td>1.10</td>
<td>.56 - 2.16</td>
</tr>
<tr>
<td>Belief Instruction* AOI* Target Presence</td>
<td>.21</td>
<td>.49</td>
<td>.19</td>
<td>1</td>
<td>.67</td>
<td>1.24</td>
<td>.47 - 3.22</td>
</tr>
</tbody>
</table>
Table 4

*Binary Logistic Regression Predicting Choosing for Control and Bystander Belief Instruction Comparisons*

| Block 1 |  |  |  |  |  |  |  |
|---------|------------------|-------|---|---|---|---|
|         | B    | S.E. | Wald| df | p  | Odds Ratio | 95% C.I. for Odds Ratio |
|         |      |      |     |    |    | Lower | Upper |
| Belief Instruction (Control/Bystander) | -.18 | .13  | 2.10 | 1  | .15 | .83  | .65  | 1.07 |
| AOI Presence | -.09 | .13  | .61 | 1  | .44 | .91  | .71  | 1.16 |
| Target Presence | 1.37 | .13  | 115.78 | 1 | <.01 | 3.95 | 3.07 | 5.07 |

| Block 2 |  |  |  |  |  |  |  |
|---------|------------------|-------|---|---|---|---|
|         | B    | S.E. | Wald| df | p  | Odds Ratio | 95% C.I. for Odds Ratio |
|         |      |      |     |    |    | Lower | Upper |
| Belief Instruction (Control/Bystander) | -.55 | .24  | 5.44 | 1  | .02 | 3.14 | .37  | .92  |
| AOI Presence | -.09 | .22  | .17 | 1  | .68 | .91  | .59  | 1.41 |
| Target Presence | 1.15 | .22  | 27.53 | 1 | <.01 | 3.14 | 2.05 | 4.82 |
| Belief Instruction* AOI | .06  | .25  | .05 | 1  | .82 | 1.06 | .65  | 1.74 |
| Belief Instruction* Target Presence | .56  | .26  | 4.68 | 1  | .03 | 1.75 | 1.05 | 2.90 |
| AOI* Target Presence | -.05 | .26  | .04 | 1  | .85 | .95  | .58  | 1.57 |

| Block 3 |  |  |  |  |  |  |  |
|---------|------------------|-------|---|---|---|---|
|         | B    | S.E. | Wald| df | p  | Odds Ratio | 95% C.I. for Odds Ratio |
|         |      |      |     |    |    | Lower | Upper |
| Belief Instruction (Control/Bystander) | -.65 | .28  | 5.30 | 1  | .02 | .52  | .30  | .91  |
| AOI Presence | -.179 | .26  | .47 | 1  | .49 | .84  | .50  | 1.39 |
| Target Presence | 1.06 | .25  | 18.57 | 1 | .00 | 2.91 | 1.79 | 4.72 |
| Belief Instruction* AOI | .26  | .40  | .43 | 1  | .51 | 1.30 | .59  | 2.86 |
| Belief Instruction* Target Presence | .73  | .37  | 3.94 | 1  | .05 | 2.08 | 1.01 | 4.27 |
| AOI* Target Presence | .09  | .34  | .08 | 1  | .77 | 1.10 | .56  | 2.16 |
| Belief Instruction* AOI* Target Presence | -.34 | .52  | .43 | 1  | .51 | .71  | .26  | 1.96 |
Table 5  
*Receiver Operating Characteristic (ROC) Analyses for Additional-Opportunity Instruction (AOI) Compared to No AOI, Collapsed Across Belief Instruction.*

<table>
<thead>
<tr>
<th>AOI</th>
<th>AOI Not Given</th>
<th>( D )</th>
<th>( p )</th>
<th>( pAUC )</th>
<th>DPP</th>
<th>95% C.I.</th>
</tr>
</thead>
<tbody>
<tr>
<td>AOI</td>
<td>AOI Not Given</td>
<td>( D )</td>
<td>( p )</td>
<td>( pAUC )</td>
<td>DPP</td>
<td>95% C.I.</td>
</tr>
<tr>
<td>.43</td>
<td>.42</td>
<td>-.49</td>
<td>.62</td>
<td></td>
<td>.76</td>
<td>.77</td>
</tr>
</tbody>
</table>

*Note.* \( pAUC \) = partial area under the curve; DPP = Deviation from perfect performance; \( D \) is defined as \((\text{AUC1}-\text{AUC2})/s\) where \( s \) is standard error of the difference between two AUCs estimated by the bootstrap method.
Table 6

Receiver Operating Characteristic (ROC) Analyses for Control Belief Instruction and Suspect Belief Instructions Comparisons

Collapsed Across Additional-Opportunity Instruction (AOI)

<table>
<thead>
<tr>
<th></th>
<th>Truncated</th>
<th></th>
<th>Extrapolated</th>
<th></th>
<th>DPP</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Control pAUC</td>
<td>Suspect pAUC</td>
<td>D</td>
<td>p</td>
<td>Control pAUC</td>
</tr>
<tr>
<td></td>
<td>.39</td>
<td>.43</td>
<td>-1.78</td>
<td>.07</td>
<td>.34</td>
</tr>
</tbody>
</table>

Note. pAUC = partial area under the curve; DPP = Deviation from perfect performance. D is defined as \((\text{AUC}_1 - \text{AUC}_2)/s\) where \(s\) is standard error of the difference between two AUCs estimated by the bootstrap method.
Table 7
Receiver Operating Characteristic (ROC) Analyses for Control Belief Instruction and Bystander Belief Instructions

Comparisons Collapsed Across Additional-Opportunity Instruction (AOI)

<table>
<thead>
<tr>
<th></th>
<th>Truncated</th>
<th></th>
<th>Extrapolated</th>
<th></th>
<th></th>
<th>95% C.I.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Control pAUC</td>
<td>Bystander pAUC</td>
<td>D</td>
<td>p</td>
<td>Control pAUC</td>
<td>Bystander pAUC</td>
</tr>
<tr>
<td></td>
<td>.47</td>
<td>.52</td>
<td>-2.02</td>
<td>.04</td>
<td>.39</td>
<td>.44</td>
</tr>
</tbody>
</table>

Note. pAUC = partial area under the curve; DPP = Deviation from perfect performance. D is defined as (AUC1-AUC2)/s where s is standard error of the difference between two AUCs estimated by the bootstrap method.
Table 8

Proportion of Accurate Suspect Identifications as a Function of Confidence Bin and Condition

<table>
<thead>
<tr>
<th>Confidence Bin</th>
<th>Control</th>
<th>Suspect</th>
<th>Bystander</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>AOI</td>
<td>No AOI</td>
<td>AOI</td>
</tr>
<tr>
<td>Low (0-60%)</td>
<td>.69</td>
<td>.69</td>
<td>.62</td>
</tr>
<tr>
<td></td>
<td>.08</td>
<td>.06</td>
<td>.07</td>
</tr>
<tr>
<td>Medium (70-80%)</td>
<td>.65</td>
<td>.66</td>
<td>.70</td>
</tr>
<tr>
<td></td>
<td>.07</td>
<td>.07</td>
<td>.06</td>
</tr>
<tr>
<td>High (90-100%)</td>
<td>.55</td>
<td>.81</td>
<td>.79</td>
</tr>
<tr>
<td></td>
<td>.09</td>
<td>.08</td>
<td>.08</td>
</tr>
</tbody>
</table>
Figure 1. Receiver Operating Characteristic (ROC) curves plotting data for those given additional-opportunities instruction (AOI) and those not given the AOI collapsed across belief instruction condition.
Figure 2. Receiver Operating Characteristic (ROC) curves plotting control belief instruction, suspect belief instruction, and bystander belief instruction collapsed across additional-opportunities instruction (AOI) levels.
Figure 3. Confidence-Accuracy Characteristic (CAC) curves for correct suspect identifiers at low (0-60%), medium (60%-80%) and high (90%-100%) levels of confidence for participants presented with our control belief instruction and not given the additional-opportunities instruction (AOI). Error bars represent standard error (S.E.).


Appendix A

Target A Photo
Appendix B

Target B Photo
Appendix C

Visual search tasks

Please scan the photo and answer the following questions to the best of your ability. The page will advance automatically after time has elapsed.

Inaccurate or partial answers will not be penalized
However, incomplete answers or unintelligible answers are potential qualifications to be excluded from data collections without payment.

1. Please describe in great detail the scene above

2. Can you find three monsters in the picture? Please describe them and what they are doing in as much detail as possible.

3. For some the details in this scene may be difficult to see. Can you see the image clearly? Please describe in as much detail as possible everything you can make out from the image.
4. Do you notice anything that stands out about this image? If so, describe in detail what is different and how many out of place items you can detect.

5. Describe in as much detail as possible what you are seeing occur in this scene.

6. Take a moment to scan the photo again, how many horses can you find in the above image? And please describe some difference among the horses.
Appendix D

Attention Check

Please recall the Crime Report you read prior to watching the crime and answer the questions to the best of your ability.

Answers that are inaccurate, incomplete or incomprehensible are potential qualifications to be excluded from data collection without payment.

1. How did police come in possession of the crime video?
   - Crime Stoppers
   - YouTube
   - Found it lying around

2. What is the new identification policy being tested today?
   - Finding faces from Facebook and seeing if anyone finds them suspicious
   - Crowd sourcing identifications from photos taken of people near the scene of the crime
   - Setting traffic cones near busy roads

3. In just a few sentences, please describe all you can remember from the crime report.
   <Provided space for participant to respond>

Now please recall the video recorded crime you watched and answer the following questions to the best of your ability.

4. What type of shirt was the perpetrator wearing?
   - White polo shirt
   - Suit jacket and under shirt
   - Plain black t-shirt

5. What crime did you watch occur?
   - Assault of a police officer
   - Theft of a laptop out of a car
   - Traffic violation - running a red light

6. In just a few words, please describe the appearance of the perpetrator (gender, race, hair, height, etc.)
   <Provided space for participant to respond>
Appendix E

Cover Story

The video you are about to watch is now property of the Miami-Dade police department and was captured by a concerned citizen parked in an overflow parking lot of a large university in South Florida. The citizen submitted this video into the Miami-Dade Crime Stoppers website as an anonymous source.

Prior to receiving this video tip, police were investigating an uptick in thefts on the university’s campus. The majority of the thefts involved items being stolen out of unlocked vehicles that were parked in overflow parking lots on campus. These parking lots have no security cameras and very little foot traffic.

The psychology department at FIU is collaborating with the Miami-Dade Police to test a new identification policy. In accordance with this new policy, police must take photos of the area and all people in the area. Shortly after photos are taken, they then must crowd source identifications made from Crime Stoppers footage. As part of the present study, you will be a part of the crowd sourced identification procedure.

You are about to be shown the Crime Stoppers footage mentioned above. Please pay attention to the video, as you will be asked to make a crowd sourced identification later.
Appendix F

Instructions by Condition

1. Control/ No AOI
   You are about to be shown a photograph of someone who may or may not be the thief from the video you witnessed earlier.

   In accordance with the new crowd sourcing policy, this person’s photo was taken because he was one of a few people near the scene of the crime, shortly after the crime had occurred.

2. Control/ AOI
   You are about to be shown a photograph of someone who may or may not be the thief from the video you witnessed earlier.

   In accordance with the new crowd sourcing policy, this person’s photo was taken because he was one of a few people near the scene of the crime, shortly after the crime had occurred.

   If you do not believe the man in the photo is the man you saw, other photos may be shown later for you to attempt to identify the man from the video.

3. Suspect/ No AOI
   You are about to be shown a photograph of someone who may or may not be the thief from the video you witnessed earlier.

   In accordance with the new crowd sourcing policy, this person’s photo was taken because he was one of a few people near the scene of the crime, shortly after the crime had occurred.

   However, police believed he was behaving suspiciously and believed the evidence pointed to him.

4. Suspect/ AOI
   You are about to be shown a photograph of someone who may or may not be the thief from the video you witnessed earlier.

   In accordance with the new crowd sourcing policy, this person’s photo was taken because he was one of a few people near the scene of the crime, shortly after the crime had occurred.

   However, police believed he was behaving suspiciously and believed the evidence pointed to him.
If you do not believe the man in the photo is the man you saw, other photos may be shown later for you to attempt to identify the man from the video.

5. Bystander/ No AOI
You are about to be shown a photograph of someone who may or may not be the thief from the video you witnessed earlier.

In accordance with the new crowd sourcing policy, this person’s photo was taken because he was one of a few people near the scene of the crime, shortly after the crime had occurred.

However, police noted he was not behaving suspiciously and do not have evidence pointing towards him.

6. Bystander/ AOI
You are about to be shown a photograph of someone who may or may not be the thief from the video you witnessed earlier.

In accordance with the new crowd sourcing policy, this person’s photo was taken because he was one of a few people near the scene of the crime, shortly after the crime had occurred.

However, police noted he was not behaving suspiciously and do not have evidence pointing towards him.

If you do not believe the man in the photo is the man you saw, other photos may be shown later for you to attempt to identify the man from the video.
Appendix G

Main Dependent Variable Questions

1. Is this man in the photo the same man from the video who stole the laptop from a car?
   - Yes
   - No

2. How confident are you in your identification decision?

   0 10 20 30 40 50 60 70 80 90 100

   Confidence % ()
Appendix H
Manipulation Check Questions

Please recall the instructions you read prior to making your identification and answer the questions to the best of your ability. Incorrect answers to the following questions are potential qualifications to be excluded from data collection without payment.

1. Were you instructed the person in the photograph may or may not be the perpetrator?
   • Yes
   • No

2. Which of the following instructions did you read after being told "this person’s photo was taken because he was one of a few people near the scene of the crime, shortly after the crime had occurred."
   • However, police noted he was not behaving suspiciously and do not have evidence pointing towards him.
   • However, police believed he was behaving suspiciously and believed the evidence pointed towards him.
   • Neither of the above

3. Were you instructed "If you do not believe the man in the photo is the man you saw, other photos may be shown later for you to attempt to identify the man from the video."
   • Yes
   • No
Appendix I

Follow-Up Questionnaire & Demographics

To what extent do you believe this is the guilty person?

1 not guilty at all
2
3
4
5
6
7 extremely guilty

To what extent do the police believe this is the guilty person?

1 not guilty at all
2
3
4
5
6
7 extremely guilty

Did you have a good basis (enough information) to make an identification?

1 no basis at all
2
3
4
5
6
7 an extremely good basis

How easy or difficult was it for you to make an identification?

1 extremely difficult
2
3
4
5
6
7 extremely easy

After you were first presented with the photos, how long did it take you to make an identification?

1 not at all long
2
3
4
5
6
7 extremely long

How clear an image of the perpetrator did you have?
1. How good of a **view** did you have of the perpetrator?

<table>
<thead>
<tr>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td>not at all clear</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>extremely clear</td>
</tr>
</tbody>
</table>

2. How **long** would you estimate that the perpetrator’s face was in your line of sight?

<table>
<thead>
<tr>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td>not at all long</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>extremely long</td>
</tr>
</tbody>
</table>

3. How **well** were you able to make out specific features of the perpetrator?

<table>
<thead>
<tr>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td>not at all well</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>extremely well</td>
</tr>
</tbody>
</table>

4. How much **attention** did you pay to the perpetrator?

<table>
<thead>
<tr>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td>no attention</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>my total attention</td>
</tr>
</tbody>
</table>

5. How **strong** was your memory for the perpetrator during the identification?
For the following questions, please provide demographic information about yourself honestly.

Age __________

Gender
- Male
- Female
- Self-Identified __________

Ethnicity
- White
- Hispanic
- Black/African American
- Asian/Pacific Islander
- Native American
- Other __________

What is your primary state of residency? __________________________
What is the highest level of education you have completed?
- High School Graduate
- Associate Degree
- Bachelors Degree
- Master’s Degree
- Other __________

Are you or a member of your immediate family a law enforcement officer?
- Yes- I am
- Yes- a family member is
- No

How familiar are you with the United States Criminal Justice system?
- Extremely familiar
- Very familiar
- Moderately familiar
- Slightly familiar
- Not familiar at all

Do you have first-hand experience with the criminal justice system
- Yes, as a suspect
- Yes, as a suspect and later defendant
• Yes, as a victim
• Yes, as a witness
• Yes, in a professional capacity (eg., law enforcement, attorney)
• No
• Prefer not to answer

Have you ever been asked to make a criminal identification as part of a real world criminal investigation?
• Yes
• No

Have you ever been investigated as part of a criminal investigation?
• Yes
• No

Have you ever been arrested?
• Yes
• No
• Prefer not to answer

How trustworthy do you find police officers to be?
• Extremely trustworthy
• Very trustworthy
• Moderately trustworthy
• Slightly familiar
• Not trustworthy at all
Appendix J

Online Debrief

The objective of the proposed study is to provide an experimental test of whether a novel pre-showup instruction—created specifically to minimize the suggestiveness of showups. We did not tell you we were examining the use of pre-identification instructions as to not influence your decision-making in any way. We misled you to believe it was a real crime as a way to test the impact of our showup instructions in a scenario similar to that a real eyewitness would during a real-world criminal identification.

If you should have any questions or concerns regarding the study and your role in it, please do not hesitate to contact Alexis Mook at amook001@fiu.edu. If you experienced any discomfort associated with the study and would like to speak to a counselor free of charge, you may contact FIU Counseling and Psychological Services Center at 305-348-2434.
VITA

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Burnet, D., Mook, A.T., Kleider, H.M., Turner, J.A. (2015). Auditory Imagery and Sentence Memory. Psychology Undergraduate Research Conference (PURC), Georgia State University, Atlanta, GA.