

THE SOCIAL CONTAGION OF MEMORY:  
MANIPULATING ITEM AND PERSON-BASED CREDIBILITY

by

Katya Terra Numbers

A thesis submitted in partial fulfillment  
of the requirements for the degree

of

Master of Science

in

Psychological Science

MONTANA STATE UNIVERSITY  
Bozeman, Montana

April 2011

©COPYRIGHT

by

Katya Terra Numbers

2011

All Rights Reserved

APPROVAL

of a thesis submitted by

Katya Terra Numbers

This thesis has been read by each member of the thesis committee and has been found to be satisfactory regarding content, English usage, format, citation, bibliographic style, and consistency and is ready for submission to The Graduate School.

Michelle L. Meade, Ph.D.

Approved for the Department of Psychology

Keith A. Hutchison, Ph.D.

Approved for The Graduate School

Carl A. Fox, Ph.D

STATEMENT OF PERMISSION TO USE

In presenting this thesis in partial fulfillment of the requirements for a master's degree at Montana State University, I agree that the Library shall make it available to borrowers under rules of the Library.

If I have indicated my intention to copyright this thesis by including a copyright notice page, copying is allowable only for scholarly purposes, consistent with "fair use" as prescribed in the U.S. Copyright Law. Requests for permission for extended quotation from or reproduction of this thesis in whole or in parts may be granted only by the copyright holder.

Katya Terra Numbers

April 2011

## TABLE OF CONTENTS

1. INTRODUCTION .....	1
2. EXPERIMENT 1 .....	9
3. METHOD .....	10
Participants.....	10
Design .....	11
Materials .....	11
Procedure .....	12
4. RESULTS .....	16
False Recall.....	16
Contagion Effects.....	16
Remember Judgments.....	18
Know Judgments.....	19
Recognition.....	20
False Source Judgments.....	20
Veridical Source Judgments .....	21
Final Questionnaires .....	22
5. EXPERIMENT 2 .....	24
6. METHOD .....	29
Participants.....	29
Design .....	29
Materials .....	29
Procedure .....	30
7. RESULTS .....	32
False Recall.....	32
Contagion Effects.....	32
Remember Judgments.....	34
Know Judgments.....	35
Recognition.....	36
False & Veridical Source Judgments.....	36
Final Questionnaires .....	38

## TABLE OF CONTENTS - CONTINUED

8. GENERAL DISCUSSION .....	40
Relation to Other Paradigms .....	41
Interpretation of Results.....	45
Practical Implications.....	49
REFERENCES CITED.....	52
APPENDICES .....	57
APPENDIX A: Confederate Scripts .....	58
APPENDIX B: Manipulation Check/Final Questionnaire .....	61
APPENDIX C: Feux Pilot Instructions.....	63
APPENDIX D: Feux Pilot Study Word List.....	65
APPENDIX E: Feux Pilot Study “Tester” (Participant) Instructions.....	67
APPENDIX F: Confederate Cheat Sheet for High- and Low-Credibility Conditions .....	69

## LIST OF TABLES

Table	Page
1. Proportion of False Recall and Remember or Know Responses for High- and Low-Expectancy Items as a Function of Proportion of Incorrect Items Suggested by the Confederates.....	18
2. Proportion of False Source Judgments for Critical Contagion Items and Veridical Source Judgments for Correct Items as a Proportion of Incorrect Items Suggested by the Confederates.....	22
3. Proportion of Participants Who Would Choose to Work With Their Partner Again, and Ratings of Partner Memory, and Partner Credibility as a Function of Incorrect Items Suggested by the Confederates.....	23
4. Proportion of False Recall, and Remember or Know Responses for High- and Low-expectancy Items as a Function of Proportion of Incorrect Items Suggested and Perceived Partner Credibility .....	34
5. Proportion of False Source Judgments for Critical Contagion Items and Veridical Source Judgments for Correct Items as a Proportion of Incorrect Items Suggested and Perceived Partner Credibility.....	37
6. Proportion of Participants Who Would Choose to Work With Their Partner Again, and Ratings of Partner Memory, and Partner Credibility as a Function of Incorrect Items Suggested and Perceived Partner Credibility .....	39

## ABSTRACT

Two experiments set out to examine the influence of item and person-based credibility on the adoption of misinformation using the social contagion paradigm developed by Roediger, Meade, & Bergman (2001). We presented dyads consisting of one naïve subject and one confederate with common household scenes and asked them to recall items from those scenes together. During this turn-taking phase, the confederate interjected 0%, 33%, 66%, or 100% false items. We termed this increasing proportion of false information suggested by the confederate item-based credibility, and manipulated it between participants. Experiment 2 added a novel manipulation of partner credibility, or the perceived memorial ability of the confederate. In general, participants who heard false items from the confederate were significantly more likely to write these items down on a later recall task, and were more likely to recognize these items as having occurred in the original slides, than participants who heard no false items. This was especially true for more typical (high-expectancy items). Interestingly, we did not obtain a significant decrease in contagion when participants encountered higher proportions of false information (Experiment 1), even when they were made aware of the confederates' "very poor" memory abilities (Experiment 2). It seems that our novel item and person-based credibility manipulations were not enough to reduce the contagion effect to any significant degree. Participants did not deduce or infer credibility based on content accuracy or memory performance on a related task, demonstrating just how susceptible our memories are to misinformation. The methods introduced here capture and extend the core phenomena of the social contagion effect, while simultaneously unveiling the robust nature of this paradigm.

## INTRODUCTION

Despite commonly held misconceptions of memorial accuracy and fixedness, research over the decades suggests that our reconstructions of the past are much more fragile, inventive, and unreliable than most people realize. We often forget some information, confuse aspects of different events, and are influenced by what other people say. In an early exploration of reconstructive memory, Bartlett (1932) read participants a Native American myth titled *The War of the Ghosts*; a tale ostensibly logical in plot but subtly illogical in details and construction. He then asked participants to recall the story in its entirety over the course of varying retention intervals. Bartlett found that the passage of time led to errors in remembering and eventually outright fabrications, with participants altering the story with successive attempts to remember it. He concluded that as participants lost details of the original event, they relied more heavily on frameworks of knowledge about the world—or schemas—to fill in the gaps.

Although the variable nature of our individual memories has been well established in the literature over time, a relatively new area of study, social misremembering, has only recently begun to garner the attention of researchers. Specifically, in more recent years efforts to illuminate antecedents, processes, and consequences of social influence on memory have begun to intensify (Echterhoff & Hirst, 2009). For example, current research has identified a phenomenon known as memory conformity. Memory conformity (see Wright, Self, & Justice, 2000; Gabbert, Memon & Allan, 2003; Gabbert, Memon & Wright, 2006) refers to the finding that when two people witness different versions of the same event and then come together to discuss it,

one person's memory report can influence what the other person subsequently claims to remember. Wright et al., (2000; Experiment 2) demonstrated that even when individuals' initial memories were quite accurate, three-quarters of participant-pairs exhibited some type of memory conformity after discussion with a partner. In other words, participants often change their responses on a memory test to conform to the responses provided by a fellow participant.

Memory conformity though robust in effect and observation, might be explained by several theoretical processes (Wright, Memon, Skagerberg, & Gabbert, 2009). Normative influences compare the cost of disagreeing with the cost of being wrong in a group setting. In his classic study of conformity, Solomon Asch (1956) led participants to believe they were taking part in a line judgment task with several peers that required them to make judgments about whether a line was the same length as several comparison lines. Asch discovered that although participants could perform this simple task in isolation, they often responded with incorrect answers when forced to give them in the presence of a unanimous majority. As such, he concluded that misreporting in these instances was due to compliance with group norms and a need to gain social acceptance. Deutsch and Gerard (1956) extended Asch's work with normative influences to tasks involving memory, and concluded informational influences can bias responses on memory tasks as well. This type of influence occurs when an individual turns to another in order to obtain information. Unlike normative conformity, informational social influence results from an individual thinking that someone else has more accurate information, such that they actually accept information obtained from others as evidence

about reality. They found informational influences were especially strong when the concern was to make accurate and valid judgments, where participants had to weigh the relative likelihood of the other person being correct versus the likelihood that they themselves were correct. Finally, memory conformity might be due to actual memorial distortions, meaning the information suggested by another person actually becomes incorporated in to one's episodic memory of the event. In the false memory paradigm most relevant to the present work, Loftus Miller, & Burns, 1978 showed participants a series of slides depicting a traffic accident, one of which contained a critical item (a stop sign). Participants then read a narrative that was either congruent (stop sign), incongruent (yield sign), or neutral (traffic sign) in regards to the critical detail presented in the original slides. On a later forced-choice recognition test, participants who had received inconsistent information were significantly less accurate than those who had received consistent information. Importantly, when participants were given a final opportunity to consider the source (slides vs. narrative) of the misleading information (yield sign), instances of erroneous reporting were still found, leading Loftus *et al*, to conclude that misreporting in their paradigm was due to actual memory distortions (see too Loftus & Palmer, 1974; Loftus, 1979). However, in these and many subsequent studies, the source of biasing information is merely implied. Only recently, as in the memory conformity studies mentioned above (e.g. Wright et al, 2000; Gabbert et al, 2003), have researchers extended this approach to examine how a physically present speaker may influence memory reconstruction.

The social contagion paradigm introduced by Roediger, Meade, and Bergman (2001) provides a technique to further examine social influences on episodic memory beyond public conformity or an informational acceptance of misinformation. Social contagion posits that if two people recall an event together, and one person recalls an erroneous detail not recalled by the other, that detail may become “contagious” such that the other person will include it in his or her later recall of that event. In the original paradigm, younger adult dyads consisting of one naïve participant and one confederate viewed slides of six common household scenes (e.g. bedroom) on a computer one at a time with instructions to remember the items from each scene for a later memory test. Next the confederate and participant completed a collaborative recall test, taking turns to recall out loud 12 items (six items each) from each scene. During this task the confederate interjected two errant items for three of the six scenes, one of which was a higher-expectancy item (e.g. alarm clock in the bedroom scene) and one of which was a lower-expectancy item (e.g. cologne in the bedroom scene). Participants were then given an individual recall test in isolation for which they were asked to write down as many items from each of the six scenes as they could remember, being sure to recall only items they were sure had been in the original slides. Further, participants were asked to assess their recollections using Tulving’s (1985) remember/know technique, meaning they were required to indicate whether they “remembered” specific details of the items they had written down or whether they just “knew” the items were there given a more general sense of familiarity. In their original study, Roediger et al. (2001) found participants recalled errant items suggested by the confederate more often than when they were not

suggested by a confederate, that participants recalled higher expectancy (more typical) items more often than lower expectancy items, and that participants tended to make more “know” judgments for the contagion items than remember judgments. In all, they concluded that the social contagion effect was probably due to private acceptance of the misinformation items rather than public conformity since individual recall tests were given in isolation, after a delay, and with the instructions to only recall items present in the scenes.

Still a question remained as to whether informational influences might be driving the contagion effect; especially given the preponderance of know, over remember, responses observed in the original experiment. In an effort to elucidate the role of private *acceptance* of misinformation versus actual disruptions in individual recollection, Meade and Roediger (2002) further explored the nature of false memories aroused in the original social contagion paradigm by adding a source monitoring test (Experiment 1). The source monitoring framework developed by Johnson, Hashrudi and Lindsay (1993) suggests that in addition to encoding an item itself, individuals also encode a variety of characteristics associated with the item. At retrieval, such additional characteristics may later serve as cues to the origin of the memory. Given this assumption, Johnson and colleagues developed a technique which provides participants the opportunity to say where (if anywhere) they recollect the occurrence of a given item in the context of the experiment. For example, participants might have the option of saying they remember a misinformation item only in the postevent suggestion, but that they do not recollect that it occurred during the original learning event. Specifically in the Meade and Roediger

(2002) study, participants were presented with a recognition/source-monitoring test that required them to identify the source of 36 items (half targets, half lures). They were asked to indicate if they recognized each item as having occurred in the scene (scene only), as having been mentioned by the other subject (other only), as having occurred in the scene and having been mentioned by the other subject (scene and other), or not having been in the scene or having been mentioned by the other subject (neither). Notably, participants were more likely to erroneously report contagion items as having occurred in the scenes even when they were given the explicit option to correctly say contagion items were *only* a product of the other subject. These findings further solidified the assumption that the social contagion effect, like the misinformation effect, is due to actual changes in episodic memory rather than public conformity or an informative acceptance of false information.

The purpose of the present set of experiments was to further explore the parameters of the social contagion effect by examining the role that credibility plays in the social contagion phenomenon. More specifically, we were interested in how item-based credibility, as well as person-based credibility, might modulate the adoption of misinformation. Item-based credibility in this case refers to the proportion of incorrect items suggested by the confederate, whereas person-based credibility refers to the memorial abilities (or expertise) of a perceived partner. Given that social contagion has only been demonstrated thus far when a small proportion of items suggested by the confederate are inaccurate, and that the paradigm typically includes a confederate “peer”,

it remains to be determined whether changes in item-based and/or person-based credibility may influence the magnitude of the social contagion effect.

The use of mostly accurate information in the social contagion paradigm is consistent with the misinformation paradigm put forth by Loftus and colleagues (see Loftus & Palmer, 1974; Loftus et al, 1978; Loftus, 1979) which employs the use of long narratives that change only minor details embedded in the context of complicated scenes. For example, a participant might be asked to delineate the presence of a stop sign versus a yield sign after viewing several scenes depicting a centrally captivating occurrence like an automobile accident. Importantly, research suggests that recollections are more likely to change if one can't immediately detect a discrepancy between misinformation and details of the original event (Tousignant, Hall & Loftus, 1986). The less detectable the discrepancy between the original learning event and postevent misinformation, the less likely that it will be rejected on a later memory test. Ironically, social contagion, as well as much of the false memory research, appears to be built on the assumption that our memories are in truth fairly accurate, such that to find an effect of misinformation the experiment must "slip in" incongruent items just below a level of immediate detection. But is this really the case? To examine item-based credibility in the current study we manipulated the proportion of incorrect items suggested by the confederate via condition (0%, 33%, 66%, or 100%). In the original social contagion paradigm a confederate interjected two errant items for three of the six scenes with the other half of the scenes serving as within-participants controls. In our modified paradigm, suggestion of contagion items was manipulated between participants, such that the condition in which

no misinformation items were suggested (0% incorrect) served as our control condition, allowing difference scores to be calculated by subtracting this condition from the remaining between-participants conditions. Our 33% incorrect condition was designed to roughly approximate the proportions used by Roediger et al, (2001) and Meade and Roediger (2002) to assure we were obtaining a comparable effect. Additionally, we increased the proportion of false items to 66% and 100% in the remaining conditions to determine whether the social contagion effect increased or decreased in the presence of mostly, or entirely, incorrect information. It was hypothesized that manipulating item-based credibility in our modified paradigm would decrease the magnitude of the social contagion effect, possibly because hearing increasingly incorrect items would draw attention to the errant nature of the items the confederate was producing.

Studies in individual memory paradigms suggest that false memories can be reduced by focusing attention at the item level. Neuschatz, Benoit, & Payne (2002) found that warnings about the convergence of items on a non-presented critical lure can substantially reduce false memories in the Deese–Roediger–McDermott (DRM) paradigm. Using the same paradigm, McCabe, Presmanes, Robertson and Smith (2004) found that item-specific processing reduced false memories both when using between, and when using within-participants designs. More evidence that items alone can act as a warning to reduce false memories can be found in recent research by Gallo, who instructed participants to use their recall of an event to reduce false-recognition on a later memory test. In his 2004 paper, Gallo explored Tulving’s (1983) recall-to-reject hypothesis by proposing two ways that recall might facilitate correct rejection of false

information: disqualifying and diagnostic strategies. Both processes rely on the strategic use of recalled information to monitor or edit memory accuracy at retrieval, either by an ability to disqualify a critical lure based on knowledge of the situation or by adoption of a diagnostic criteria that allows one to make inferences about the likelihood of a lure occurring. In his first experiment, Gallo found that explaining recall-to-reject strategies to participants helped them to later reject critical lures for shorter word lists, a finding that further suggests that in individual memory paradigms, items themselves can act as a cue to reject false information on later recall and recognition tests.

Taken together, these studies suggest that false memories can be diminished by focusing participants' attention at the item level through explicit warning about misleading effects. In the current paper we take a more equivocal approach to item warnings, assuming that higher proportions of false items alone could alert participants to discrepancies between original learning and postevent misinformation.

### Experiment 1

Given these types of item-based rejection strategies, Experiment 1 explores the effect of item accuracy on the likelihood that participants adopt confederate suggestions into their own memory reports. We hypothesized that increasing the proportion of false items suggested in each condition would act as a similar item-based warning, urging participants to consider the authenticity of the information being produced by their partner. Further, we hypothesized the detection of these errant items would translate into a means of rejections on a later individual recall test, meaning higher proportions of false

items should be inversely related to the magnitude of the social contagion effect. On a final note, we were interested in whether item-based credibility effects vary as a function of item expectancy. As such, our study includes both high expectancy items (those rated by many people to be typical of a given scene) and low expectancy items (those rated by few people as being typical of a given scene). Past research (see Roediger et al, 2001; Meade & Roediger, 2002) has shown that higher expectancy items are more prone to contagion than lower expectancy items; most likely the result of source confusion. Of interest to the current study was whether high and low expectancy items would be differentially affected by our manipulation of item-based credibility.

## Method

### Participants

Participants were 82 Montana State University Psychology 100 undergraduates who participated for partial course credit. Five participants had difficulty understanding/following instructions, two participants reported suspicion about the confederate, two participants were non-native English speakers and one subject was run in the wrong condition, so their data were excluded from analyses. As a result, our final analysis included the remaining 72 participants, with 18 participants comprising each of the four conditions.

### Design

This experiment consisted of a 2 x 4 mixed design. Expectancy of the contagion items within each scene (high-expectancy or low-expectancy) was manipulated within

participants, and the proportion of false information offered for each scene (0%, 33%, 66% or 100%) was manipulated between participants. The primary dependent variables were false recall and false recognition of the critical suggested items.

### Materials

Materials were the same as those used in Roediger et al. (2001). Six slides were presented on the computer depicting schematically consistent common household scenes (toolbox, bathroom, kitchen, bedroom, closet, and desk). Each scene contained an average of 23.8 items that were either highly typical for the scene (high-expectancy) or less typical in the context of the scene (low-expectancy). Item expectancy was determined through pilot testing conducted in the original Roediger *et al* study where students were cued with the name of each scene (e.g. bedroom) and were asked to write down any items they would expect to find within that scene. High- and low-expectancy items were then purposely excluded from each slide so that these items could be used as contagion items. The same contagion items generated by the original authors were used to construct our 33% incorrect condition; however our study required additional false items for each scene. As such, we ran a pilot study using the same materials as Roediger *et al* to generate 4 new contagion items and 4 alternate contagion items (8 items total) for each scene. A contagion item refers to any item suggested by the confederate that was not actually present in any of the scenes. Other materials included a filler task comprised of basic math (addition) problems, individual cued recall sheets, and a final individual recognition test. The recognition test contained ninety-six items; thirty-six of which represented contagion items, or items that were not in the scenes but may have been

suggested by the confederate in a given condition. Forty-eight of the items were items that had actually appeared in the scenes. The remaining twelve items served as filler items, meaning they did not appear in any of the scenes and were not suggested by the confederate. Additional materials included a final manipulation check questionnaire and demographic questionnaire, both locally developed (see Appendix B).

### Procedure

Participants arrived to the lab with a same age confederate whom they believed would be participating as their partner for the duration of the memory experiment. Participants and confederates were seated at computer terminals, with the participant always seated in front of the confederate so they could not view the confederates' computer screen. They then viewed slides of 6 common household scenes for 15 seconds each and were told they would need to remember items from each scene for a later memory test. The slides were always presented in the same order for every participant (toolbox, bathroom, kitchen, bedroom, closet, and desk) and were verbally labeled by the experimenter as they appeared on the screen. After viewing the six scenes, both the participant and confederate completed a four minute filler task to limit rehearsal of the slides. Next, the confederate and participant took turns recalling items from each scene, one scene at a time, until each had recalled 6 items (12 total) for all 6 of the scenes. It was during this collaborative recall phase that the confederate interjected some, or no, errant items for each of the scenes. Condition 1 (0% false items) served as our control condition as no false items were offered by the confederate for any of the scenes. This condition allowed us to assess a baseline probability that our thematically consistent critical items

would be generated by participants when they were not offered by the confederate (cf. Brewer & Treyens, 1983). Condition 2 (33% false items) was an ostensible replication of Meade & Roediger's (2002) paradigm, such that we used the same high- and low-expectancy items and presented them in the same positions (four and six, respectively) as the original authors. However, it is important to note that our participants heard two false items for *all six* scenes, as opposed to just three of the scenes, which was the case in the original authors' study. In condition 3 (66% false items) our confederate suggested two high-and two low-expectancy false items for each scene. High expectancy items were always offered in positions four and five, and low-expectancy items were always offered in positions two and six. In condition 4 (100% false items), all items offered by our confederate were false—three high-expectancy (positions one, four and five) and three low-expectancy (positions two, three and six). Confederates were armed with a script that automatically advanced on their computer screen (out of view of the participant) after the original household scenes had appeared. There were four scripts tailored to the four conditions. Each script contained some (or no) high- and low-expectancy veridical and errant items, as well as alternatives that could be offered in the event a participant freely recalled one the confederates' scripted items (see Appendix A for example scripts). Confederates were further instructed to match the subject's level of recall and confidence throughout the experiment. In this way our confederate appeared in all respects to be identical to the participant in terms of memorial abilities.

Following group recall, the participant and confederate were separated into two rooms on the pretense that both would be completing an individual recall test.

Participants were given a sheet of paper by the experimenter cuing them with the title of the scene (e.g. bedroom) and were told they had two minutes to write down any item, in any order, they could remember as having occurred in the scene. After two minutes the experimenter would reenter the room to collect their responses and give them the next sheet of paper; this continued until the participant had recalled items from all six of the scenes. The door to the confederate's room was also opened every two minutes to continue the guise that the confederate was completing the same set of tests. Participants were also asked to make remember/know judgments for each of the items they wrote down (see Gardiner, 1988; Tulving, 1985; Rajaram, 1993). A "remember" response was meant to indicate that participants recollected something specific about the item they were recalling (e.g. they remembered having noted something about the way an item looked or its position in the scene). A "know" response was given when participants lacked any memory of specific details about the item, but nonetheless were left with a general sense of familiarity meaning they just "knew" the item was there.

After the recall test, participants completed a 96-item source recognition test which allowed them to indicate where, if anywhere, they encountered a given item in the context of the experiment. Participants were asked to indicate for each item whether they recognized it as having occurred in one of the scenes (scene only), as having been mentioned by the other subject (other subject only) as having been both in the scene and mentioned by the other subject (scene and other), or not having been in the scene and not mentioned by the other subject (neither). There was no time limit for this task, though most participants completed it in less than 10 minutes. Finally, participants were given a

manipulation check that required them to indicate how credible they thought their partner was, how accurate they thought their partner's memory was, and whether or not they would choose to work with their partner again if monetary compensation was contingent on partner accuracy. The complete questionnaire is provided in Appendix B. After completing this final questionnaire and demographic information, participants were fully debriefed and thanked for their time.

## RESULTS

False RecallContagion Effects

False recall results for critical contagion items are shown in Table 1 separately for participants who heard 0%, 33%, 66% and 100% false items, as well as for high- and low-expectancy items. The top four rows in the table show individual recall of the critical contagion items as a function of proportion of incorrect critical items encountered, the next four rows illustrate remember judgments for critical contagion items produced on the individual recall test, and the final four rows report know judgments for critical contagion items on the individual recall test. Critical contagion items are defined as the two false items offered for each scene by confederates in the 33% incorrect condition (replicating the items used by Roediger et al. 2001 and Meade & Roediger, 2002). These items were always offered in positions four and six and were tracked across all conditions, regardless of proportion of false information, to obtain our contagion effect.

A 4 (proportion of incorrect items: 0%, 33%, 66%, 100%)  $\times$  2 (item expectancy: high vs. low) mixed factorial ANOVA, with item expectancy as a within-participants variable, was conducted on the mean proportion of critical contagion items recalled. A main effect of proportion of incorrect items was found,  $F(3, 68) = 9.227$ ,  $MSE = 0.023$ ,  $p < .001$ . Follow up tests confirmed that participants in the 33% incorrect condition ( $M = .38$ ) showed more contagion than those in the control (0% incorrect) condition ( $M = .14$ ,  $t(34) = -4.72$ ,  $SEM = .050$ ,  $p < .001$ ); that participants in the 66% incorrect condition ( $M =$

.39) showed more contagion than those in the control condition,  $t(34) = -4.560$ ,  $SEM = 0.023$ ,  $p = .006$ ; and that participants in the 100% incorrect condition ( $M = .30$ ) showed more contagion than those in the control condition,  $t(34) = -4.420$ ,  $SEM = 0.027$ ,  $p < .001$ . Further, these tests revealed no significant differences for recall of contagion items between the 33%, 66% and 100% incorrect conditions ( $t$ 's  $\leq 1.582$ ,  $p$ 's  $> .05$ ). In other words, the significant main effect of proportion appears to be driven solely by the difference in recall for false items that were suggested by the confederate compared to when those items were not suggested by the confederate. This is known as the contagion effect and replicates previous findings by both Roediger et al. (2001) and Meade and Roediger (2002). There was also a significant main effect of item expectancy,  $F(1,68) = 86.956$ ,  $MSE = 0.023$ ,  $p < .001$ , meaning participants were more likely to recall higher expectancy (more schematically consistent) contagion items ( $M = .42$ ) than lower expectancy (less schematically consistent) contagion items ( $M = .19$ ). Greater recall for high- as compared to low-expectancy items also replicates Roediger et al. (2001) and Meade and Roediger (2002), and is consistent with other literature on associated word items. No interaction emerged between the main effects of proportion and expectancy,  $F(3,68) = 1.304$ ,  $MSE = .030$ ,  $p > .05$ , meaning participants were more likely to recall higher expectancy items in general, regardless of condition. Contrary to our hypothesis, these results suggest that increasing the proportion of false information suggested by the confederate (to the point where *every* item suggested by the confederate was false) had little, if any, effect on participants' false recall of the high and low expectancy critical items.

Table 1. Mean proportion of false recall, and remember or know responses for high- and low-expectancy items as a function of proportion of incorrect items suggested by the confederate. Standard deviations are reported in parentheses.

Proportion Incorrect Items	High Expectancy Items	Low Expectancy Items	Overall Contagion Effect
<b>Recall</b>			
0%	.23 (.14)	.06 (.08)	.15
33%	.51 (.18)	.25 (.22)	.38
66%	.49 (.26)	.29 (.24)	.39
100%	.45 (.19)	.19 (.13)	.32
<b>Remember</b>			
0%	.05 (.17)	.02 (.07)	.04
33%	.16 (.16)	.05 (.19)	.11
66%	.19 (.20)	.08 (.19)	.14
100%	.17 (.21)	.06 (.13)	.12
<b>Know</b>			
0%	.18 (.17)	.04 (.07)	.11
33%	.35 (.16)	.20 (.19)	.28
66%	.30 (.20)	.17 (.19)	.24
100%	.28 (.21)	.13 (.13)	.21

### Remember Judgments

Remember and know responses were analyzed separately to determine how they were influenced by proportion of incorrect items and expectancy. A 4 (proportion: 0%, 33%, 66%, 100%) X 2 (high- or low-expectancy) mixed ANOVA revealed a significant main effect of proportion,  $F(3, 68) = 4.22$ ,  $MSE = 0.021$ ,  $p = .009$ , and expectancy  $F(1,68) = 19.62$ ,  $MSE = 0.021$ ,  $p < .001$ , but no significant interaction between these variables  $F(3,68) = 1.30$ ,  $MSE = 0.021$ ,  $p > .05$ . Follow up tests revealed a similar pattern as results obtained for false recall, such that participants in the 33% incorrect

condition ( $M = .107$ ) gave more remember responses for critical contagion items than those in the control condition ( $M = .04$ ;  $t(34) = -2.64$ ,  $SEM = 0.026$ ,  $p = .012$ ). The same was true for participants in the 66% incorrect condition ( $M = .16$ ;  $t(34) = -3.79$ ,  $SEM = 0.032$   $p = .001$ ) and the 100% incorrect condition ( $M = .10$ ,  $t(34) = -2.26$ ,  $SEM = 0.029$   $p = .03$ ). As with false recall, these tests also revealed no significant differences for remember judgments when comparing the 33%, 66%, and 100% incorrect conditions ( $t$ 's  $\leq 1.38$ ,  $p$ 's  $> .05$ ). These results suggest participants are more likely to give remember responses for false items when they are suggested by a confederate compared to when they are not suggested by a confederate, but that increasing the proportion of false items from 33% to 66%, or 66% to 100% had no significant effect on the proportion of remember judgments for those conditions. Expectancy on the other hand does appear to moderate remember responses as participants were more likely to give remember judgment to high-expectancy than to low-expectancy items, again replicating Roediger et al. (2001) and Meade and Roediger's (2002) findings.

### Know Judgments

A separate 4 (proportion: 0%, 33%, 66%, 100%) X 2 (high- or low-expectancy) mixed ANOVA was run on know responses, once again revealing a significant main effect of proportion  $F(3, 68) = 4.896$ ,  $MSE = 0.021$ ,  $p = .004$ , and expectancy  $F(1,68) = 35.061$ ,  $MSE = 0.037$ ,  $p < .001$ , with no significant interaction occurring between the two main effects  $F(3,68) = .075$ ,  $MSE = 0.021$ ,  $p > .05$ . Moreover, follow up tests reveal the same pattern of results obtained in the previous two analyses, meaning participants in the 33% incorrect condition ( $M = .27$ ) gave more know responses for critical contagion items

than those in the control condition ( $M = .11$ ;  $t(34) = -3.87$ ,  $SEM = 0.043$ ,  $p < .001$ ). This same pattern was obtained for participants in the 66% incorrect condition ( $M = .23$ ;  $t(34) = -2.68$ ,  $SEM = 0.047$   $p = .011$ ) and the 100% incorrect condition ( $M = .20$ ;  $t(34) = -2.58$ ,  $SEM = 0.036$   $p = .014$ ). Coinciding with the false recall and remember results, these tests also revealed no significant differences for remember judgments for contagion items when comparing the 33% and 66% incorrect conditions and the 66% and 100% incorrect conditions [ $t$ 's  $\leq 1.67$ ,  $p$ 's  $> .05$ ]. These findings indicate participants were more likely to give know responses for items that were suggested by a confederate compared to those not suggested by a confederate, but that participants in the 33%, 66%, and 100% incorrect conditions were equally likely to give know judgments for false items. Further, know responses were more likely for high-expectancy versus low-expectancy items, once again replicating Roediger et al. (2001) and Meade and Roediger (2002).

## Recognition

### False Source Judgments

Participants' mean performance on the recognition/source monitoring test is shown in Table 2 for contagion items and correct items as a function of proportion of incorrect items offered by the confederate (condition). Participants were given the option to say they encountered an item in the context of the slides only ('scene'), that they heard the item from the other subject only ('other'), that they both saw and heard the item ('scene and other'), or that the item was completely novel ('neither'). False recognition on the source-monitoring test was defined at the proportion of critical contagion items

participants recognized as having occurred in the slides ('scene') plus the proportion of items participants recognized as having occurred in both the slides and as having been suggested by the confederate ('scene and other'). In both instances, participants erroneously attribute a contagion item to having occurred in the scene when in fact it was only suggested by the confederate. These data are shown in the first column of Table 2. Participants were numerically more likely to report contagion items as having occurred in the scene when they had been suggested by a confederate ( $M = .60$  for 33% incorrect;  $M = .59$  for 66% incorrect;  $M = .61$  for 100% incorrect) than they were to attribute them to the scene when not suggested by a confederate ( $M = .43$  for 0% incorrect), although this difference was statistically nonsignificant  $F(3,68) = 1.948$ ,  $MSE = 0.07$ ,  $p = .13$ . However, given our a priori assumptions, follow up planned contrasts were ran which revealed that participants who heard no contagion items (0% incorrect) were significantly less likely to attribute those items to having occurred in the scenes than were participants who heard the confederate suggest these items during recall ( $t(68) = 2.405$ ,  $SEM = .216$ ,  $p = .019$ ). This finding also coincides with the original Meade & Roediger (2002) false recognition data.

### Veridical Source Judgments

Accurate source judgments for correct items (or items that actually occurred in the scene) on the recognition/source-monitoring test were again defined as the proportion of correct items that participants recognized as having occurred in the scenes ('scene') plus the proportion of correct items participants judged as having occurred in the scenes and as having been suggested by the confederate ('scene and other'). Using the same criteria

to determine recognition accuracy as before, this time participants were correct when judging items as coming from the scene or the scene and other person. These data are presented in the second column of Table 2. Participants in all four conditions were equally likely to correctly attribute veridical items to the scenes,  $F(3,68) = .999$ ,  $MSE = 0.028$   $p > .05$ , correctly recognizing a mean proportion of .63 items across conditions. These data confirm that on the final recognition test participants are generally able to appropriately recognize correct items as having occurred in the scene, regardless of condition, suggesting memory ability was equated across conditions.

Table 2. Mean proportion of false source judgments for critical contagion items and veridical source judgments for correct items as a function of proportion of incorrect items suggested by the confederate. Standard deviations are reported in parentheses.

Proportion of Incorrect Items	False	Correct
	Recognition	Recognition
0%	.43 (.27)	.66 (.22)
33%	.60 (.28)	.65 (.15)
66%	.59 (.25)	.63 (.14)
100%	.56 (.25)	.57 (.13)

### Final Questionnaires

We ran an additional analysis on our final questionnaires to determine whether proportion of incorrect information influenced participants' willingness to work with their partner (the confederate) again, or their subjective ratings of their partner's credibility and memorial abilities. These data are reported in Table 3 as a function of

proportion of incorrect items suggested by the confederate for all three questions. Interestingly, no significant effects of condition emerged for any of our three final questions ( $F$ 's < 1.12,  $p$ 's > .05). There are two possible explanations for these null results. First, it is likely that participants never caught on to discrepancies in their partner's recall, even as the proportion of false items dramatically increased across conditions. This explanation is supported by the equal contagion effects obtained across all conditions when excluding the control condition. Simply put, item-based credibility may not have been a strong enough manipulation to warrant discounting a same age peer. A second plausible explanation could be that due to experimenter error, the Likert scales used in experiment 1 were reverse ordered (1= best memory/most credible, while 5 = worst memory/least credible), which some participants reported confusing.

Table 3. Mean proportion of participants who would choose to work with their partner again (if monetary reward was contingent on accuracy), mean ratings of partner memory, and mean ratings of partner credibility as a function of proportion of incorrect information suggested by the confederate. Standard deviations are reported in parentheses. Note: Likert scales for Partner Memory and Credibility were reverse ordered (1=best memory/most credible; 5=worst memory/least credible).

Proportion of Incorrect Items			
	Choose Again	Partner Memory	Partner Credibility
0%	.94 (.24)	2.3 (1.1)	2.3 (1.1)
33%	.89 (.32)	2.5 (1.2)	2.3 (1.1)
66%	.94 (.24)	2.7 (1.1)	2.1 (.83)
100%	.77 (.43)	2.4 (.97)	2.5 (1.2)

## Experiment 2

In Experiment 1 we expected the proportion of false information participants adopted would be inversely related to the proportion of false information presented. However, we did not obtain a significant decrease in the contagion effect when participants encountered higher proportions of false information as predicted. It seems item-based credibility alone may not be enough to alert participants to item discrepancies in the social contagion paradigm; a finding of particular interest given the number of false memory studies reporting a decrease in effects when item discrepancies are more salient. Initially we believed that dramatically increasing the proportion of false items offered by a confederate would suffice as item discrepancy, and would ultimately alert participants to pay attention to the inaccuracies embedded in their partner's recall. However, a failure to obtain significant effects across conditions urged us to consider whether another persons' actual presence overrides the ability to detect discrepant items typically seen in other false memory paradigms where the biasing source is not present or is assumed to be the experimenter or researcher. This consideration is backed by several studies demonstrating the importance of encountering misinformation socially as opposed to embedded in the context of a narrative, or from an "implied other" (see Gabbert, Memon, Allan & Wright, 2004; Wright et al, 2000; Meade & Roediger, 2002; Reysen, 2005; Allan & Gabbert, 2008). Perhaps participants' perception that the confederate is similar to themselves may result in reduced item specific processing. Experiment 2 extends the modified social contagion paradigm developed in Experiment 1, to examine whether perceptions of the social source of misinformation (person-based credibility) interacts

with proportion of false items (item-based credibility) to influence the contagion effect. In other words, will participants still be likely to adopt items in the 100 percent false condition when they are explicitly aware that the confederate has a “poor” memory relative to when they are aware that the confederate has a “good memory”? More generally, what role does our perception of the source of misinformation play in the adoption of false memories, and how does this perception of person-based credibility interact with item-based credibility?

In their 1951 paper Hovland and Weiss demonstrated that participants (at the time of exposure) discounted materials they believed were coming from an “untrustworthy” or low credibility source. Though the authors in this case were more interested in how perceptions of merit (credibility) influenced the effectiveness of communication, their study paved the way for a more relevant area of research that examined source credibility, warnings and the misinformation effect (see Dodd & Bradshaw, 1980; Underwood & Pezdek, 1998; Chambers & Zaragoza, 2001; Echterhoff, Higgins, & Groll, 2005; Echterhoff, Groll, & Hirst, 2007; Highhouse & Bottrill, 1995). One such study by Smith and Ellsworth (1987) examined the effects of questioner expertise on the error rates of participants who were asked misleading or unbiased questions. The authors concluded that participants who believed information was coming from a more credible (knowledgeable) source showed significantly higher error rates when given misleading questions than those who believed information was coming from a less credible source. Another study by Hoffman, Granhag, Kwong See, & Loftus (2001) found that social conformity could be reduced by undermining a confederate’s credibility. When the

confederate was assumed to be a 'graduate student from the University of Washington' participants showed more conformity for errant items than did participants who believed misinformation items were being randomly generated by a computer.

Other literature on source monitoring has distinguished between social warnings that characterize an individual as untrustworthy and social warnings that characterize the individual as incompetent. When communicators intentionally render a false version of the original event they are seen as untrustworthy. When communicators lack sufficient knowledge or competence to provide a correct description they are seen as incompetent (Dodd & Bradshaw, 1980; Smith & Ellsworth, 1987). An interesting paper by Echterhoff, Hirst, & Hussy (2005) looked at social warnings which characterized the source of incoming information as a low-credibility source that was either untrustworthy or incompetent. The authors found diminished misinformation effects for both cued recall and recognition tests when source credibility was low, suggesting that framing the source as untrustworthy or incompetent was an effective means by which to reduce false memories. Further, they found that social postwarnings yielded the same results as an explicit source-monitoring appeal and led to longer response times for postevent items. Taken together, their findings suggest that the reduced misinformation effect was due to more thorough monitoring of memory characteristics by warned participants, rather than to a stricter response criterion or to an enhanced event memory.

The question of paramount concern for the purpose of this study was whether manipulating perceptions of person-based credibility—via perceived memorial ability—would serve as a sort of pretest warning urging participants to more closely monitor the

output of an ostensible partner. Literature suggests that individuals are less likely to pick up on discrepant items coming from a highly credible source, and conversely are more likely to detect these items when closely scrutinizing the output of a less credible source. The source monitoring framework developed by Johnson and colleagues (1993) suggests that information about a source is stored in memory and can later serve as a reminder to where the memory originated. Further, according to the source monitoring framework, heightened attention to the source of information should lead to greater processing of the information that source is producing. More processing during encoding should lead to greater discriminability for discrepancies between items that appeared in the original learning event and postevent misinformation. Thus, if the source of information is presented as less-credible— or in this case, as having a very poor memory—and participants can explicitly remember a certain item came from this source, logic suggests that the item should be discounted rather than reported on a later memory test. In a related study examining effects of overcorrection for misinformation, Echterhoff et al. (2007) concluded that warning participants about a potentially misleading source induced them to attend more carefully to source cues on a memory test. This heightened attention to the source of incoming information then allowed participants to detect both correct and incorrect information coming from that source on later memory tests. Though the authors here were more interested in the reduction of veridical recall in the face of explicit source warnings, their conclusion that the “tainted truth” effect was due to participants’ more careful monitoring of information from a suspicious source rather than mere memory impairment lends credence to the rationale used to design Experiment 2.

In Experiment 2 we hypothesized that leading participants to believe they were working with a partner that had a very poor memory (low-credibility source) should direct them to more closely monitor the output of that individual. The opposite was predicted for participants who were led to believe they were remembering with a partner who had a very good memory (high-credibility source). In this case, trusting the memorial ability of one's partner should result in a greater degree of discrepant information going undetected. We were particularly interested in how this new manipulation of partner credibility would interact with the same manipulation of item-based credibility used in Experiment 1. Although no significant differences in false memory emerged in the presence of increasingly higher proportions of false items in Experiment 1, we believed including a manipulation of source credibility would increase the likelihood of finding such effects. Specifically we theorized that participants who believed they were recalling with a partner that had a very good memory (higher credibility) would recall more false items in the 33% and 100% incorrect conditions compared to participants in who encountered the same proportions of false items in the absence of any source information (Experiment 1). In contrast, for participants who believed they were recalling with a partner who had a very poor memory (lower credibility), we predicted that they should reduce social contagion effects in the 100% inaccurate condition relative to the 33% inaccurate condition, given the source monitoring framework and logic outlined above.

## Method

### Participants

Our participants were 115 Montana State University Psychology 100 undergraduates who participated for partial course credit. Seven participants were not included in the final analysis again due to suspicion about the confederate being involved in the experiment, an inability to speak or understand English fluently, or a failure to understand/follow instructions. As a result, our final analysis included 108 participants, with 18 in each cell.

### Design

This experiment consisted of a 2 x 3 x 2 mixed design. Expectancy of the contagion items within each scene (high-expectancy or low-expectancy) was manipulated within participants, and the proportion of false information offered for each scene (0%, 33% or 100%) as well as perceptions of partner credibility (very good memory or very poor memory) was manipulated between participants. The primary dependent variables were again false recall and false recognition of the critical suggested items.

### Materials

The same materials used in Experiment 1 were also used in Experiment 2 with a few exceptions. An additional feux “pilot” study presented to participants at the beginning of each experiment which contained our manipulation of partner credibility required new materials. Examples of the slides containing instructions and the word lists, the recall sheet participants used to rate their ‘partner’s’ memory, as well as the

confederate scripts, can be found in Appendices C, B, D, E and F. The final recognition task was also shortened to replicate the source-monitoring task used by Meade and Roediger (2002), taking the total number of items from 96 to 36. Half of the items were previously studied items (three items from each of the six scenes), 12 of the items were potentially misleading (the high- and low-expectancy items from each scene), and the remaining items were six concrete nouns that did not appear in any of the scenes. Finally, we reversed the order of the Likert scales used in our final manipulation check as some participants in Experiment 1 reported the direction of the scale was counterintuitive (i.e. 1 previously indicated the highest score possible, rather than the lowest).

### Procedure

Participants again arrived to the lab with a same-age confederate, and were situated at their computer terminals in an identical fashion as Experiment 1. Before participants viewed the common household scenes they were told they would they would be taking part in a brief (and unrelated) pilot task. This feux pilot task contained our critical manipulation of partner credibility. Both participants and confederates viewed a slide informing them they would study identical word lists on the computer for 60 seconds under the pretense that one of them would be randomly selected to recall the list aloud (and in order) and that the other would be randomly selected to record their responses. Participants and confederates then viewed a 15-item categorized word list (developed from Battig & Montague, 1969) for 60 seconds before their computers automatically advanced to the next slide informing them of the role they would play in the upcoming recall task. In truth, participants were always selected to be the “tester” or

person recording the other's responses, and confederates were always selected to be the "tested" or the person recalling the list aloud. Next, the experimenter handed the participant a recall sheet containing the 15-item word list just studied, with instructions to mark with an "X" the words the confederate recalled only if they were in the correct order. The recall sheet also contained instructions that directed the participant to total the correct number of responses, and for additional salience, to conclude by circling a category that ostensibly corresponded to the total number of words recalled. In the high credibility conditions the confederate recalled 13 of the 15 words correctly, and in the appropriate order, using a script displayed on their computer screen. Participants totaled these responses and then circled the corresponding "very good memory" category on their paper before turning it over to be collected by the experimenter. In the low credibility conditions the confederate recalled only three of the 15 words correctly, appeared to struggle and inserted one intrusion ("llama") using a script displayed on their computer screen. Participants totaled these responses and this time circled the corresponding "very poor memory" category on their paper before turning it over to be collected by the experimenter.

Upon completion of the partner credibility manipulation (fake "pilot test"), the remainder of the procedure was identical to that of Experiment 1 with one exception: The 66% incorrect condition was dropped as it was numerically equated with the 33% incorrect condition in our first experiment.

## RESULTS

False RecallContagion Effects

The results of Experiment 2 replicated those of Experiment 1 such that significant levels of social contagion were obtained and the effect was again driven by higher-expectancy rather than lower-expectancy items. False recall results for critical contagion items are shown in Table 4 separately for participants who heard 0%, 33% and 100% false items, as well as for high- and low-expectancy items as a function of partner credibility. The top three rows in the table show the mean proportion of individual recall for the contagion items, the next three rows illustrate remember judgments for critical contagion items, and the final three rows report know judgments for critical contagion items. Additionally, the first two columns depict mean responses for participants who believed they were recalling with a high credibility partner whereas columns three and four show data for participants who believed they were recalling with a low credibility partner.

A 3 (proportion of incorrect items: 0%, 33%, 100%)  $\times$  2 (partner credibility: high vs. low)  $\times$  2 (item expectancy: high vs. low) mixed factorial ANOVA, with item expectancy as a within-participants variable, was conducted on the mean proportion of critical contagion items recalled. Critical contagion items were defined the same way as in Experiment 1, and were once again tracked across all conditions (regardless of proportion) to obtain our contagion effect. A main effect of proportion of incorrect items

was found  $F(2, 102) = 25.24$ ,  $MSE = 0.038$ ,  $p < .001$ . As was the case in Experiment 1, follow up tests confirmed the main effect of proportion was predicated by the difference between the control condition and the other two conditions. So, collapsed across partner memory and item expectancy, participants in the 33% incorrect condition showed greater contagion ( $M = .30$ ) than participants in the control (0% incorrect) condition ( $M = .11$ ;  $t(70) = -7.23$ ,  $SEM = .028$ ,  $p < .001$ ). As did participants in the 100% incorrect condition ( $M = .34$ ;  $t(70) = -7.11$ ,  $SEM = .032$ ,  $p < .001$ ). However the 33 percent and 100 percent incorrect conditions did not differ from one another ( $t(70) = -.666$ ,  $SEM = .038$ ,  $p > .05$ ).

In short, these data uncover a significant contagion effect replicating Roediger et al. (2001) and Meade and Roediger (2002); while increasing the proportion of false items did not appear to moderate this effect. There was also a significant main effect of item expectancy,  $F(1,102) = 137.76$ ,  $MSE = 0.018$ ,  $p < .001$ , meaning participants were also more likely to recall higher expectancy (more schematically consistent) contagion items ( $M = .35$ ) than lower expectancy (less schematically consistent) contagion items ( $M = .14$ ). As discussed earlier, this finding is typical in the social contagion literature and also replicates expectancy data from Experiment 1. The main effect of partner credibility was not significant,  $F(1,102) = .773$ ,  $MSE = 0.038$ ,  $p > .05$ , meaning participants were as likely to adopt false information from a partner they believed had a very poor memory as they were to adopt false information from a partner they thought had a very good memory. No interactions were significant (all  $F$ 's  $\leq 1.84$ ,  $p$ 's  $> .05$ ), suggesting that increasing the proportion of false items from 33 to 100 percent had no real effect on participants' recall of the critical items, regardless of partner credibility.

Table 4. Mean proportion of false recall, and remember or know responses for high- and low-expectancy items as a function of proportion of incorrect items suggested by the confederate and perceived partner credibility. Standard deviations are reported in parentheses.

Proportion Incorrect Items	High Partner Credibility			Low Partner Credibility		
	High Exp Items	Low Exp Items	Overall Contagion Effect	High Exp Items	Low Exp Items	Overall Contagion Effect
<b>Recall</b>						
0%	.19 (.17)	.03 (.06)	.11	.20 (.20)	.03 (.06)	.12
33%	.41 (.14)	.19 (.15)	.30	.42 (.19)	.17 (.17)	.30
100%	.47 (.26)	.24 (.22)	.34	.42 (.17)	.16 (.18)	.29
<b>Remember</b>						
0%	.07 (.16)	.00 (.00)	.04	.02 (.05)	.01 (.04)	.02
33%	.12 (.11)	.05 (.10)	.09	.13 (.18)	.05 (.13)	.09
100%	.15 (.18)	.05 (.10)	.10	.13 (.16)	.06 (.11)	.10
<b>Know</b>						
0%	.13 (.13)	.03 (.07)	.10	.19 (.15)	.19 (.05)	.19
33%	.38 (.16)	.15 (.14)	.27	.29 (.19)	.12 (.15)	.21
100%	.32 (.20)	.18 (.21)	.30	.37 (.21)	.10 (.12)	.24

### Remember Judgments

In terms of remember responses, a 3 (proportion of incorrect items: 0%, 33%, 100%)  $\times$  2 (partner credibility: high vs. low)  $\times$  2 (item expectancy: high vs. low) mixed factorial ANOVA again confirmed a main effect of proportion of incorrect information ( $F(2, 102) = 6.59, MSE = 0.017, p = .002$ ) and item expectancy ( $F(1,102) = 20.74 MSE$

= 0.013,  $p < .001$ ), but no main effect of partner credibility ( $F(1,102) = .205$ ,  $MSE = 0.017$ ,  $p > .05$ ). Once more, no interactions were significant (all  $F$ 's  $\leq 1.20$ ,  $p$ 's  $> .05$ ). Follow up tests confirmed all conditions differed significantly from the control condition ( $t(70) = -3.02$ ,  $SEM = 0.021$ ,  $p = .004$  for the 0% and 33% incorrect conditions;  $t(70) = -3.78$ ,  $SEM = 0.020$ ,  $p < .001$  for the 33% and 100% incorrect conditions), but again the remaining conditions did not differ from one another in terms of remember responses ( $t(70) = -.196$ ,  $SEM = 0.083$ ,  $p > .05$  for 33% and 100%). Participants were more willing to give remember responses to items suggested by the confederate than to items not suggested by the confederate, and to high-expectancy items over low-expectancy items, but manipulating the perceived memorial ability of the confederate did not moderate this effect. These data are shown in the second three rows of Table 4.

### Know Judgments

Know responses were analyzed across conditions in the same manner. Similar to the analysis of remember responses, know responses also revealed a main effect of proportion ( $F(2, 102) = 16.85$ ,  $MSE = 0.030$ ,  $p < .001$ ), and item expectancy ( $F(1,102) = 96.06$ ,  $MSE = 0.018$ ,  $p < .001$ ), no main effect of partner credibility ( $F(1,102) = .418$ ,  $MSE = 0.030$ ,  $p > .05$ ), with no interactions emerging between main effects (all  $F$ 's  $\leq 2.45$ ,  $p$ 's  $> .05$ ). Follow up tests for know responses once again showed the same pattern obtained for false recall and remember responses. The control condition differed significantly from the 33% incorrect condition ( $t(70) = -5.55$ ,  $SEM = 0.026$ ,  $p < .001$ ), and the 100% incorrect condition, ( $t(70) = -5.39$ ,  $SEM = 0.028$ ,  $p < .001$ ). However the 33% and 100% incorrect conditions did not differ significantly from one another,  $t(70) = -.222$ ,  $SEM =$

0.033,  $p > .05$ . As with remember responses, participants gave more know responses to items suggested by the confederate than to items not suggested by the confederate, and to high-expectancy items over low-expectancy items, regardless of the perceived memorial ability of the confederate. These data are shown in the last three rows of Table 4.

## Recognition

### False and Veridical Source Judgments

The mean proportions of participants' responses on the final recognition/source-monitoring tests are displayed in Table 5 for both false and veridical source judgments across the varying proportions of misinformation and high or low perceptions of partner credibility.

For Experiment 2 we chose to use the original 36-item recognition source-monitoring task developed by Meade and Roediger (2002), rather than the modified recognition task used in Experiment 1. This allowed us to determine whether we could obtain the same effects as the original authors when using their materials. A 3 (proportion incorrect information: 0%, 33%, 100%)  $\times$  2 (partner credibility: high vs. low) between participants ANOVA revealed a significant main effect of proportion ( $F(2, 102) = 4.95$ ,  $MSE = .100$ ,  $p = .009$ ), with no main effect of partner credibility ( $F(1, 102) = .541$ ,  $MSE = .100$ ,  $p > .05$ ), and no interaction between the main effects ( $F(2, 102) = 1.63$ ,  $MSE = .100$ ,  $p > .05$ ) emerging. These results are shown in the first two columns of Table 5. Follow up tests verified both contagion conditions differed significantly from the control condition ( $t(70) = -2.85$ ,  $SEM = 0.068$ ,  $p = .006$  for 0% and 33% incorrect conditions;

$t(70) = -2.90$ ,  $SEM = 0.073$ ,  $p = .005$  for 0% and 100% incorrect conditions), but that they did not differ from one another,  $t(70) = -.196$ ,  $SEM = 0.083$ ,  $p > .05$ ). Also, it appears that manipulations of perceived partner credibility did not have an effect on erroneous source judgments for contagion items. Further, we believe our ability to replicate Meade and Roediger when using their original source-monitoring materials supports our earlier assumptions that lack of replication in Experiment 1 on this task was most likely due to the extension and modification of their original test rather than the absence of the effect.

Accurate source judgment for veridical items was again equated across conditions, with no main effects or interactions emerging (all  $F$ 's  $\leq 1.80$ ,  $p$ 's  $> .05$ ). Once again data confirmed that on the final recognition test, participants were generally able to recognize correct items as having occurred in the scene regardless of condition or partner credibility. This suggests memorial ability in general was similar across conditions; with no one group outperforming another. These data replicate Experiment 1 and are shown in the second two columns of Table 5.

Table 5. Mean proportion of false source judgments for critical contagion items and veridical source judgments for correct items as a function of proportion of incorrect items suggested by the confederate and perceived partner credibility. Standard deviations are shown in parentheses.

Proportion of Incorrect Items	Total False Recognition		Total Correct Recognition	
	High Partner Cred	Low Partner Cred	High Partner Cred	Low Partner Cred
0%	.33 (.29)	.21 (.18)	.63 (.26)	.64 (.26)
33%	.54 (.32)	.50 (.35)	.65 (.14)	.66 (.20)
100%	.44 (.34)	.52 (.37)	.56 (.21)	.57 (.16)

### Final Questionnaires

An interesting pattern of results emerged when we ran analyses on our final questionnaire/manipulation check for Experiment 2. We used the same questions as in Experiment 1 (save for reversing the order of our Likert scales) to determine whether our manipulations of item or partner credibility had any effect on participants' subjective judgments of their partner's accuracy or their willingness to work with their partner again if money was contingent on accuracy. A 3 (proportion of incorrect items: 0%, 33% 100%)  $\times$  2 (partner credibility: high vs. low) between participants ANOVA revealed no main effect of proportion, or significant interactions for any of these questions (all  $F$ 's  $\leq$  1.67,  $p$ 's  $>$  .05). However, these data did show a significant main effect of partner credibility for all three of the final questions ( $F(1, 102) = 15.7$ ,  $MSE = .170$ ,  $p < .001$  for choose again,  $F(1, 102) = 11.30$ ,  $MSE = .379$ ,  $p = .001$  for partner credibility, and  $F(1, 102) = 15.96$ ,  $MSE = .471$ ,  $p < .001$  for partner memory). These data are strong support that our manipulation of partner credibility in Experiment 2 was successful as evidenced by less willingness to work with, and lower scores given to, partners that participants were led to believe had a 'very poor memory'. Yet despite this awareness of reduced memorial abilities, it appears participants were nonetheless unable to avoid adopting errant information from partners who were viewed as less credible. Mean proportion of participants' willingness to work with their partner again, as well as subjective ratings of memory abilities and partner credibility, are shown in Table 6.

Table 6. Mean proportion of participants who would choose to work with their partner again (if monetary consumption was contingent on accuracy), mean ratings of partner memory, and mean ratings of partner credibility as a function of proportion of incorrect information suggested by the confederate and perceived credibility (memorial ability) of the confederate. Standard deviations are shown in parentheses.

---

Proportion of Incorrect Items	High Partner Credibility			Low Partner Credibility		
	Choose Again	Partner Credibility	Partner Memory	Choose Again	Partner Credibility	Partner Memory
0%	.94 (.24)	4.1 (.73)	4.2 (.65)	.56 (.51)	3.7 (.46)	3.7 (.57)
33%	.94 (.24)	4.1 (.58)	4.3 (.67)	.61 (.50)	3.4 (.62)	3.4 (.70)
100%	.83 (.38)	3.7 (.75)	3.9 (.96)	.61 (.50)	3.5 (.50)	3.6 (.49)

---

## GENERAL DISCUSSION

The two experiments reported here replicated previous social contagion research (Roediger et al., 2001; Meade & Roediger, 2002) by demonstrating that participants incorporate erroneous confederate suggestions into their individual memory reports, and that this is especially true for high expectancy items. In addition, the current experiments demonstrated several new and important findings regarding the influence of item-based credibility and person-based credibility on the social contagion effect. Experiment 1 included a novel item-based credibility manipulation that extended the proportion of false information participants encountered to the most extreme end of the spectrum; literally providing *only* incorrect information for the *duration* of the experiment in one condition. Much to our surprise, even this 100 percent incorrect condition did not encumber social contagion to any significant degree. Instead, we found participants were as willing to adopt false information from a partner who was wrong 100 percent of the time as they were to adopt these false items from a partner who was only wrong 33 percent of the time. The same was true for a 66 percent incorrect condition. As such, we concluded that merely increasing the proportion of false information presented for each scene was not enough to overcome false memories in this paradigm. Moreover, it appears this manipulation was also not enough to urge participants to discredit the confederate on the final manipulation check questionnaire. Participants were as willing to work with the confederate again (if remuneration was dependent on partner accuracy), and gave similarly high ratings of perceived memorial abilities and credibility, regardless of how much false information they encountered.

Experiment 2 replicated the null effect of item-based credibility on the social contagion effect, and further demonstrated that person-based credibility did not influence the magnitude of the effect. Participants were as likely to adopt false items from a low-credibility source, who was subsequently wrong 100 percent of the time, as they were to adopt false items from a highly credible source who was wrong just 33 percent of the time. Given evidence that warnings about the credibility of the source of misinformation can reduce memory and suggestibility errors (Smith & Ellsworth, 1987), it is striking that the present results do not support our initial hypothesis that a source credibility ‘warning’ would interact with an item-based credibility ‘warning’ to reduce the contagion effect. Importantly, data from the final questionnaires suggest that participants were aware of partner credibility as they rated low credibility confederates as having poorer memories, and were less likely to say they would work with them again. However, it appears that participants subjectively discount partners they are told have poor memories while simultaneously adopting significant amounts of false information from them. These results are rather provocative, and of course bear replication by future experiments.

#### Relation to Other Paradigms

Given that the social contagion paradigm shares theoretical roots with other false memory, misinformation and memory conformity paradigms, we designed a set of experiments to see if the same variables that can reduce erroneous adoption of false items in these other paradigms would generalize to our study. As we did not achieve similar reductions in false memories, it is important to consider the subtle differences between

our paradigm and the others mentioned above. The present study differed from previous warning studies in a number of ways, any of which may have reduced the likelihood of obtaining a person-based credibility warning effect. Most obviously, past research that typically yields successful reductions in the recall or recognition of errant items has done so only after an explicit warning about the effect was given to participants. For example, successful warnings in other paradigms like the DRM (Neuschatz et al., 2002; Watson et al., 2004) and recall to reject (Gallo, 2004), often begin by outlining in detail the specific nature of the false memory effect being studied to participants. Even in the original Meade and Roediger (2002) study, the authors openly warned participants that confederates' responses could influence their own to reduce the contagion effect. In their review, Chambers and Zaragoza (2001) outlined several examples of the explicit nature of successful item warnings (p. 1120). One such example came from Lindsay (1990), who had to tell participants that "there are no questions on this test for which the correct answer was mentioned in the story". Another example was taken from Wright (1993), who was even more extreme telling participants "In the slide sequence the woman did not have any cereal with her breakfast". By comparison our item based warning required significantly more attention and deductive reasoning on the part of our participants than the other studies mentioned. Put another way, the warning used in our study required participants to infer that the confederate was suggesting erroneous items 100 percent of the time, and as such, they should discredit the items produced by that person.

Second, the partner credibility manipulation employed in this study could be considered less vivid, emotionally interesting, and self-relevant to the participants than

manipulations used in studies we drew from. For example, Underwood and Pezdek (1998) told participants that misleading information came from either a fourth grade boy or a memory professor, Hoffman et al (2001) told their participants information came from a graduate student or was randomly generated by a computer, Dodd and Bradshaw (1980) told participants their information came from a person who was intentionally lying to them, and finally Echterhoff et al. (2005), told participants information came from either a high school student who underperformed on an aptitude test (incompetent) or a witness motivated to lie during litigation (untrustworthy). The potentially less vivid warning used here (i.e. confederates' perceived memory ability) may have decreased the likelihood that participants would synthesize information we provided about their partner when conjuring items for a later recall or source judgment task. Future research could examine this hypothesis by including a more explicit source warning in conjuncture with the new item-based credibility manipulation we developed.

It is important to note however, that the person-based credibility manipulation used in the present study could also be considered more relevant to the task at hand. In contrast to hearing a hypothetical, or more general, warning regarding a perceived partner's credibility as in previous studies, participants in the current study witnessed firsthand the confederate's memory ability. Further, they were required to actually score the confederate as they completed the related memory task before categorizing them as having either a "poor memory" or a "very good memory." That such a manipulation did not influence the magnitude of the social contagion effect is quite remarkable. We feel confident that the null result obtained in Experiment 2 regarding partner credibility

cannot be explained by some idiosyncratic parameter of the social contagion paradigm, as general, explicit warnings against confederate errors have been shown to reduce the effect (Meade & Roediger, 2002). Rather, because the manipulation was less explicit, participants in Experiment 2 were again meant to make inferences about the confederate's memory ability. Specifically, they were meant to infer that since the confederate had a poor memory for a related task, he/she might also have poor memory on the experimental task—and should be discredited as a result. Considered together, the data from the person-based credibility manipulation and the item-based credibility manipulation suggest that participants in the social contagion paradigm are unlikely to assume credibility based on item accuracy and memorial performance on a related task.

Of course the above interpretations are based on null effects and so should be interpreted with caution. Once again though, we are confident that these null effects of item and person-based credibility are not the result of failed manipulations. Regarding item-based credibility, the current study included a condition in which 100 percent of the items offered by the confederate were incorrect. As such, it was not possible to have included a stronger item based manipulation of credibility in this study as we operationally defined this term as the proportion of false items offered by the confederate. The person-based credibility manipulation was also theoretically strong: Participants watched the confederate perform a memory task similar to the experimental task, scored their output, and categorized the confederate as having either a “very good” or “poor” memory. Auxiliary evidence for this hypothesis comes from the emergence of a main effect of person-based credibility on the final manipulation check questionnaire, which

confirms that this manipulation was not weak in the sense that it went unnoticed by participants. Finally, the current studies obtained and replicated several significant effects regarding contagion and expectancy, suggesting our experimental procedures were indeed robust enough to obtain significant effects.

### Interpretation of Results

In addition to some of the methodological differences between our studies and others that have used more explicit item or source warnings, there are other plausible theoretical explanations for our results. Described below is an interpretation of our results in terms of Johnson et al.'s (1993) source-monitoring framework. The source monitoring framework suggests that items are encoded along with various characteristics that may be diagnostic of source. Source attributions result from participants comparing the characteristics associated with a given item to the characteristics of a perceived class of sources (e.g. if a given item has many perceptual details associated with it, one might attribute the item to having actually occurred, as real objects typically have many perceptual details). Participants establish differential decisional criteria to determine how much overlap is required between the characteristics of a given item and the perceived class of sources. As with previous social contagion research (see Meade & Roediger, 2002), we feel this perspective nicely distills many of our results, and in a manner that may help to direct future research. To begin, the present study showed that increasing the proportion of false information participants heard had no effect on the amount of false

items later produced or recognized on an individual memory task. This finding was counterintuitive and urged us to consider the nature of the false items we were using.

Regardless of expectancy (high vs. low), items were always schematically consistent, and plausible for each scene. Thus, participants may have refrained from attributing false items to the confederate on the basis that the items seemed familiar or plausible, or because they were able to call up a visual image of the errant item themselves. For example, both screws (high-expectancy item) and a ruler (low-expectancy item) might spring to mind when one conjures their mental ‘schema’ of a toolbox. According to the source-monitoring framework, self-generated or imagined items are believed to possess a great deal of cognitive operations and possibly perceptual details that are stored in memory during encoding. Thus, if the perceptual details are rich and vivid enough, then the event could be judged as having been externally experienced—or in the case of our experiment, as having been present in one of the original slides (see Johnson et al., 1993, and Bink, Marsh & Hicks, 1999, for discussion). Several other studies (see Johnson & Raye, 1998, and Roediger & McDermott, 1995) suggest that source attribution errors increase as a function of increased perceptual and semantic similarity between items. Further, our own item-expectancy data shows high-expectancy (more typical) items drive the contagion effect in both Experiments 1 and 2. These results are consistent with both Roediger et al. (2001) and Meade and Roediger’s (2002) studies. Future research should vary the high/low item expectancy ratio, or include items that are not schematically consistent, to elucidate this hypothesis.

Next we consider how varying perceptions of the social source of information might distort the source attribution process. Many studies have confirmed the extreme potency of misinformation encountered through interactions with another person/confederate who is physically present, compared to an implied source (e.g. Meade & Roediger, 2002; Gabbert et al, 2003 & 2004; Allan & Gabbert, 2008; Hoffman et al, 2001; Paterson & Kemp, 2006). Harris, Paterson and Kemp (2008) suggest the reason socially introduced misinformation is particularly persuasive is that people are accustomed to encountering the memories of others during conversations, and using them to help construct their own memories. Additionally, not only do individuals often remember in a group context, they often remember in the context of a group of similar peers. In our study, we had participants initially recall in the presence of a same-age, physically present, confederate; a person ostensibly very similar to themselves. In the context of our experiments, it appears that remembering an event with an individual who is quite similar to oneself reduces a motivation to closely monitor the output of that person. As a result, perhaps less attention is paid to the items that similar person is producing, and/or participants are setting a different decisional criterion for sources similar to themselves, both of which would reduce the ability to make accurate source judgments later on. Initially we believed ‘warning’ participants about (or at least offering insight into) their partner’s memory would make confederates in the low-credibility conditions appear more salient. This, we thought, would direct participants to monitor these confederates’ recall with increased effort or stringency compared to participants paired with confederates in the high-credibility conditions. However this was not the

case. Rather, it appears there is something quite powerful about remembering alongside someone who is perceptually very similar to oneself that overrides the subtle person-based credibility warning we developed. In a relevant study by Davis and Meade (in prep), the authors examined age differences in the social contagion paradigm by manipulating the age of both the participant and the confederate factorially. Notably, younger adult participants could successfully discredit false items from older adult confederates, but the same was not true when younger participants were paired with a confederate of the same age. Though not the only possible explanation for these results, it is plausible that younger adults had a much easier time discriminating between items being produced by an older compared to a younger (more similar) confederate.

In the context of the social contagion effect, the source-monitoring framework may be interpreted to suggest that people pay less attention to partners perceived to be very similar—perhaps because they see no reason to discount or scrutinize peers quite like themselves. Superficial attention to output during an initial recall task and/or altered decision criterion for sources similar to oneself, allows memories of (highly schematic) misleading items to pass as memories of a witnessed event on a later source judgment task. Again it bears mentioning that our manipulation of person-based credibility was quite subtle compared to other studies we looked at. Most notably, we did not alter the perceived similarity between the participant and the confederate (save for possible memorial abilities). Given the source-monitoring literature and logic outlined above, one might predict that only warnings which portray the confederate as being very different from the participant (i.e. as a fourth grader, a computer, or a high school student who just

failed an aptitude test) would be successful in reducing misinformation effects. In fact, this does appear to be the case. The findings of Experiment 2 are thus consistent with the hypothesis that participants are not strongly motivated to monitor the output of a similar peer compared to a partner perceived to be radically different. Further, according to the source monitoring framework, less attention to the source at time of encoding often leads to less accurate source judgments on later tests (see Johnson et al., 1993). As of yet, this hypothesis has not received the due attention and investigation that the present results perhaps justify and other research is necessary to confirm such assumptions.

### Practical Implications

Allan & Gabbert (2008) seem to foreshadow our current work, suggesting many past studies have considerably underestimated the level of distortion produced by social interaction. We believe results from this series of experiments echoes that sentiment. We were first surprised to find that providing participants with 100 percent false information did very little to deter adoption or reporting of false items. In addition, we found this manipulation also did not augment participants' accurate source judgments, nor did it influence subjective ratings of partner credibility. In Experiment 2 we showed that if participants' attention was *explicitly* directed to consider their partners' memorial abilities (i.e. on the final manipulation check questionnaires), they could rather easily use information provided to them earlier to appropriately discount confederates in the low-credibility conditions. However, during the collaborative phase of our experiment this information is lost and fails to alert participants to discrepancies between their own

memory of the scenes and the confederate's errant recall. In a sense, Experiment 2 provided participants with causally related pieces of a puzzle to see if they could draw conclusions about the memorial abilities of the person with whom they were about to be partnered. We were quite certain we would see an adjustment in the criteria participants used to monitor their partner's output as a function of credibility. This, of course, was not the case. In the end it seemed participants were capable of analyzing, but not synthesizing, the pieces of the "memory puzzle" we gave them.

In recent years, false memory paradigms have sought to investigate the effects of social influences using more ecologically valid designs. Still, how these person perception manipulations alter the memory influence exerted by one individual upon another is essentially unknown. Research thus far has been rather equivocal in regards to whether such person perception factors can exacerbate, reduce, or even eliminate prominent false memory effects.

We believe the warnings we used in our experiments were reminiscent of actual encounters with social misinformation, and thus, telling of the power of false memories. Our study not only uses a well established paradigm in the literature—one that is ecologically sound and that reliably produces misinformation effects—it also introduces two novel realistic manipulations of credibility. By manipulating the proportion of false information introduced by our confederate we showed that schematically consistent false information has virtually no limitations when it comes to inducing false memories. Second, by adding a subtle manipulation of person-based credibility, we were able to show that even when participants are aware that the person they are remembering with

has a '*poor memory*' this information is not enough to induce them to attend to that persons' output, even on a *memory* task. These findings imply a very unfortunate outcome in forensic settings in which witnesses often express inaccuracies while discussing a crime together. Not only are most errant details of a witnesses' testimony very schematically consistent (i.e. the color of the shoes the assailant was wearing), but witnesses are also often corroborating with other individuals who are very similar to themselves. Furthermore, it seems very unlikely that in the real world, one would ever be as unambiguously warned about the memorial abilities of individuals they are remembering with as most participants are in false memory studies.

In conclusion, the present study elucidated several interesting and unexpected findings in its search to uncover the parameters of the social contagion effect. In all, it seems that item and person-based credibility failed to influence the magnitude of social contagion because participants did not deduce or infer credibility based on content accuracy and memory performance on a related task. We believe these results are even more fascinating than our original predictions and show how susceptible our memories are to misinformation. The methods introduced here capture and extend the core phenomena of the social contagion effect, while simultaneously unveiling the robust nature of this paradigm. The present method, in our opinion, establishes a more ecologically valid tool for manipulating subtle social cognitive factors that might determine individuals' susceptibility or resistance to false memories. As we have not yet uncovered the limits of social contagion, we look forward to future research validating and extending the findings we present here.

REFERENCES CITED

- Allan, K., & Gabbert, F. (2008). I still think it was a banana: Memorable 'lies' and forgettable 'truths'. *Acta Psychologica*, *127*, 299-308.
- Asch, S.E. (1956). Studies of Independence and Conformity: A minority of one against a unanimous majority. *Psychological Monographs*, *70* (Whole No. 416).
- Battig, W. F., & Montague, W. E. (1969). Category norms for verbal items in 56 categories: A replication and extension of the Connecticut norms. *Journal of Experimental Psychology*, *80*, 1-46
- Bartlett, F.C. (1932). Experiments on Remembering. The method of repeated reproduction. In F.C. Bartlett, *Remembering: A Study in Experimental and Social Psychology*. New York, NY: Cambridge University Press.
- Basden, B.H., Basden, D.R., Bryner, S., & Thomas, R.L. (1997). A comparison of group and individual remembering: Does collaboration disrupt retrieval strategies? *Journal of Experimental Psychology: Learning, memory, and Cognition*, *16*, 225-246.
- Bink, M.L., Marsh, R.L., & Hicks, J.L. (1999). An alternative conceptualization to memory "strength" in reality monitoring. *Journal of Experimental Psychology: Learning, Memory, & Cognition*, *25*, 804-809.
- Chambers, K.L., & Zaragoza, M.S. (2001). Intended and unintended effects of explicit warnings on eyewitness suggestibility: Evidence from source identification tests. *Memory & Cognition*, *29*, 1120-1129.
- Craik, F.I.M., & Lockhart, R.S. (1972). Levels of processing: A framework for memory research. *Journal of Verbal Learning and Verbal Behavior*, *11*, 671-684.
- Deutsch, M, & Gerard, H.B. (1956). A study of normative and informational social influences upon individual judgment. *Journal of Abnormal and Social Psychology*, *51*, 629-636.
- Dodd, D.H., & Bradshaw, J.T. (1980). Leading questions and memory: Pragmatic constraints. *Journal of Verbal Learning and Verbal Behavior*, *19*, 695-704.
- Echterhoff, G., Groll, S., & Hirst, W. (2007). Tainted truth: Overcorrection for misinformation influence on eyewitness memory. *Social Cognition*, *25*, 367-409.
- Echterhoff, G., Hirst, W., & Hussy, W. (2005). How eyewitnesses resist misinformation: Social postwarnings and the monitoring of memory characteristics. *Memory & Cognition*, *33*, 770-782.

- Echterhoff, G., Higgins, E.T., & Groll, S. (2005). Audience-tuning effects on memory: The role of shared reality. *Journal of Personality and Social Psychology*, *89*, 257-276.
- Echterhoff, G. & Hirst, W. (2009). Social Influence on Memory. *Social Psychology*, *40*, 106-110.
- Gabbert, F., & Memon, A., & Allan, K. (2003). Memory conformity: Can eyewitnesses influence each other's memories for an event? *Applied Cognitive Psychology*, *17*, 533-543.
- Gabbert, F., Memon, A., Allan, K., & Wright, D.B. (2004). Say it to my face: Examining the effects of social encountered misinformation. *Legal and Criminological Psychology*, *9*, 215-227.
- Gabbert, F., & Memon, A., & Wright, D. B. (2006). Memory conformity: Disentangling the steps toward influence during a discussion. [Electronic version] *Psychonomic Bulletin & Review*, *13*(3), 480-485.
- Gallo, D.A. (2004). Using recall to reduce false recollection: Diagnostic and disqualifying monitoring. *Journal of Experimental Psychology: Learning, Memory and Cognition*, *30*, 120-128.
- Harris, C.B., Paterson, H.M., & Kemp, R.I. (2008). Collaborative recall and collective memory: What happens when we remember together? *Memory*, *16*, 213-230.
- Highhouse, S., & Bottrill, K.V. (1995). The influence of social (mis)information on memory for behavior in an employment interview. *Organizational Behavior & Human Decision Processes*, *62*, 220-229.
- Hoffman, H.G., Granhag, P., Kwong See, S.T., & Loftus, E.F. (2001). Social influences on reality monitoring decisions. *Memory & Cognition*, *29*, 394-404.
- Hovland, C.I., & Weiss, W. (1951). The influence of source credibility on communication effectiveness. *Public Opinion Quarterly*, *15*, 635-650.
- Hunt, R.R. (2003). Two contributions of distinctive processing to accurate memory. *Journal of Memory and Language*, *48*, 811-825.
- Johnson, M.K., Hastroudi, S., & Lindsay, D.S. (1993). Source monitoring. *Psychological Bulletin*, *114*, 3-28.
- Johnson, M.K., & Raye, C. (1998). False memories and confabulations. *Trends in Cognitive Sciences*, *2*, 137-145.

- Lindsay, D.S. (1990). Misleading suggestions can impair eyewitness' ability to remember event details. *Journal of Experimental Psychology: Learning, Memory, & Cognition*, *16*, 1077-1083.
- Loftus, E.F., Miller, D.G., & Burns, H.J. (1978). Semantic integration of verbal information into a visual memory. *Journal of Experimental Psychology: Human Learning and Memory*, *4*, 19-31.
- Loftus, E.F. (1979). The malleability of human memory. *American Scientists*, *67*, 312-320.
- Loftus, E.F., & Palmer, J.C. (1974). Reconstruction of automobile destruction: An example of the interaction between language and memory. *Journal of Verbal Learning & Verbal Behavior*, *13*, 585-589.
- McCabe, D.P., Presmanes, A.G., Robertson, C.L., & Smith, A.D. (2004). Item-specific processing reduces false memories. *Psychonomic Bulletin and Review*, *11*, 1074-1079.
- Meade, M.L., & Roediger, H.L., III. (2002). Explorations in the social contagion of memory. *Memory & Cognition*, *30*, 995-1009.
- Neuschatz, J.S., Benoit, G.E., & Payne, D.G. (2002). Effective warnings in the Deese-Roediger-McDermott false-memory paradigm: The role of identifiability. *Journal of Experimental Psychology: Learning, Memory and Cognition*, *29*, 35-41.
- Paterson, H.M., & Kemp, R.I. (2006). Comparing methods of encountering postevent information: The power of co-witness suggestions. *Applied Cognitive Psychology*, *20*, 1083-1099.
- Reysen, M. B. (2005). The effects of social pressure on group recall. *Memory & Cognition*, *31*, 1163-1168.
- Roediger, H.L., III, & McDermott, K.B. (1995). Creating false memories: Remembering words not presented in lists. *Journal of Experimental Psychology: Learning, Memory, Cognition*, *21*, 803-814.
- Roediger, H.L., III, & McDermott, K.B. (2000). Distortions of memory. In E. Tulving & F.I.M. Craik (Eds.), *The Oxford handbook of memory* (pp.149-162). Oxford: Oxford University Press.
- Roediger, H.L., III, Meade, M.L., & Bergman, E.T. (2001). The social contagion of memory. *Psychonomic Bulletin & Review*, *8*, 365-371.

- Smith, V.L., & Ellsworth, P.C. (1987). The social psychology of eyewitness accuracy: Leading questions and communicator expertise. *Journal of Applied Psychology*, 72, 292-300.
- Tousignant, J.P., Hall, D., & Loftus, E.F. (1986). Discrepancy detection and vulnerability to misleading poste-event information. *Memory & Cognition*, 14, 329-338.
- Tulving, E. (1983). *Elements of episodic memory*. London: Oxford University Press.
- Tulving, E. (1985). Memory and consciousness. *Canadian Psychologist*, 26, 1-12.
- Underwood, J., & Pezdek, K. (1998). Memory suggestibility as an example of the sleeper effect. *Psychonomic Bulletin & Review*, 5, 449-453.
- Watson, J.M., McDermott, K.B., & Balota, D.A. (2004). Attempting to avoid false memories in the Deese/Roediger-McDermott paradigm: Assessing the combined influence of practice and warnings in young and old adults. *Memory & Cognition*, 32, 135-141.
- Weldon, M.S., & Bellinger, K.D. (1997). Collective memory: Collaborative and individual processes in remembering. *Journal of Experimental Psychology: Learning, Memory and Cognition*, 23, 1160-1175.
- Wright, D.B. (1993). Misinformation and warnings in eyewitness testimony: A new testing procedure to differentiate explanations. *Memory*, 1, 153-166.
- Wright, D.B., Memon, A., Skagerberg, E.M., & Gabbert, F. (2009). When eyewitnesses talk. *Current Directions in Psychological Science*, 18, 174-178.
- Wright, D. B., & Self, G., & Justice, C. (2000). Memory conformity: Exploring misinformation effects when presented by another person. [Electronic version] *British Journal of Psychology*, 91(2), 189-202.

APPENDICES

APPENDIX A

CONFEDERATE SCRIPTS

Condition 1		
<u>Toolbox</u> Knife (metal supports) Bungee cord (tool box) Extension cord (table) Wrench (Hammer) Washers (Saw) Hair clip (Banana)	<u>Bathroom</u> Shampoo (Tissue Box) Toothpaste (sink) Razor (Mirror) Magazine (Cup) Deodorant (Medicine Cabinet) Shaving Gel (Scissors)	<u>Kitchen</u> Coffee maker (Wire whisk) Phone (Books) Cutting Board (Metal spoons) Magnets (Spices) Paper towel (stove) Tea Kettle (Spice rack)
<u>Bedroom</u> Candle (wine glass) Fan (video) Pillow (window) Bed (Picture) Vacuum (Comforter) Laundry bag (Dresser)	<u>Closet</u> Suitcase (Gloves) Tie (Hat) Flashlight (helmet) Cooler (Soup) Umbrella (Hangers) Frisbee (shirts)	<u>Desk</u> Plant (monitor) Tape dispenser (pen) Planner (dictionary) Filing cabinet (Chair) Keyboard (Lamp) Mouse Pad (Photos)

(0% false information).

Condition 2		
<u>Toolbox</u> Knife (metal supports) Bungee cord (tool box) Extension cord (table) <b>Screws (Pliers)</b> Washers (Saw) <b>Ruler (Pencil)</b>	<u>Bathroom</u> Shampoo (Tissue Box) Toothpaste (sink) Razor (Mirror) <b>Toothbrush (Shower)</b> Deodorant (Medicine Cabinet) <b>Hair Brush (Contact Solution)</b>	<u>Kitchen</u> Coffee maker (Wire whisk) Phone (Books) Cutting Board (Metal spoons) <b>Toaster (Cups)</b> Paper towel (stove) <b>Oven Mitts (Napkins)</b>
<u>Bedroom</u> Candle (wine glass) Fan (video) Pillow (window) <b>Night Stand (Mirror)</b> Vacuum (Comforter) <b>Cologne (Quilt)</b>	<u>Closet</u> Suitcase (Gloves) Tie (Hat) Flashlight (helmet) <b>Shoes (Boxes)</b> Umbrella (Hangers) <b>Ball (Gym Shorts)</b>	<u>Desk</u> Plant (monitor) Tape dispenser (pen) Planner (dictionary) <b>Printer (Paper Clips)</b> Keyboard (Lamp) <b>Rolodex (Radio)</b>

(33% false information). Note: bolded words denote false items.

<b>Condition 3</b>		
<u>Toolbox</u> Knife (metal supports) <b>Chisel (Duct Tape)</b> Extension cord (table) <b>Screws (Pliers)</b> <b>Tape Measure (Bolts)</b> <b>Ruler (Pencil)</b>	<u>Bathroom</u> Shampoo (Tissue Box) <b>Hairspray (Dental floss)</b> Razor (Mirror) <b>Toothbrush (Shower)</b> <b>Soap (Bath rug)</b> <b>Hair Brush (Contact Solution)</b>	<u>Kitchen</u> Coffee maker (Wire whisk) <b>Blender (Spatula)</b> Cutting Board (Metal spoons) <b>Toaster (Cups)</b> <b>Knives (Pots)</b> <b>Oven Mitts (Napkins)</b>
<u>Bedroom</u> Candle (wine glass) <b>Water glass (Rug)</b> Pillow (window) <b>Night Stand (Mirror)</b> <b>TV (Lamp)</b> <b>Cologne (Quilt)</b>	<u>Closet</u> Suitcase (Gloves) <b>Slippers (Boots)</b> Flashlight (helmet) <b>Shoes (Boxes)</b> <b>Belt (Bags)</b> <b>Ball (Gym Shorts)</b>	<u>Desk</u> Plant (monitor) <b>Briefcase (Glue)</b> Planner (dictionary) <b>Printer (Paper Clips)</b> <b>Calendar (Phone)</b> <b>Rolodex (Radio)</b>

(66% false information). Note: bolded words denote false items.

<b>Condition 4</b>		
<u>Toolbox</u> <b>Bolts (Electric Tape)</b> <b>Chisel (Duct Tape)</b> <b>Band-aid (Coins)</b> <b>Screws (Pliers)</b> <b>Tape Measure (Bolts)</b> <b>Ruler (Pencil)</b>	<u>Bathroom</u> <b>Mouthwash (Bathtub)</b> <b>Hairspray (Dental floss)</b> <b>Nail clippers (Plunger)</b> <b>Toothbrush (Shower)</b> <b>Soap (Bath rug)</b> <b>Hair Brush (Contact Solution)</b>	<u>Kitchen</u> <b>Dishwasher (Forks)</b> <b>Blender (Spatula)</b> <b>Measuring Spoons (Sugar)</b> <b>Toaster (Cups)</b> <b>Knives (Pots)</b> <b>Oven Mitts (Napkins)</b>
<u>Bedroom</u> <b>Alarm Clock (Books)</b> <b>Water glass (Rug)</b> <b>Reading Glasses (Backpack)</b> <b>Night Stand (Mirror)</b> <b>TV (Lamp)</b> <b>Cologne (Quilt)</b>	<u>Closet</u> <b>Coats (Socks)</b> <b>Slippers (Boots)</b> <b>Suit (Lotion)</b> <b>Shoes (Boxes)</b> <b>Belt (Bags)</b> <b>Ball (Gym Shorts)</b>	<u>Desk</u> <b>Stapler (Books)</b> <b>Briefcase (Glue)</b> <b>Money (Candy)</b> <b>Printer (Paper Clips)</b> <b>Calendar (Phone)</b> <b>Rolodex (Radio)</b>

(100% false information). Note: bolded words denote false items.

APPENDIX B

MANIPULATION CHECK/FINAL QUESTIONNAIRE

Current literature suggest that people may work together more cohesively as partners on memory tasks if there is some sort of reward associated with accurate recall. Simply put, it has been shown that participant-pairs are able to recall more correct information when engaged in a memory task if there is a monetary reward involved. Keeping these findings in mind, pretend you are being asked to take part in this study once again, but this time \$50 is being offered to participant-pairs whose *accurate recall* of items is greater than 75%:

- 1) Would you choose to be paired with your partner from today's study if you were participating for money?**

**Yes**

**No**

- 2) How credible would you say your partner from today's study was? (1-Very Credible to 5-Not Credible at all).**

**1 2 3 4 5**

- 3) How accurate would you rate the memory of your partner from today's study? (1-Very accurate to 5-Not accurate at all).**

**1 2 3 4 5**

APPENDIX C

FEUX PILOT STUDY INSTRUCTIONS

Thank you for agreeing to participate in our study on memory. Before we begin our primary task, we will be administering a brief (5-10min) pilot test. For this task, each of you will study an identical word list on the computer for 1 minute. Your task will be to pay attention to the displayed slides as one of you will be randomly selected to be the tester and the other the tested for a verbal recall task. If you are the tested, you will simply recall aloud as many words as you can remember in the correct order to the tester. If you are the tester, you will record the other subjects' responses on a sheet provided by the experimenter. Are there any questions?

APPENDIX D

FEUX PILOT STUDY WORD LIST

You have two minutes to memorize this word list to the best of your abilities. When the two minutes have elapsed, you will need to turn over this paper and verbally recount this list, being mindful of order, to the other subject. Questions?

- 1) DOG
- 2) CAT
- 3) HORSE
- 4) COW
- 5) LION
- 6) TIGER
- 7) ELEPHANT
- 8) PIG
- 9) BEAR
- 10) MOUSE
- 11) RAT
- 12) DEER
- 13) SHEEP
- 14) GIRAFFE
- 15) GOAT

APPENDIX E

FEUX PILOT STUDY “TESTER” (PARTICIPANT) INSTRUCTIONS

Please indicate below using an X whether a word was accurately recalled—the X in this case indicates the word was recalled correctly and in the right order. If the item was not recalled, please leave the space provided BLANK.

- 1) DOG \_\_\_\_\_
- 2) CAT \_\_\_\_\_
- 3) HORSE \_\_\_\_\_
- 4) COW \_\_\_\_\_
- 5) LION \_\_\_\_\_
- 6) TIGER \_\_\_\_\_
- 7) ELEPHANT \_\_\_\_\_
- 8) PIG \_\_\_\_\_
- 9) BEAR \_\_\_\_\_
- 10) MOUSE \_\_\_\_\_
- 11) RAT \_\_\_\_\_
- 12) DEER \_\_\_\_\_
- 13) SHEEP \_\_\_\_\_
- 14) GIRAFFE \_\_\_\_\_
- 15) GOAT \_\_\_\_\_

Total Words Accurately Recalled (out of 15): \_\_\_\_\_

Given the number of items accurately recalled, please circle the corresponding memory category below:

**10-15 items**  
**Very Good Memory**

**5-10 items**  
**Average Memory**

**0-5 items**  
**Poor Memory**

\*\*When you have finished please turn this sheet OVER and wait for you experimenter before proceeding to the next task.

APPENDIX F

CONFEDERATE CHEAT SHEETS FOR HIGH- AND  
LOW-CREDIBILITY CONDITIONS

**Expert**

- |               |   |
|---------------|---|
| 1) "DOG"      |   |
| 2) "CAT"      | 9) "BEAR"   |
| 3) "HORSE"    | 10) "MOUSE"   |
| 4) "COW"      | 11) Pause 2 seconds                                     |
| 5) "LION"     | 12) "DEER"  |
| 6) "TIGER"    | 13) Pause 2 seconds..."the last two were definitely..." |
| 7) "ELEPHANT" | 14) "GIRAFFE and..."                                    |
| 8) "PIG"      | 15) "GOAT"  |

**Non Expert**

- |                         |  |
|-------------------------|--|
| 1) "DOG"                |  |
| 2) "CAT"                | 9) "I'm not sure..." Pause 2 seconds   |
| 3) Pause 8 seconds      | 10) "UH..." Pause 2 seconds            |
| 4)                      | 11) "LLAMA maybe?".....Pause 5 seconds |
| 5)                      | 12)                                    |
| 6)                      | 13)                                    |
| 7)                      | 14)                                    |
| 8) "Um" Pause 2 seconds | 15) "The last one was GOAT I think..." |