Title
Choice Blindness as Misinformation: Memory Distortion in an Eyewitness Identification Task

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Publication Date
2015

UNIVERSITY OF CALIFORNIA,
IRVINE

Choice Blindness as Misinformation: Memory Distortion in an Eyewitness Identification Task

THESIS

submitted in partial satisfaction of the requirements
for the degree of

MASTER OF ARTS

in Social Ecology

by

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2015

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ACKNOWLEDGMENTS

This material is based upon work supported by the National Science Foundation Graduate Research Fellowship DGE-1321846. Any opinion, findings, and conclusions or recommendations expressed in this material are those of the authors(s) and do not necessarily reflect the views of the National Science Foundation.

ABSTRACT OF THE THESIS

Choice Blindness as Misinformation: Memory Distortion in an Eyewitness Identification Task

By

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Master of Arts in Social Ecology

University of California, Irvine, 2015

Professor Elizabeth F. Loftus, Chair

Despite lay intuition, research has shown people often do not notice when an outcome they are presented with differs from a choice they made. The novel choice blindness paradigm investigates the extent to which this occurs and how it effects later decisions. Theoretically and methodologically choice blindness parallels studies on the misinformation effect. In both paradigms, participants view an event (or make a decision), receive inaccurate post-event feedback, and complete a memory test. Results from misinformation studies focus on whether people recall the misinformation or the actual details of the event while the choice blindness paradigm focuses on whether people detect the misinformation. The current study uses findings from the misinformation literature and applies them to a choice blindness paradigm for an eyewitness identification decision. Participants witnessed an event and made a lineup identification. Later, they were given accurate, inaccurate, or no feedback about their identification. Inaccurate feedback came in the form of a manipulated version of the participants' own report. Finally, participants again viewed the initial lineup. Results indicated that a substantial proportion of participants failed to notice the misinformation, and that these participants were most likely to select a different lineup member in lineup 2 than they did in lineup 1. No behavioral or personality factors were found to distinguish between those who did and did not notice the misinformation. This study provides a link between the misinformation and choice blindness literatures and extends the research on the effect of the choice blindness manipulation on later memory.

INTRODUCTION

Years of research on the misinformation effect has demonstrated that people's memory for events are malleable (Loftus, 2005). Inaccurate information learned after an event can be incorporated into one's memory for the event and entire, rich false memories can be implanted. Despite this, little research has focused on whether a person's own report of events can be manipulated and used as its own source of misinformation. The new paradigm of choice blindness can be used to investigate this question. The present study uses a choice blindness manipulation to investigate whether presenting witnesses with altered versions of their own lineup identification affects their identification on a subsequent lineup.

CHAPTER 1: CHOICE BLINDNESS AND MISINFORMATION

Choice Blindness

In the original choice blindness experiment, participants viewed two cards, each depicting a different female face and then picked which face they found more attractive (Johansson, Hall, Sikström, & Olsson, 2005b). On some trials, after the participant made his choice, the experimenter gave the participant back the card he picked asked him why he chose this person. On a subset of these trials—called manipulated trials—the experimenter used a sleight of hand trick such that the card the participant viewed was actually *not* the person they had selected (i.e. if the participant selected person "A" the card they saw depicted person "B"). Overall, only 26% of participants detected the manipulation and there were no significant differences in detection between high similarity and low similarity pairs. These findings demonstrate choice blindness—people are often unaware of a mismatch between a choice they previously made and the option they are currently shown.

Not only do participants fail to notice the manipulation, they often confabulate reasons for their new-found "choice". Researchers analyzed participants' responses when they explained why they picked the woman they did to explore whether the manipulation affected their reports of their decision-making process (Johansson et al., 2005b). Results demonstrated that participants verbalized reasons for their decision even in manipulated trials when they were viewing the non-chosen option. There were no significant differences in the rates of "empty reports"—where the participant could not verbalize a reason for their choice—between manipulated and non-manipulated (i.e. control) trials. Additionally, the responses in the two types of trials did not differ in emotionality, specificity, or certainty.

Responses in the manipulated condition were further analyzed to measure the degree of confabulation. About 13% of responses were categorized as “specific confabulation.” In this category, the participant specifically referenced features of the manipulated picture that were nonexistent in the originally chosen face. For instance, one participant said that he liked the woman’s earrings. The woman in the picture he had chosen was not wearing earrings but the woman in the manipulated photograph was. On the other end of the spectrum was the “original choice” category. The 11.2% of participants coded in this category described features of the original face even while looking at the manipulated face (Johansson et al., 2005b). Taken as a whole, these results indicate a substantial amount of confabulation. The vast majority of participants articulated a reason for their decision even when presented with a face different from the one they actually selected. Moreover, a small, but significant minority of participants specifically described basing their decision on a feature that only occurred in the manipulated face. This suggests that blindness is not a result of participants failing to attend to the manipulation. Rather, viewing the manipulated face can cause people to actively construct post-hoc reasons for a choice they never made.

Central to choice blindness studies is the measure of whether participants notice the manipulation, also called detection. Detection in the choice blindness paradigm is typically measured in two ways: concurrent detection and retrospective detection (Johansson, Hall, Sikström, & Olsson, 2005a). Concurrent detection occurs when participants give an indication of noticing the manipulation during or directly after the manipulation. For example, if a participant commented that the researcher had made a mistaken and given him the wrong face, he would be coded as concurrently detecting the manipulation. The terms detection and blindness are used synonymously in the literature. A person who fails to detect the manipulation is blind to the manipulation. Retrospective detection is assessed at the end of a study using a funneled debriefing procedure. The experimenter asks a series of increasingly direct questions that probe whether participants had any indication of the manipulation occurring. Retrospective detection is a conservative measure that likely provides an upper-bound limit on the percent of detectors. Only participants who give no indication of detection on all of the questions are coded as choice blind. If participants answer even one question with some indication of detection, then they have retrospectively detected the manipulation. Often, concurrent and retrospective rates are combined into an overall measure of detection. The term "detectors" refers to individuals who either concurrently or retrospectively notice the manipulation and "non-detectors" refers to individuals who do not notice the manipulation.

Choice blindness has been applied to a variety of sensory mediums. In a field study at a supermarket, shoppers volunteered to participate in a quality control test (Hall, Johansson, Tärning, Sikström, & Deutgen, 2010). They tasted two flavors of jam and smelled two types of tea and then indicated their favorites of each pair. After this, they again evaluated their favorite tea and jam and verbally described how they picked their favorites. However, on half of the trials, the second option they evaluated was actually the non-selected jam or tea. Overall, 13.8% of the tea trials and 14.4% of the jam trials were concurrently detected. An additional 6-7% of these trials were retrospectively detected. Interestingly, incentives decreased rates of detection for the tea condition. Participants told they would receive the tea they preferred as a gift were significantly less likely to detect the manipulation (19.6%) than those not told about the gift (46.3%), although this difference did not occur for the jam condition.

The choice blindness paradigm applies to the auditory domain as well. Participants listened to pairs of voice clips and selected the voice that displayed the highest either criminality or sympathy (Sauerland, Sagana, & Otgaar, 2013). Later, participants listened to the chosen clip again. As with other choice blindness studies, the second voice clip was of the non-chosen option in the pair. Less than 30% of participants detected the manipulation either concurrently or retrospectively even though the identical voice clip was played both at the decision phase and during the manipulation. These studies suggest that the reasons behind the choice blindness findings cannot be attributed solely to factors regarding the processing of visual stimuli. Rather, the paradigm reveals underlying mechanisms in the way a broader range of inputs are processed.

While choice blindness clearly extend to a range of decisions, one could argue that the manipulations relate to somewhat superficial preference decisions. Being blind to buying the wrong brand of jam or seeing a different face as attractive are generally low cost, low consequence mistakes. Therefore, a possible moderator of detection effects could be the level of self-relevance or consequentiality of the decision. To investigate whether choice blindness occurred in these kinds of domains, Hall, Johansson, and Strandberg (2012) manipulated people's responses about moral and ethical issues. Participants in this study completed a survey about their moral principles (e.g. society should "promote the welfare of the citizens than to protect their personal integrity") and their views on several current moral issues (e.g. government surveillance of the Internet) on a clipboard (Hall et al., 2012, p. 45457). At the end of the survey, the researcher asked the participant to flip back to the first page and to read and discuss his answers to several questions. Unbeknownst to participants, a sticky note placed on the back of the clipboard attached itself to the survey when the participant returned to the first page. The sticky note reversed the wording of several items, in essence reverse scoring the statements. For instance, an original item read "large scale government surveillance of e-mail and internet traffic ought to be forbidden" whereas the manipulated statement read "…ought to be permitted." Thus a person who originally strongly agreed with the statement would seem to be agreeing with the opposite side of the issue when he reviewed his answer.

Despite the more self-relevant nature of the task, detection rates were near floor level, only 8% concurrent, when the manipulated statements described about moral principles. Detection rate increased to about 50% combined detection when the manipulated statements described current moral issues. Across conditions, 69% of participants did not detect at least one of the manipulated statements. This demonstrates that people are blind to decisions about more consequential decisions. However, it also provides an indication that the type of task does matter in determining rates of detection. When asked about their views on current moral issues, which may be more salient, rates of detection were significantly higher than when the statements were about moral principles, which may be less salient in day to day life.

Using a similar clipboard procedure, researchers studied whether people would be choice blind to statements about political attitudes (Hall et al., 2013). Participants rated their agreement with a variety of issues from official political party platforms on a 0-100 scale. Four of these statements were manipulated such that when participants discussed their responses, they seemed to be supporting the opposite attitude. Forty-seven percent of participants were blind to the manipulation on all four of the statements. Moreover, at the end of the study, the researchers created a composite score of voting intention based on the participants' answers. In the manipulated condition, 92% of participants did not detect that their summary score indicated they supported the opposite party that they endorsed beliefs for. The consequences of blindness in these studies are not trivial. Participants shifted their voting intentions on average 15 points (on a 100 point scale) from pretest to posttest in the manipulated condition compared to only 1.7

points in the control condition. Overall, 10% of people switched from strongly supporting one party to strongly supporting the other (Hall et al., 2013).

Blindness to consequential manipulation also occurs for one's own behaviors. Sauerland, Schell and colleagues (2013) had participants complete the Questionnaire About History of Norm-Violating Behaviors. This scale includes items such as whether an individual ever cheated on a high school exam or parked in a disabled parking spot. Participants' responses on some of the items were increased two scale points (i.e., from never to sometimes) and the experimenter then asked the participant to elaborate on their manipulated answer. Nearly 18% of these responses were not concurrently detected. While this number is much lower than most other choice blindness studies, the consequences of being blind to this manipulation are significantly higher. Several of the behaviors listed on the questionnaire are not only norm violating, they are also illegal. Being blind to a change from never to sometimes on one of these behaviors is, in essence, confessing to a crime that one did not commit (Sauerland, Schell, et al., 2013).

In all of these studies, the key, and sometimes only, dependent variable of interest is the rate of detection. This is characteristic of research in the choice blindness field. Research has concentrated on varying the context in which the manipulation occurs (e.g. Hall et al., 2010) or the type of decision that is manipulated (e.g. Hall et al., 2012) to investigate whether blindness occurs, and to what extent, under these conditions. While blindness to a change between a choice and an outcome certainly is an important finding, especially in domains such as one's own illegal behavior, it only tells part of the story. What happens after the manipulation?

Johansson, Hall, Tärning, Sikström, and Chater (2014) explored this question using the same facial attractiveness stimuli and procedure as Johansson and colleagues (2005b). After the procedure in the initial study, participants again viewed all pairs of faces. They provided ratings of each face in the pair and again selected the one they found more attractive. There were no manipulations in the second round of viewing. Replicating the first study, rates of detection were low. The data from the second round of ratings showed the consequences of being exposed to the manipulation. For the non-manipulated trials, participants consistently chose the same face on both rounds for 93.3% of the pairs. On manipulated trials, consistency dropped to 56.6%. The manipulation not only affected consistency, but also ratings of the faces. Participants in the manipulated condition rated their originally chosen face as less attractive on the second trial and rated their originally rejected (i.e. the manipulated face) face as more attractive. So if participants chose face "A" originally but were shown face "B," on the second round they rated face "A" as less attractive and face "B" as more attractive. Presenting the non-selected option as if it were the participant's actual choice caused participants to like that option more and simultaneously like their original choice less. Detection played a significant role in these effects. For non-detected manipulated trials, consistency was 43.8% compared to 82.5% for detected trials. For non-detected manipulated trials the difference in attractiveness ratings between rounds was significantly greater than for detected manipulated trials. Thus, detecting the manipulation served a protective effect against the consequences of being exposed to the manipulation (Johansson et al., 2014).

These effects last longer than just the initial session they occur in. In Merckelbach, Jelicic, and Pieters (2011), participants completed the Symptom Checklist-90 (SCL-90) and were led through a typical choice blindness procedure wherein two of their answers were manipulated. For instance, if a participant reported that they "not at all" worry too much about things, this was changed to "occasionally" worry too much about things. They then completed the same checklist one week later. Participants who detected the manipulation at time one reported equal symptoms on the SCL-90 at time one and two. However, non-detectors reported significantly greater manipulated symptoms one week later. Blindness to the manipulation caused participants memories to change such that they reported more occurrence of symptoms after they received false feedback about their own symptom reporting.

Choice Blindness and Misinformation

When participants complete a second round of ratings after the manipulation, choice blindness studies closely resemble studies on the misinformation effect. In many ways, the choice blindness manipulation can be viewed as a new type of misinformation. Methodologically, the two paradigms share many features. In a traditional misinformation study, participants view an event, receive post-event information, and complete a memory test (Loftus, 2005). Misinformation is implanted to the extent that participants recall the post-event information rather than the details of the original event. In the classic example of the misinformation effect, participants watched a slideshow of a car accident (Loftus, Miller, & Burns, 1978). The critical slide depicted a car stopped at a stop sign. Later in the study, participants answered questions regarding their memory for the original event. For participants in the misinformation condition, one leading question presupposed the car stopped at a yield sign. Compared to the control condition, participants who received the misinformation more often reported that the car had stopped at a yield sign.

A typical choice blindness study directly parallels that of studies on the misinformation effect. Participants view multiple choices and make a decision (the original event) and then are told they made a choice that they actually did not (i.e., they are exposed to misinformation). Later, they are asked again to make the original choice (i.e., they are tested to see if the misinformation affected their later decision making). In fact, most misinformation studies could be transformed into choice blindness studies by incorporating two changes. For instance, in the stop sign study, before the critical item participants would first need to be asked to pick which street sign they saw (Loftus et al., 1978). If the participant actually saw a yield sign, then the second change would be modifying the critical item from "Did another car pass the red Datsun while it was stopped at the stop sign?" to "Earlier, you said another car passed the red Datsun while it was stopped at the stop sign" (Loftus et al., 1978, p. 22). Making these two simple changes, and adding in a measure of detection, would essentially change the study into one measuring the effects of choice blindness instead of misinformation.

The main factor that differentiates choice blindness misinformation from more typical misinformation is the source the post-event information originates from. In choice blindness, the misinformation comes ostensibly from the self in the form of modified version of participants' own reports (Sagana, Sauerland, & Merckelbach, 2014). In misinformation studies, the post-event information comes from outside the self. This can occur through suggestive questioning or when participants read reports of other witness' account of events (Hope, Ost, Gabbert, Healey, & Lenton, 2008; Loftus et al., 1978).

As the two paradigms share many aspects, it is important to differentiate between the key factors driving the findings: "consequences of the manipulation" and "consequences of blindness" (Sagana et al., 2014, p. 762). Effects caused by consequences of the manipulation occur only because of the presentation of the misinformation and occur independently of detection status. Effects caused by consequences of blindness are results dependent on acceptance of the manipulated outcome. The increased report of symptoms for only blind participants would be an example of consequences of blindness (Merckelbach et al., 2011). The Discrepancy Detection principle provides a theoretical explanation for why such findings occur

(Tousignant, Hall, & Loftus, 1986). The principle states that memory changes most often when a person does not notice (is blind to) the difference between the misinformation and the original event.

Whereas consequences of blindness relate most to results from choice blindness, consequences of the manipulation correspond to the misinformation effect. Overall, participants in the misinformation condition incorporate the post-event information into their memory more often than those in the control condition (Loftus et al., 1978). Detection is rarely, if ever, measured in misinformation studies—a key factor that distinguishes it from choice blindness studies which focus on detection (Tousignant et al., 1986). Although detection is not often measured in studies on the misinformation effect, this does not mean the phenomenon does not affect the results. Rather, detection could be a powerful unmeasured factor in determining acceptance of misinformation. In fact, Loftus (1993) proposed that the reason misinformation is incorporated into later memory is "precisely because we do not detect its influence," however this intuition is still in need of empirical support (p. 530).

Given the procedural and conceptual similarities between these two paradigms, it is worthwhile to investigate the interplay between them. Two studies have done this (Sagana, Sauerland, & Merckelbach, 2013; Sagana et al., 2014). The basic methodology of these studies parallels other choice blindness studies. However, instead of asking participants to complete a scale or make a preference decision, participants answer questions about their memory for a previously witnessed event. Both studies use a lineup as the critical item. After making an identification, participants are told they will be given the photograph of the person they chose and are then asked to explain why they picked this person. On manipulated trials, the photograph shown is actually a randomly selected other member of the lineup.

This procedure was first used in a field study (Sagana et al., 2013). Two confederates approached a tourist to ask for directions. Shortly thereafter, another experimenter approached the individual and informed him that the people asking for directions were actually confederates in a research study. The person was then led through the choice blindness paradigm where they identified the two experimenters from a lineup. Only 31% of participants concurrently detected the manipulation. Detection status did not affect identification accuracy. While this study does center on memory, it is not directly a misinformation study. Participants do complete the first two stages of the misinformation paradigm—witnessing an event and receiving misinformation—but their memory for the event is not later tested. Thus, no assessment of how the misinformation affected participants' memory can be measured.

The second study on memory and choice blindness added in this final stage (Sagana et al., 2014). Overall, the manipulation did not affect participants' identification on a second, post-manipulation, lineup. However, this finding may be an artifact of the small sample size used. In Experiment 2, at most, two people switched their identification on the second lineup. Rates of blindness may also have played a role in the null results. Blindness varied as a function of the length of the retention interval between the original identification and the manipulation. When the manipulation occurred immediately after the lineup, rates of combined detection were near 100%. However, when the manipulation occurred 24 hours after the initial lineup, rates of detection decreased and almost 20% of participants were blind to both manipulations (Sagana et al., 2014). This is consistent with what the misinformation literature would predict. Misinformation is particularly likely to be incorporated into memory when there the delay between the original event and the misinformation increases. This time allows for the memory of the original event to decay and thus participants are less likely to notice the misinformation

(Loftus, 2005). Given the similarities between the two paradigms, the emerging literature on choice blindness would benefit from applying the findings of the misinformation literature. Indeed, Sagana and colleagues (2014) stated outright that "we believe that future studies on choice blindness and eyewitness identification might profit from an explicit consideration of the misinformation literature" (p. 762).

The Current Study

The current study uses principles from the misinformation literature and applies them to a choice blindness paradigm for an eyewitness identification decision. Unlike Sagana and colleagues (2014), the current study uses a target-absent lineup. If a target-present lineup were used, then participants who made a correct identification would receive misinformation that led them away from the correct answer whereas the rest of participants would receive misinformation that led them from one false answer to a different false answer. By using a target-absent lineup, this confound is avoided.

One limitation of the current choice blindness literature is that it relies on small sample sizes (e.g. Johansson et al., 2014; Sagana et al., 2014). The original sample is often further divided between detectors and non-detectors and between participants who show evidence of memory change and those who do not, thus further reducing power. Drawing from the misinformation literature (Loftus, 2005), this study uses longer retention intervals between the original event and the misinformation and between the misinformation and the later test than in typical choice blindness studies (e.g. Johansson et al., 2014). These longer retention intervals decrease the chances of detection and increase the chances of memory change at the later test. This increases the likelihood of having more participants who are non-detectors and more participants who show evidence of memory change, allowing for greater power to examine these understudied groups.

In the current study, participants watched a slideshow and made a lineup identification. They were then presented with accurate, inaccurate, or no feedback. At the end of the study, participants again made a lineup identification. It was hypothesized that a significant portion of participants would be blind to the manipulation. As the costs of blindness in this study are less consequential than that of falsely reporting on norm-violating behaviors, we expected a lower rate of detection than the 82.1% found in the past study (Sauerland, Schell, et al., 2013). However, as the manipulation here is of a categorical variable, detection may be more likely than in studies manipulating responses on a likert-type scale since the manipulation is distinctly different than the chosen option (Hall et al., 2012; Tousignant et al., 1986). It was further hypothesized that those exposed to choice blindness misinformation would be more likely to change their identification on a post-manipulation lineup than those receiving accurate feedback or no feedback. This effect was hypothesized to be stronger for non-detectors than for detectors based on the discrepancy detection principle (Tousignant et al., 1986).

Several exploratory analyses were run to investigate whether detectors differed from non-detectors on a variety of behavioral and individual differences measures including confidence, response time, and word count. Due to the ethnic diversity of the sample, an investigation of possible cross-race effects was conducted. Based on the literature demonstrating that cross-race identifications are less accurate than same-race identifications, it was hypothesized that participants making a cross-race identification would be more likely to fail to detect the manipulation than those making a same-race identification (Meissner & Brigham, 2001). Similar to Johansson and colleagues (2005b), participants' free responses about the reasons for their choice were analyzed for degree and type of confabulation. It was hypothesized that participants receiving the misinformation would be less likely to mention physical aspects of the person they chose than those receiving no or accurate feedback (Schooler, Gerhard, & Loftus, 1986).

CHAPTER 2: METHODOLOGY

Participants

Participants were undergraduate students from the University of California, Irvine (*N* = 392). Due to technical issues, 13 participants did not watch the critical slideshow and were excluded from analysis leaving a final sample of 379. Participants (78.4% female) earned partial course extra credit for their participation. Consistent with the demographics of the undergraduate student population, the sample was mostly Asian-American (45.4%) and Hispanic (27.4%). Caucasians comprised 17.2% of the sample and the remaining 10% identified as another race or chose not to respond. Ages ranged from 18-58 (*M* = 21, *SD* = 3.9).

Design

The present study had three conditions: control, confirming information (called "non-manipulated" in choice blindness studies), and manipulated. In the control condition, participants received no feedback about their identification. In the confirming information condition, participants received accurate feedback about their identification. In the manipulated condition, participants received misleading feedback about their identification decision. The misleading feedback was presented as if it were the participant's own prior identification.

Materials

Slideshow. The slideshow depicted a Caucasian man on a residential street. He walked to a car in front of a home and broke into the car. He stole the radio and left. The slideshow contained eleven slides shown for two seconds each and the man's face was in view for 18 seconds.

Lineups. Lineup photographs were taken from two databases: the Psychological Image Collection at Stirling (pics.stir.ac.uk) and the Center for Vital Longevity Face Database (Ebner, 2008). Photographs were in color and 200x200 pixels. All faces were edited to be on a white background using Adobe Photoshop Elements 9.

To pilot test the faces to be included in the lineup, a separate sample of twenty-four UCI undergraduate students (83.3% female) was recruited. Participants rated the similarity of pairs of faces on a 7 point scale (1- "*not at all similar*," 7- "*highly similar*"). The goal was to create a lineup of dissimilar faces so that changes in identification decisions from lineup 1 to lineup 2 could be attributed to the manipulation rather than confusion due to facial similarity. Average similarity of a filler face to the target ranged from 1.87 to 5.25. Faces whose similarity to the target was greater than the midpoint were excluded. Six of the remaining photographs were chosen for use in the current study. Mean similarity of the final sample range from 1.87 to 3.41 (M = 2.53, SD = 0.77).

Both lineup 1 and lineup 2 were simultaneous, six-person, target-absent lineups (i.e. the actual perpetrator was not included). Photographs were presented in random order in two columns and participants were not given the option to reject either lineup. Lineup 1 and lineup 2 contained the same six photographs but the order of each was randomized so that the two lineups were not identical. This was done to ensure participants were not making an identification in lineup 2 based on the position of their choice in lineup 1.

Procedure

Event and Lineup 1. Participants accessed a link to the study (hosted on Qualtrics) on their personal computers. They first watched the critical slideshow. Next, they completed filler tasks (e.g. analogies, mental rotation tasks) for 10 minutes. These tasks were consistent with the cover story given to participants when they signed up for the study. These filler tasks served as a retention interval to allow participants memory for the slideshow to partially decay. Following this, participants were asked several questions about the slideshow they witnessed at the beginning of the study. The initial questions queried about details unrelated to the thief (e.g. “What color was the car?”) to further increase the retention interval. Then participants viewed lineup 1 and rated their confidence on an 11-point scale (1- “0% confident my decision was correct,” 11- “100% confident my decision was correct”).

Manipulation. Participants then finished another retention interval where they completed personality and individual difference questionnaires. This, and the subsequent retention interval, lasted between 10-15 minutes. This retention interval allowed time to pass between the participant’s identification and the manipulation so that rates of detection would not be at ceiling level.

At this point, participants were randomly assigned to one of the three conditions: control, confirming information, or manipulated. Those assigned to the confirming information condition instead read the following statement: “Earlier in the study, you picked the photo of the man you saw in the slideshow. On the next page, you will briefly see the photo of this person.” When participants advanced to the next page, the exact photograph they picked was shown for four seconds. After this, participants were presented with a free response question where they were asked to explain why they selected that person from the lineup. Instructions read: “Use the space to describe, in detail, why you selected this picture. There is no right or wrong answer. Take as much time as you need and please be as detailed as possible in your response.” Below the instructions, participants were given space to type in their response. Unlimited space was given and no time restrictions were imposed.

Participants in the manipulated condition viewed the same instructions as those in the confirming information condition. They were told that they would see a photograph of the man they selected from the lineup. However, when they advanced to the next page, the photograph shown was a randomly selected, non-chosen option from lineup 1. After this, participants received the same instructions as the confirming information condition.

In the control condition, participants were not shown a photograph. Prior to the free response question, control participants were asked to think back to when they selected the man they saw from the slideshow. They were then asked to explain why they picked this person. Instructions for this task were the same as instructions after the photograph was shown for the confirming information and manipulated conditions.

Lineup 2. Following another retention interval similar to the previous one, participants completed lineup 2. Lineup 2 was identical to lineup 1 except that the order of faces was randomized. Confidence in this choice was assessed with the same scale described previously. Finally, participants completed a basic demographics questionnaire and a funneled retrospective detection debriefing.

CHAPTER 3: RESULTS

Detection

Concurrent detection was measured by coding participants' free responses when they described their reasons for making their identification. Two research assistants, blind to hypotheses, were trained in evaluating participants' responses to assess whether they detected the manipulation (see Tables 1 and 2 for examples). The two raters agreed on 99.5% of responses. Disagreement was resolved by a third rater. Zero responses in the confirming information and control conditions were rated as concurrently detected providing further indication of the accuracy of the coding. In the manipulated condition, 47.2% of participants concurrently detected the manipulation.

Memory change from lineup 1 to lineup 2

Memory change was operationalized through participant's consistency at lineup 1 and lineup 2. Memory change occurred when participants selected a different lineup member for lineup 2 than they did for lineup 1. Participants who made the same identification for lineup 1 and lineup 2 were coded as showing no memory change.

Overall, 25% of participants showed evidence of memory change. A 3 (condition: confirming information, control, manipulated) x 2 (memory change: change or no change) chi-square test was conducted to investigate if this differed amongst conditions. Results showed significant differences between the conditions, χ^2 (2, N = 379) = 10.69, p = .005, Cramer's V = .168. Three planned, follow-up 2x2 chi-square tests were conducted to investigate where differences occurred. Memory change did not differ between the confirming information and control conditions, χ^2 (1, N = 252) = 0.90, p = .343. Receiving accurate feedback about one's identification decision did not significantly decrease the degree of memory change compared to those who received no feedback. However, participants in the manipulated condition showed a significantly higher rate of memory change than the confirming information condition, χ^2 (1, N = 253) = 4.80, p = .029, Φ = .138, and the control condition, χ^2 (1, N = 253) = 9.69, p = .002, Φ = .196. These results indicate that being exposed to the misinformation caused participants to more often change their identification from lineup 1 to lineup 2 than participants not receiving misinformation (Figure 1).

However, this analysis may be misleading as it is collapsed across detection status. Detectors changed 13.3% of the time from lineup 1 to lineup 2. This differs significantly from non-detectors who changed 53.7% of the time, χ^2 (1, N = 127) = 22.82, p < .001, φ^2 = .42. A 3 (group: confirming information/control, detectors, non-detectors) x 2 (memory change: change or no change) chi-square test was conducted to investigate whether detectors and non-detectors differed from those not receiving misinformation on memory change. As there were no significant differences between the control and confirming information condition in the previous analysis, these conditions were collapsed. There were differences amongst the groups, χ^2 (2, N = 379) = 37.62, p < .001, Cramer's V = .315. Two planned 2x2 chi-square tests were conducted to investigate where differences occurred. There were no significant difference between detectors and the control/confirming information groups, χ^2 (1, N = 312) = 1.36, p = .244. Non-detectors switched their identification from lineup 1 to lineup 2 significantly more than participants in the control/confirming information groups, χ^2 (1, N = 319) = 30.87, p < .001, Φ= .311 (Figure 2). These results suggest that it is not the misinformation itself that is causing memory change, it is detection status. Detectors are no more likely to show memory change that people not exposed to misinformation. However, non-detectors are significantly more likely to show memory change than any other group. In fact, the majority (53.7%) of non-detectors switched their identification from lineup 1to lineup 2.

When participants in the manipulated condition changed their identification at lineup 2, they mostly changed in the direction of the manipulation. That is, of those in the manipulated condition who switched, 57% switched to the manipulated face. The average participant thus changed to another face roughly 10% of the time. This rate of change is significantly greater than that expected by chance, χ^2 (1, N = 94) = 13.58, $p < .001$.

Confidence

Despite viewing a target-absent lineup, participants had relatively high confidence in their lineup decision both at lineup 1 (M = 5.63, SD = 2.48) and lineup 2 (M = 6.16, SD = 2.69; see Figure 3). Overall, confidence in lineup 2 was significantly greater than in lineup 1, t (378) = 6.72, $p < .001$. Confidence change did not differ amongst conditions, F (2, 378) = 2.23, p = .108, or between detectors and non-detectors, t (125) = 0.74, p = .46. Confidence in lineup 1 had no effect on whether participants later concurrently detected the manipulation, t (125) = 0.37, p = .722. Moreover, neither condition, F (2, 378) = 0.85, p = .427 nor detection status, t (125) = 0.80, p = .423, affected confidence in lineup 2.

As participants were not given the option to select “none of the above” during the lineup, it was decided a priori that confidence would be used as a proxy for estimating which participants would have chosen this option. Participants at the lowest two levels of confidence during lineup 1 were removed and the analyses were re-run to investigate whether the effects differed if only those participants who likely would have made a lineup identification were included. This left a sample size of 335 participants. Concurrent detection rate for this sample was 45% compared to 47.2% for the complete sample.

For the modified sample 17.1% of participants in the control condition, 22.1% of the confirming information condition, and 32.4% of the manipulated condition changed their identification from lineup 1 to lineup 2—rates very similar to that of the full sample. The results of the chi-square analyses comparing the three conditions are similar to that of the full sample although the comparison between the manipulated condition and the confirming information condition is now marginally significant, χ^2 (1, N = 224) = 3.00, p = .083. The significance levels for the results of chi-square analysis separating across detection status remain the same with the modified and full sample.

Distinguishing between detectors and non-detectors

A secondary purpose of the study was to conduct exploratory analyses examining differences between detectors and non-detectors. It was hypothesized that participants making a cross-race identification would be less likely to concurrently detect the manipulation. This hypothesis was not supported; Caucasian participants were not any less likely to concurrently detect the manipulation than participants of other races χ^2 (1, N = 127) = 0.37, p = .543. Several behavioral and individual difference measures were analyzed to investigate whether detection was based in part on personality. Overall, detectors and non-detectors were remarkably similar on all of these measures (Table 3). While this may be discouraging to those trying to predict who is most susceptible to choice blindness manipulations, the similarity of scores on the social desirability measure do provide some evidence that detectors are not simply more likely to be responding to demand characteristics.

Detectors and non-detectors differed on the number of words they wrote during the free response section, t (125) = 2.73, p = .007. Detectors (M = 23.12, SD = 21.75) wrote fewer words than non-detectors (M = 33.48, SD = 21.02). This effect was likely due to the fact that a majority of detectors simply responded that the picture they were shown was not the one they chose and did not elaborate further (see Table 1). To test whether non-detectors differed from participants in the control and confirming information condition, a one-way ANOVA with number of words written serving as the dependent variable and condition separated by blindness (control, confirming information, detectors, and non-detectors) serving as the independent variable was conducted. There were significant differences between the groups, $F(3, 378) = 9.25$, $p < .001$. Post-hoc analyses revealed that detectors wrote significantly fewer words than participants in the control and confirming information condition, *p*s < .001. However, non-detectors did not significantly differ from participants in the control or confirming information conditions, *p*s > .10. Non-detectors wrote no fewer words when describing their reasons for their identification than participants in the control and confirming information conditions despite being exposed to and being blind to misinformation (Figure 4).

Confabulation

Confabulation was defined as any response that explained a reason for the participant's lineup choice in the manipulated condition (see Table 2 for examples). Examples of responses not coded as confabulation were "That doesn't look like the picture I choose" and "I just chose someone I honestly do not remember." In the manipulated condition, 66.1% of participants showed evidence of confabulation. Non-detectors comprised most of this estimate; 91% of non-detectors confabulated a response for their choice. Consistent with past research, people easily confabulated reasons for a choice they never made (Johansson et al., 2005b).

Two research assistants, blind to hypotheses coded the first 50 participants free response data for type of confabulation. They were consistent on 86.5% of responses. The subsequent data was coded by one research assistant. It was hypothesized that participants in the manipulated condition would be less likely to mention physical aspects of the person in the slideshow compared to those not receiving the misinformation. For this analysis, the confirming information and control conditions were collapsed to allow for a comparison between those exposed and not exposed to the misinformation. Additionally, the analysis excluded detectors. A large number of detectors wrote only that they were shown the incorrect face and were thus obviously coded as not mentioning physical features. If included, these participants would obscure the effects of interest in this analysis. The hypothesis was not supported, χ^2 (1, N = 319) = 0.01, p = .921. Non-detectors reported on the physical features of the chosen face to the same degree that participants in the control and confirming information did.

CHAPTER 4: DISCUSSION AND CONCLUSIONS

Overall, our main hypotheses were supported. Over half of participants did not concurrently detect the manipulation. This number fits in well with the results of past studies. In the current study, the number of detectors was more than double that of the study where participant's judged facial attractiveness and somewhat smaller than when participant's reported on their norm violating behaviors (Johansson et al., 2005b; Sauerland, Schell, et al., 2013). Increasing the length of the retention intervals resulted in the predicted effect of decreasing the number of detectors as compared with past literature (Sagana et al., 2013, 2014).

Moreover, our hypotheses about the effect of the manipulation on memory change for lineup 2 were supported. Those in the manipulated condition were significantly more likely to show memory change—that is to make a different identification in lineup 2 than they did in lineup 1—than those who did not receive misinformation. Surprisingly, there were no differences between the control and confirming information groups. It was predicted that participants in the confirming information group would be less susceptible to memory change as they were provided with an accurate retrieval cue. One possible explanation for this is that although both the confirming information and control groups were asked the same question, the presence of the photograph before the question for the confirming information group may have caused these participants to construe the question differently and thus use different aspects of their memory to answer it. Participants in the confirming information group may have focused solely on their memory of the person's face because of the recent retrieval cue whereas participants in the confirming information group may have recalled more general features of the event. This may have resulted in a greater verbal overshadowing effect for the confirming information group compared to the control group, thus resulting in a greater rate of memory change for the confirming information group (Schooler & Engstler-Schooler, 1990).

Consistent with the Discrepancy Detection principle, detection status affected the likelihood of memory change (Tousignant et al., 1986). Non-detectors demonstrated a greater rate of memory change than detectors. This is particularly problematic given rates of blindness were high, over 50%. When non-detectors picked a different person in lineup 2 than they did in lineup 1, 54% of the time this person was the manipulated face. This indicates that the presence of the misinformation is not simply degrading the original memory trace. If this were the case, then we would expect a high rate of memory change, but no pattern to non-detectors lineup 2 choice. This is not the case here suggesting that the misinformation is being incorporated into participants' memory such that they are significantly more likely to pick the manipulated face from the second lineup. While the rates are significantly different from chance, they are not near ceiling level. Why are some non-detectors switching to a different filler face? One explanation for this is that although the presence of the misinformation has degraded their original memory, it was not sufficiently powerful enough to be incorporated into these participants' memory. Therefore, their memory for the original picture has been disturbed but since the misinformation has not been fully implanted, they do not have a clear memory of who to pick. Evidence for this is shown in data on reaction times. For the manipulated condition, those who switched to a new filler (M = 22.15, SD = 6.54) took significantly longer to make an identification from lineup 2 than participants who switched to the misinformation face (M = 13.36, SD = 6.08), t (42) = 4.59, $p < .001$, suggesting, perhaps, greater uncertainty in their memories.

Exploratory analysis of detectors and non-detectors revealed mostly null effects. Detectors and non-detectors are indistinguishable in the current sample on a variety of individual difference measures and show no differences in response time or confidence in their identification for lineup 1 or lineup 2. This coheres with past literature which used latent semantic analysis and found nearly no differences between responses on manipulated and non-manipulated trials (Johansson, Hall, Sikström, Tärning, & Lind, 2006). This is troublesome as susceptibility to memory change at lineup 2 is nearly doubled for non-detectors versus all other participants. Therefore, if a witness were somewhat aware of the switch but explained it away as a problem with the computer rather than acknowledging or pointing out the mistake the misinformation would likely be incorporated into their memory. Presently there are few distinguishing characteristics to determine what type of person would be most at-risk for this kind of error. Future research could investigate this using new techniques such as eye-tracking or facial expression measures (Johansson et al., 2014).

Limitations and Future Directions

One limitation of the current study is that it did not provide a direct measure of confabulation. A person was coded as confabulating if they provided any reason for their choice when they were in the manipulated condition. As all of the photographs in the lineup were relatively similar, it was not possible to determine if the participant described their original choice or the manipulated choice ("specific confabulation;" Johansson et al., 2005b). Future studies should use materials that can differentiate between these types of confabulation. It would useful to see if those participants who fully accepted the manipulation and were coded as specific confabulation were also those most likely to switch the manipulated face in lineup 2. One reason gathering a more direct measure of confabulation should be central to future choice blindness studies is that confabulation goes a step beyond measuring blindness. While blindness measures a failure to notice change, confabulation represents an active, constructive process of creating an explanation for a choice that did not happen. Measures of confabulation also represent a source of rich data which may provide insight into mechanisms underlying memory change such as whether participants reporting specific confabulation are the most likely to fall for the manipulation.

Future research could also expand on this study by using target-present lineups and specifically testing whether participants are more likely to be blind when they are manipulated away from the correct answer versus when they are manipulated away from a filler identification. This would provide an indication of whether choice blindness misinformation affects false memories to the same degree that they affect true memories.

Conclusion

This study is one of the first to apply principles from the misinformation literature to a choice blindness paradigm. The successful fusion of these two related paradigms revealed that memory change is largely caused by non-detection of misinformation rather than by the presentation of misinformation alone. These findings could be applied back to the misinformation literature. Including a measure of detection in misinformation studies could reveal whether those participants who incorporate the misinformation in their memories are also those who do not detect it (Tousignant et al., 1986). This would provide more information about how the two paradigms compare.

Past literature has shown that human beings have poor introspective awareness into why they make the decisions they do (Nisbett & Wilson, 1977). The current study extends this line of research and provides evidence that people not only are unaware of the reasons for their decisions, they are, under certain circumstances, even unaware of being presented with an outcome different than their original decision. Furthermore, they will often confabulate reasons for these non-made choices. The consequences of this are problematic, especially in cases of eyewitness identification as eyewitness testimony represents one of the most persuasive forms of evidence in a courtroom despite its inaccuracies (Brigham & Bothwell, 1983).

One unanswered question in the choice blindness literature is whether the choice blindness manipulation can be used to positively change behavior. Some research on the misinformation effect has explored this question. Misinformation suggesting that participants loved eating asparagus as a child increased participants self-reported liking of asparagus (Laney, Morris, Bernstein, Wakefield, & Loftus, 2008). Participants who fell for the manipulation and developed a false memory reported that they were willing to spend more money on asparagus at the grocery store and reported being more likely to eat asparagus at a restaurant. This question has yet to be explored in a choice blindness paradigm. For instance, researchers could measure students' pre-exam emotions and then give students given inaccurate feedback suggesting they felt less stressed than they actually did. The extent to which people detect the false feedback and what effect the manipulation has on stress before a second exam could be investigated. The results of the current study confirm that the choice blindness manipulation can affect later memory and future research can build on this to examine how that memory can change behavior.

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Table 1

Example statements of detectors

"This was not the picture I selected."	"I don't think that's the picture I chose..."
"i dont recall choosing this person, i believe it was someone else"	"I think this is not a photo that I selected earlier. I chose a person with more round face, longer eyebrow and smaller eyes."
"I didn't even select this photo--I chose the guy with the only decent looking hair... I see what you're trying to do."	"I do not belive this was the photo I chose. If so, my mind is playing tricks on me."
"I don't think that was the person I picked out. However, If that really was the person I picked out then I guess there are similar features like the jawline and the nose. The hair thew me off a bit."	"No.There has been a mistake. I did not select that person. He looked wrong to me so i chose the sad looking guy. I know tests like these throw participants off so i went with the least likely one. You gave me the wrong guy."
"I did not pick this guy...so I am a bit confused as to why it said I picked him. I was debating on him and the guy I actually chose, and I went with the other guy because of having read what it said that it may not look exactly like the original."	"I was not trying to pick that person. I must have not checked that I picked the guy with the beard. I had wanted to pick the guy with the beard and long hair because that is who looked liked the most agreeable person to what I saw."

Note. Original spelling is included.

Table 2

Example statements of non-detectors

"I picked this photo because the guy looked very mono-toned. He had a sort of wide headed head and face."	"I think he has the same facial construction as the guy in the slideshow."
"I picked that picture because that picture closely resembled the picture of the man in the car. I did not vividly remember the man who is in the car so I picked the one that closely resembled him"	"I couldn't decide which picture resembled the guy in the slideshow the most, but I went with this one. His sideburns seem to relate the the original, and his eyes were not too dissimilar."
"I selected this picture because the person's facial features looked most recognizable to me from the slideshow."	"I chose the picture because based on my memory I thought it was the person from the video. I thought his hair style and face shape was similar to the man in the video."
"I remember seeing a man walking with his back hunched. I remember his body was chubby, so I picked the man with the chubbiest cheeks"	"I honestly couldn't tell between the pictures shown, so I simply picked the one that I thought best represented the image I thought I remembered. Especially his eyes I thought matched those of the man in the slideshow"
"I selected this picture because he looked like the man presented in the slideshow. The men in the other pictures either looked too thin or too heavy."	"I thought I chose the right person because I remember the side burns that the person had and I looked for the person who distinctively had them."

Note. Original spelling is included.

Table 3

Distinguishing detectors and non-detectors

	Non-detectors		Detectors			
	M	*SD*	*M*	*SD*	*t*	*p*
Response Time, Lineup 1	27.49	14.26	27.56	15.89	0.98	0.33
Response Time, Lineup 2	16.55	6.70	16.55	6.08	0.00	0.99
Social Desirability Scale	11.1	3.59	10.77	3.63	0.53	0.60
Need for Cognition	2.78	0.54	2.79	0.57	0.13	0.90
Cognitive Response Test	0.97	1.11	0.68	0.91	1.58	0.12
Cognitive Failures Questionnaire	3.08	0.47	3.08	0.57	0.02	0.99

Note. Values more than three standard deviations above the mean on response times were excluded from the analysis. (Broadbent, Cooper, FitzGerald, & Parkes, 1982; Cacioppo, Petty, & Kao, 1984; Crowne & Marlowe, 1960; Frederick, 2005)

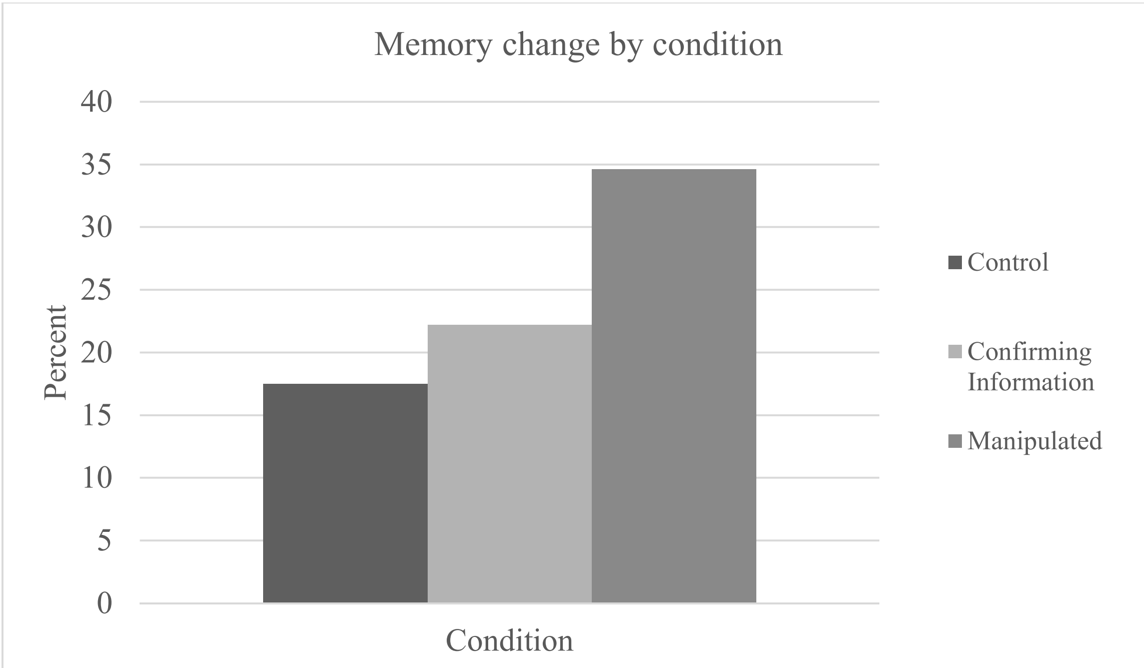

Figure 1. Percent of participants in each condition who changed their identification from lineup 1 to lineup 2.

Figure 2. Percent of participants who changed their lineup pick from lineup 1 to lineup 2 broken up by detection status.

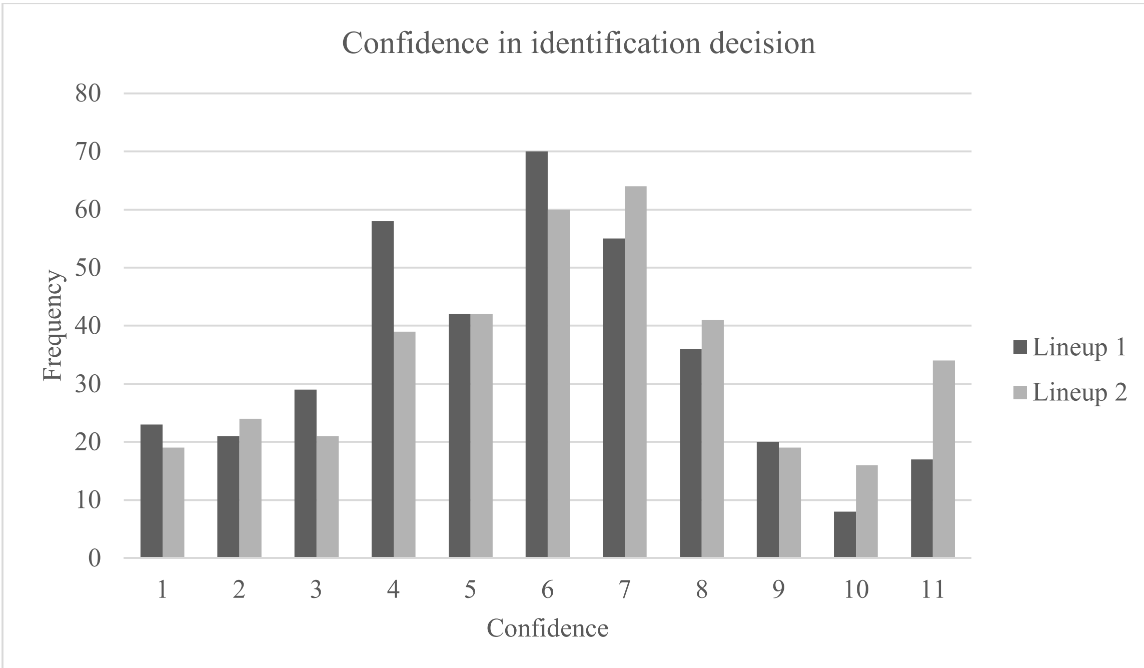

Figure 3. Confidence distribution in lineup 1 and lineup 2.

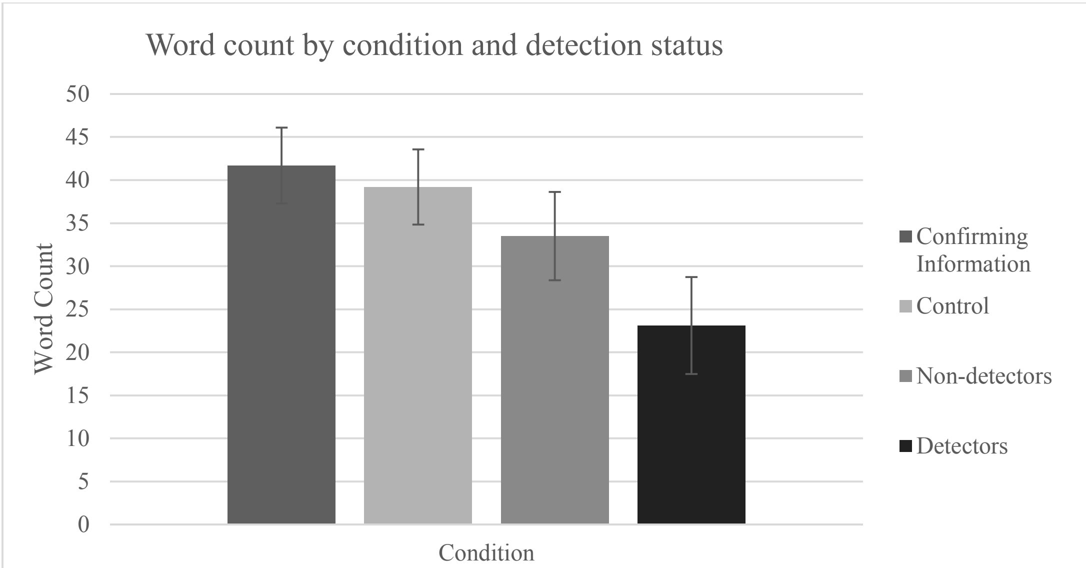

Figure 4. Number of words written during the free response section broken down by condition and detection status.