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UNIVERSITY OF CALIFORNIA,  
IRVINE

Influence of Misinformation for Prior Affective Reports on Subsequent Memory and Behavior

DISSERTATION

submitted in partial satisfaction of the requirements  
for the degree of

DOCTOR OF PHILOSOPHY

in Psychology & Social Behavior

by

Kevin Jacob Cochran

Dissertation Committee:  
Professor Elizabeth Loftus, Chair  
Professor Linda Levine  
Professor Peter Ditto

2018



## **Dedication**

To my advisors, Beth and Linda: I can't imagine what graduate school would have been like had I not been working with and learning from you. I am extremely lucky to have benefited from the tutelage of two brilliant, compassionate, and bold researchers. Your ability at once to give your students the freedom to ask the questions they most care about and to lead them with sage advice, support, and wisdom is rare and invaluable.

To the other faculty who supported this project, Sarah, Pete, Liz, and Craig: Your guidance, expertise, and criticism all benefited this dissertation greatly. I am grateful to you all for contributing your feedback to this project when it was still being developed, and for your continued support and encouragement.

To my professors, Dan Stokols, Sal Maddi, JoAnn Prause, Joanne Zinger, Roxy Silver, Pete Ditto, Linda Levine, Beth Loftus, George Farkas, and Karen Rook: Thank you for sharing your knowledge and your passion with me. It is because of the discussions, the debates, and the ideas shared in your courses that I am the researcher that I am today.

To my co-researchers, Emily, Marie, and Amanda: Thank you all for everything you did for the cold pressor study. I think it's a rare and lucky thing that a team of friends brought together more or less by chance complement each other so well, but I think we've set a high bar for future teammates to meet.

To all of my research assistants, Alex Resari, Claudia Rodriguez, Steven Schwartz, Kingsley Abel, Melissa Ma, Judy Lee, Yun-Ju Chen, Missy Wilson, Ben Duewell, Phoebe Kao, Jacob Scocca, Alice Chou, Kelly Kang, Kevin McKinnon, Travis Nguyen-Tran, Harmanjeet Dhillon, Cindy Tsai, Brittney Yee, Selene Nguyen, Alexis Pan, Angela Sandoval, Wei Huang, Idalia Mejia, Dilpreet Singh, Ronald Zavaleta, Esther Kim, Marixa Maldonado, Aya Labanieh, Nadia Huynh, Julie Phuong, Dillon Sun, and Maggie Hofstadter: I can say with confidence that my research would have been impossible without you. Thank you so much for your incredible contributions, and for helping to foster an environment where we could learn from each other.

To the research subjects: I take seriously the unpleasantness that you may have experienced in participating in this research. When data are deidentified, it can be easy to think of people as rows in a spreadsheet, and when research projects need IRB approval, it can be easy to think of unpleasant tasks as common laboratory methods. I truly appreciate your participation, and I hope that this dissertation represents a contribution to the ever-growing body of psychological knowledge worthy of your effort.

To my friends: what an amazing group of people you are. I have learned and grown from my friendships with each of you. This project is all about biases in memories for emotional events, and among the most well-known memory biases are the peak and end rules. Graduate school has been a roller coaster of peaks and valleys, and I don't know how that will all shake out when I look back on these six years. But if writing and defending this dissertation with your boundless love and support is how graduate school will end, then I look forward to memories of grad school biased by that feeling.

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## Curriculum Vitae Kevin Cochran

### Education

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- 2018   **Ph.D., Psychology and Social Behavior**  
University of California – Irvine  
Concentration: Social Psychology                      Minor: Psychology and Law
- 2015           **M.A., Social Ecology**  
University of California, Irvine
- 2012           **B.A., Psychological & Brain Sciences and English**  
Johns Hopkins University  
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### Grants, Awards, and Honors

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- 2018   UC Irvine Social Ecology Dean’s Dissertation Writing Fellowship.
- 2018   UC Irvine Psychology & Social Behavior Winter Quarter Writing Fellowship.
- 2017   UC Consortium on Social Science and Law Summer Fellowship (\$2,333).
- 2016   Social Ecology Graduate Student Mentoring Award, UC Irvine (\$200).
- 2015   Peterson Family Fellowship for Psychology and Law. Center for Psychology and Law, UC Irvine.
- 2013   American Psychological Foundation/Council of Graduate Departments of Psychology (APF/COGDOP) nominee, UC Irvine.
- 2012   Newkirk Family Fellowship. Department of Psychology and Social Behavior, UC Irvine.
- 2012   Phi Beta Kappa, national liberal arts honor society, inducted
- 2010   Psi Chi, national psychology honor society, inducted

### Publications

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#### *Peer-reviewed*

Cochran, K. C., Greenspan, R. L., Bogart, D. F., & Loftus, E. F. (2016). Memory blindness: Altered memory reports lead to distortion in eyewitness memory, *Memory & Cognition* 44, 717-726.

#### *Law Reviews*

Cochran, K. C., Greenspan, R. L., Bogart, D. F., & Loftus, E. F. (2018). (Choice) Blind Justice: Legal Implications of the Choice Blindness Phenomenon, *UC Irvine Law Review*.

### ***In edited volumes***

Cochran, K.C., Bogart, D., Peterson, T., & Loftus, E.F. Memory: Reconstructive (2015). In A. Jamieson & A. Moenssens (Eds.) *Wiley Encyclopedia of Forensic Science*. Wiley.

Cochran, K. J., Patihis, L., Frenda, S. J., & Mogilner, T. (2014). Photographs (Altered). In T. R. Levine (Ed.) *Encyclopedia of Deception*. Thousand Oaks, CA: Sage Publications.

### **Poster Presentations**

---

Chen, Y., Duewell, B., Urban, E. J., Cochran, K. J., & Loftus, E. F. (2017, April). *Influence of Personality on Pain Perception: Link Between Neuroticism and Self-Reported Pain*. Poster presented at the 97<sup>th</sup> Annual Convention of the Western Psychological Association, Sacramento, CA.

Urban, E.J., Cochran, K.J., Acevedo, A., Cross, M.P., Pressman, S.D., & Loftus, E.F. (2017, January). *Underestimating pain: Memory blindness for a self-reported painful experience*. Poster presented at the annual Society for Personality and Social Psychology conference, San Antonio, TX.

Greenspan, R. L., Cochran, K. J., Loftus, E. F. (2016, January). *Choice blindness as misinformation: Memory distortion in an eyewitness identification task*. Poster at the annual Society for Personality and Social Psychology conference, San Diego, CA.

Cochran, K.C., Bogart, D., & Loftus, E.F. (2014, May). *Altered Self-Reports Cause Memory Distortion: A Case for "Memory-Blindness"*. Poster presented at the 26<sup>th</sup> Annual Meeting of the Association for Psychological Science, San Francisco, CA.

Keeton, C.P., Cochran K., Santana, E., Ginsburg, G.S. (2012). *Sibling Relationship Quality and Anxiety Among Offspring of Anxious Parents*. Poster presented at the 32nd Annual Meeting of the Anxiety Disorders Association of America, Arlington, VA.

### **Oral Presentations**

---

"You Might Feel a Pinch: Misinformation for Prior Appraisals Influences Subsequent Memory and Behavior." Presentation for Associated Graduate Students Symposium, UC Irvine, April 21, 2017. Awarded \$200 for People's Choice award.

"Influence of Misinformation for Prior Appraisals on Subsequent Memory & Behavior," for Psychology and Social Behavior Departmental Colloquium, UC Irvine, 4/4/2017

"When You're Angry, it Pays to Be a Man." Presentation for Associated Graduate Students Symposium, UC Irvine, April 22, 2016. Awarded \$200 for People's Choice award.

"Manipulated Eyewitness Memory and the Integrity of Memory Evidence." Presentation for the Annual UC Grad Slam, UC Irvine, April 1, 2015. Semifinalist.

"Choice Blindness in an Eyewitness Misinformation Paradigm." Presentation for symposium

“Raising the Blinds: Understanding the Underpinnings of Choice Blindness Across Four Practical Contexts,” Sagana & Cochran (Chairs). 1<sup>st</sup> Annual Meeting of the International Convention of Psychological Science, March 14, 2015, Amsterdam, The Netherlands.

“Manipulated Eyewitness Reports Cause Memory Distortion.” Presentation for Associated Graduate Students Symposium, UC Irvine, March 6, 2015. Awarded \$500 for Judge’s Choice award.

## **Research Experience**

---

Graduate researcher, UC Irvine, 2012 - present. Expertise: Memory, Emotion, Error & Bias. Web-based and in-person research, designing coding schemes for qualitative data, IRB review process, statistical analysis with R and SPSS software, training and managing research assistants, survey design with Qualtrics, preparation of manuscripts, and dissemination of results to broad audiences.

Undergraduate research assistant, Child Anxiety Prevention Lab, Johns Hopkins Medical Institution, June 2011 - May 2012.

Administered questionnaires and behavioral tasks to participants, coded behavioral observation tapes for specific participant behaviors, provided general laboratory support.

## **Teaching Experience**

---

Teaching Assistant, 2013-Present

Assisted with: Psychology Fundamentals, Sport Psychology, The Social Animal, Error & Bias, Statistical Analysis, Naturalistic Field Research, Violence & Society, Abnormal Psychology, Health Psychology, Eyewitness Testimony

Teaching Assistant for Intro to R workshop, UCI Data Science Initiative, 4/13/2017

Guest Lectures (at UC Irvine unless specified):

“Abnormal Psychology,” for Psychology Fundamentals, 8/30/2013

“Evolutionary Mechanisms in Social Psychology,” for The Social Animal, 2/13/14

“Research Methods for False Confessions,” for Eyewitness Testimony, 7/14/14

“Memory,” for Introduction to Psychology, 4/23/15

“Social Cognition,” for The Social Animal, 7/26/15

“Helping, Attraction, and Love” for Psychology Fundamentals, 10/15/15

“Attitudes and Persuasion” for The Social Animal, 1/28/16

“False Memory” for Error and Bias, 3/8/16

“Judgment and Decision Making,” for Psychology Fundamentals, 4/28/16

“Judgment and Decision Making,” for Psychology Fundamentals, 10/25/16

“Choice Blindness and Misinformation,” Saddleback College, 2/22/17

“Judgment and Decision Making,” for Psychology Fundamentals, 10/31/17

## **Leadership and Service**

---

Department Graduate Recruitment Chair, 2017

Organized and facilitated interview weekend for new graduate student recruits for the Psychology and Social Behavior department.

Palo Verde Residents Council, external liaison, July 2013 – July 2016, July 2017 – July 2018.  
Elected to represent residents of the Palo Verde graduate housing community at UC, Irvine. The External Liaison interfaces with other campus leadership groups, such as the Associated Graduate Students (AGS), and the Coordinated Graduate Governance (CGG), including the Senior Executive Director of Student Housing.

Ride Director, Ulman Cancer Fund, 2012, and Rider, 2010.  
Oversaw daily operation of a 27-person cross-country bike tour aimed at raising funds and spreading awareness for the Ulman Cancer Fund. Supervised fundraising, safety standards, community outreach, and public relations.

Director, Trainer, & Listener, A Place to Talk, February 2009 – May 2012  
Provided listening and crisis intervention services for peers, with an emphasis on active listening and open-ended question. Served as a trainer for new members in the final two years, and as co-director of the group

## **Abstract of the Dissertation**

Influence of Misinformation for Prior Affective Reports on Subsequent Memory and Behavior

By

Kevin Jacob Cochran

Doctor of Philosophy in Psychology and Social Behavior

University of California, Irvine, 2018

Distinguished Professor Elizabeth F. Loftus, Chair

Previous research on choice blindness has shown that people often fail to notice changes in their reports about their own preferences, experiences, and internal states, but scarce few studies have investigated whether this failure to detect changes can lead to longer-term consequences.

Another body of research on the misinformation effect has demonstrated that exposing people to misleading information about a witnessed event can alter their subsequent memories for that event, but little is known about whether misleading people about their own reactions to *experienced* events might alter their memories for those events. This question is important because people make decisions about future events in part based upon their memories for their experiences in similar situations in the past. The present experiments apply the principles of the misinformation effect to the methodology of choice blindness in order to answer this question. In Study 1, subjects underwent a painful cold pressor task and reported the level of pain they experienced. Later, some subjects were told they reported less pain than they actually did. Many subjects who received this misinformation failed to detect it. In addition, exposure to this misinformation caused subjects to exhibit a greater reduction in their memory for pain, especially when they failed to detect it. However, this misinformation did not cause subjects to become significantly more willing to complete a similar study in the future. In Study 2, subjects

underwent a Trier Social Stress Test (TSST) and reported the levels of anxiety and excitement they experienced. Later, some subjects were told they reported less anxiety and more excitement than they truly reported. Again, many subjects failed to detect this misinformation. Subjects misinformed in this way later exhibited a greater positive bias in their memory for their anxiety and excitement, especially when they failed to detect the misinformation. Non-detectors also exhibited a more positive change in their experiences on a second TSST. However, neither subjects exposed to misinformation, nor those who failed to detect it, exhibited a greater increase in performance on the second TSST. These findings have important implications for how people recall unpleasant events and how they experience similar subsequent events.

## Introduction

I need to return briefly to a few incidents that have grown into anecdotes, to some approximate memories which time has deformed into certainty. If I can't be sure of the actual events any more, I can at least be true to the impressions those facts left. That's the best I can manage.

-Tony Webster, in Julian Barnes, *The Sense of an Ending*, p. 4

People are often faced with situations that are unpleasant. Whether it is paying taxes, having teeth pulled, or giving a presentation at work, people are exposed to tasks in a variety of contexts that they would rather not have to complete. When an unpleasant situation arises that people might choose to avoid, how do they decide whether to confront it or not? One method people might use is to recall how they felt in similar situations in the past. In deciding whether to schedule a colonoscopy, for example, people might try to recall how much pain or negative emotion they experienced at their last colonoscopy (Redelmeier & Kahneman, 1996). Someone who remembers less pain and less negative emotion should be more willing to undergo a future procedure than someone who remembers more pain and more negative emotion. How people remember negative past events could also be an important determinant of how they perform on similar events in the future. Someone who remembers feeling a high amount of anxiety before giving a presentation at work might anticipate more anxiety for a future presentation, and this increased anxiety might hinder performance (Brooks, 2014; Yerkes & Dodson, 1908).

While peoples' memories of the emotions they felt during past events can be important determinants of their future decision-making and performance, those memories for emotion are not always accurate. Some inaccuracy occurs because of natural biases in memory: for example,



people might fail to account for the duration of a painful experience in remembering how much pain they felt (Redelmeier & Kahneman, 1996), or they might reconstruct their past emotions for an event through the lens of their current feelings towards that event (Levine, Schmidt, Kang, & Tinti, 2012; Bem, 1972). In both of these cases, a person might have an inaccurate recollection without any external source acting on their memory. On the other hand, suggestive influences can also cause memory distortions (Loftus, 2005). When people receive misleading information about events they witnessed (Loftus, Miller, & Burns, 1978) or memories they reported (Cochran, Greenspan, Bogart, & Loftus, 2016), they often incorporate that misinformation into their memories. While a wealth of research has examined the misinformation effect in the context of witnessed events (Loftus, 2005), little work has applied the misinformation effect in an investigation of memory distortion for *reactions* to experienced events.

This brief overview yields a number of important questions: can people be misinformed about their emotional responses during a past event? If they are misinformed about their own affective appraisals, will this misinformation lead to distortions in their memories for the event, increased willingness to experience a similar event in the future, and improved performance on similar events in the future? These questions are important because misinformation about past affective experiences is abundant. A sports reporter might preface a question to an athlete by saying that she appeared confident and “fired up” on the field; a coworker might encourage a colleague before a presentation by mentioning how calm and deliberate the colleague seemed in a previous presentation; a parent might prepare a child for an inoculation by reminding the child how tough and grown-up they acted the last time they received a shot. In each case, someone is receiving information that might not match their memory for their prior emotional state, and that information might cause distortion in their memory. While it may seem as though it would be

difficult to mislead people about their own affective experiences, research on a phenomenon called choice blindness suggests that it might be simpler than expected.

### **Choice Blindness**

Choice blindness refers to the finding that people can often be misled about their own decisions. In a seminal study of choice blindness, subjects were shown pairs of photographs of female faces and asked to choose the one they found more attractive (Johansson, Hall, Sikström, & Olsson, 2005). Then, subjects were given the photograph they chose and asked to elaborate on their reasons for their decision. But on some trials, the experimenters used a double-card ploy to give participants the photograph they had *not* chosen. The researchers were interested in whether subjects would spontaneously report noticing this manipulation, which they called “concurrent detection,” and also whether subjects would say that they had noticed the manipulation during the debriefing, which they called “retrospective detection.” Their findings were clear: subjects only concurrently detected this discrepancy on 13% of manipulated trials, and only retrospectively detected it for 26% of trials. What’s more, when subjects were asked about why they selected a face that they never truly chose, 72% of the time they confabulated some reason for making a decision they had never truly made. For instance, one subject who chose a woman without earrings was told they chose a woman with earrings. They justified their decision by saying, “she’s radiant. I would rather have approached her at a bar than the other one. I like earrings!” (Johansson et al., 2005, p. 118).

The researchers also compared the verbal reports made for non-manipulated trials (in which subjects received the face they had actually chosen) and for manipulated trials (in which subjects received the non-chosen face), in terms of how emotional, specific, and certain those reports were. This type of comparison could potentially show that “blind” subjects (that is,

subjects who failed to detect the mismatch between what they chose and what they were told they chose) weren't paying as much attention during the study, or that they were able to provide reasons for their choice but that those reasons were characterized by confusion and uncertainty. However, the researchers found no differences in these dimensions. Thus, the experiment found that people can often be misled about their own decisions, and that when asked about their reasons for their choices, these blind participants come up with reasonable explanations that look very similar to their explanations for choices they actually *did* make (Johansson et al., 2005).

Since this inaugural study, research on choice blindness has examined this effect in a variety of different contexts. Choice blindness has been demonstrated for taste and smell preferences (Hall, Johansson, Tärning, Sikström, & Deutgen, 2010), political preferences (Hall et al., 2013), and moral decision making (Hall, Johansson & Strandberg, 2012). Choice blindness has also been investigated in more applied settings, such as financial decision making (McLaughlin & Somerville, 2013) and eyewitness identification (Sagana, Sauerland, & Merckelbach, 2013). A cursory review of these studies suggests that the rate of choice blindness found is inversely related to the importance of the choice. When subjects were misled about which flavor of jam they said they preferred, they only detected the manipulation concurrently in about 14% of trials (Hall et al., 2010). But when subjects were misled about their own moral decisions, they detected the manipulation in 41% of trials (Hall et al., 2012). Another way in which these studies vary is in the exact procedure they use. For example, in some studies, rather than having subjects choose between two discrete options, the researchers have subjects make a rating on a scale, and manipulate the subjects' ratings by several scale points (e.g., Hall, Johansson, & Strandberg, 2012). For example, if a subject had rated an object as a 2 on a 7-point scale, the experimenter might later tell the subject he had rated the object as a 4. This might

make for a more liberal test of the effect, since there is more ambiguity in the differences between ratings on a scale than in the difference between dichotomous options. In other studies, rather than confronting subjects immediately with their (manipulated) choice and asking them to elaborate on their reasons for choosing it, subjects are only exposed to the manipulation after a delay (e.g., Sauerland et al., 2013). Including this delay, too, might increase the rate of blindness, since subjects' memories for their initial choice could degrade over time (Loftus et al., 1978). In sum, choice blindness has been shown to be a robust phenomenon that can occur in a variety of different contexts and with a variety of research procedures.

**Consequences of choice blindness.** One question that has received more attention recently is whether choice blindness can have lasting consequences. That is, while it is interesting that people can be misled about their own decisions and that they often confabulate reasons why they made a decision that they never actually made, does this blindness have any long-term influence? One study has examined whether choice blindness can cause preference change (Johansson, Hall, Tärning, Sikström, & Chater, 2013). In the first part of this study, subjects completed 15 trials in which they were shown pairs of female faces and asked to pick which they found more attractive. After some of those trials, subjects were shown the face they had chosen and asked to explain why they chose it. However, in a subset of those trials (that is, in manipulated trials), the face they were told they chose was actually the non-selected face. After subjects elaborated on their decision, they rated the attractiveness of both faces on a 10-point scale. In the second portion of the study, subjects were shown the same pairs of faces a second time and asked again to select the face they found more attractive. After the second round of choices, participants viewed each face one by one and rated its attractiveness. The researchers found that subjects only concurrently detected the manipulations on 11% of trials. In addition,

subjects were highly consistent in their preferences across the two rounds of choosing for non-manipulated trials: they chose the same face in both rounds 93% of the time. However, in manipulated trials, they only chose the same face 57% of the time; in the other 43% of trials, their preference changed after the choice-blindness manipulation. Thus, when subjects were misled about which of two faces they had said was more attractive, their preference shifted, and they later selected the face they had initially rejected. What's more, their attractiveness ratings followed the same pattern. Subjects rated the faces they had initially rejected as more attractive when they had been misled about which face they chose, but not when they hadn't been misled. This study demonstrated that the misinformation present in the choice blindness paradigm can not only go unnoticed, but it can actually lead to preference to change in subjects.

In another study on the long-term effects of choice blindness, the researchers were interested in whether choice blindness can have implications for eyewitness memory (Cochran et al., 2016). In one experiment, subjects viewed a slideshow depicting a crime, and were later asked ten questions about their memories for episodic details of the crime. Subjects responded to these questions on 15-point continuous scales. For example, subjects were asked how tall the thief was and what color his jacket was. Later, in the misinformation stage of the study, subjects were shown their responses to those questions and were asked follow-up questions, like what brand the jacket was. Importantly, for three of the items, the subjects' initial answers had been altered by four points along the scales; this constituted the misinformation. At the end of the experiment, subjects were asked the ten memory questions a second time. The results showed that for control items, subjects' responses to the memory questions did not change between the two tests. However, for manipulated items, subjects' answers changed in the direction of the misinformation. The magnitude of the misinformation effect for subjects who received this "self-

sourced” misinformation was comparable to that of subjects who were told during the misinformation stage that they were seeing the responses another participant had given.

In a second experiment, Cochran et al (2016) investigated whether choice blindness could influence eyewitnesses’ lineup identification decisions. Subjects first watched a slideshow depicting a crime. After a retention interval, they were asked to identify the perpetrator from a target-absent lineup. After another retention interval, they were shown the target they had identified and asked why they selected him, but for some subjects, the face they were shown was one of the non-chosen options. After a final retention interval, they were shown the same faces from the original lineup and asked to identify the perpetrator a second time. The results indicated that 53% of subjects were concurrently blind to the manipulation. Furthermore, overall 35% of subjects in the misinformation condition changed their identifications between the two lineups. The subjects in the misinformation group who were blind to the manipulation were especially likely to change their responses, with 54% doing so. In addition, when subjects changed their responses between the two lineups, the majority of the time they changed to the target implicated by the misinformation. Taken together, these studies demonstrated that people can be blind to changes in their eyewitness reports and identifications, and that this blindness can lead to changes their memories consistent with the misinformation they received.

One choice blindness study has examined how misleading people about their own internal states might have lasting consequences (Merckelbach, Jelicic, & Pieters, 2011). In this experiment, subjects first completed a questionnaire assessing psychological symptoms. For example, subjects assessed how often they had experienced repeated unpleasant thoughts, using a 0-4 scale, where 0 indicated not at all, and 4 indicated all the time. After a short retention interval, subjects were shown their responses to some of the items on the questionnaire, and

asked to recall why they rated those items the way that they had. However, the researchers surreptitiously increased subjects' ratings on two items by two full scale points. Participants were then given the questionnaire a second time for an immediate retest, and were given the questionnaire a third time one week later. The researchers found that 63% of participants were blind to both manipulations. Furthermore, while blind subjects did not differ in their ratings of manipulated and control symptoms at baseline, they rated manipulated symptoms significantly higher both at immediate follow-up and at one-week follow-up. Non-blind participants showed no difference between manipulated and control symptoms at any time. This study demonstrates that people can be misinformed about their own internal states. Moreover, this misinformation causes people to feel differently; if they are told they reported having more unpleasant thoughts, they actually experience having more unpleasant thoughts. Finally, this consequence lingers up to one week after subjects experience the misinformation.

**Choice blindness and self-perception theory.** Choice blindness research shows that people often fail to notice that they are given an option they never selected, and that people generate plausible reasons for decisions that they never made that are indistinguishable from reasons for decisions they *have* made. These findings fall squarely in line with self-perception theory (Bem, 1972). The central tenant of self-perception theory is that people understand “their own attitudes, emotions, and other internal states partially by inferring them from observations of their own overt behavior” (Bem, 1972, p. 2). In other words, when confronted with a situation in which a person's attitudes might be ambivalent or unclear, that person might deduce his or her attitude based on the available evidence: what his or her behavior indicates. For example, if a man feels anxious when he meets a woman, he might believe he is attracted to that woman based on the presence of his anxiety, even if that anxiety is caused by situational factors like walking

on a shaky wooden bridge (Dutton & Aron, 1974). In the context of a choice blindness experiment, if a subject completes a survey and is later asked to explain some of his responses, if some of those responses have been altered by the experimenter, the subject may fail to notice this manipulation. The subject may then infer his attitude based on the new, manipulated responses, rather than his true initial responses.

Self-perception theory also explains how subjects in choice blindness studies confabulate reasons why they made decisions that they never actually made. Nisbett & Wilson (1977) argue that people aren't actually able to know their true reasons behind their attitudes and behaviors. Rather, when asked to explain their cognitive processes, people generate plausible explanations based on their behavior. The same could be true for choice blindness subjects: rather than explaining their true motivations for choosing an option that they never truly chose, subjects instead might examine the option and come up with plausible reasons why someone *might* choose that option (Johansson et al., 2006). The subjects' attitudes are made ambiguous by the nature of the experiment; they are asked to explain a choice that they never made. Just as self-perception theory posits, when their attitudes are made ambiguous, they interpret them like an observer interpreting another person's behavior: by generating reasonable explanations about why one might have made those choices.

**Choice blindness: Summary and future directions.** Taken together, research on choice blindness has demonstrated that people can often be misled about their own decisions. Sometimes these decisions are for more trivial matters like their preferences between two faces or consumer products (Johansson et al., 2005; Hall et al., 2010), but sometimes they are for more important matters like morality and politics (Hall, Johansson, & Strandberg, 2012; Hall et al., 2013). Research has also shown that choice blindness can have lasting consequences for peoples'



preferences (Hall et al., 2013), eyewitness memories (Cochran et al., 2016), and internal psychological states (Merckelbach et al., 2011). Additionally, when people are told they made a decision or report that they never truly made and asked why they did, they nevertheless come up with plausible reasons and confabulations (Johansson et al., 2005; Johansson, Hall, Sikström, Tärning, & Lind, 2006). These reasons are indistinguishable from the reasons people give for decisions they actually made on the dimensions of emotionality, certainty, and specificity, and thus it seems as though this type of reporting is more of an active, constructive process than a retrospective memory test (Johansson et al., 2006; Nisbett & Wilson, 1977; Bem, 1972).

One potential avenue for future research on choice blindness is to examine whether people could be misled about their own affective responses to events. Affective responses have been shown to be malleable (Brooks, 2014) and prone to bias (Levine et al., 2012). Furthermore, people might interpret their emotions based on observations of their own behavior (Bem, 1972). If people naturally have ambiguous or evolving affective reactions to events, it might be easier to mislead them about the exact nature of those reactions. And since people make decisions about future events based on their memories for their emotions in response to similar events in the past (Levine et al., 2012), misleading people about their prior affective responses could have important downstream implications for their willingness and ability to complete similar tasks in the future. While it could be argued that any choice blindness study assessing preferences between options already examines affective responses (Zajonc, 1980), these studies do not examine how people react to affectively charged events, but rather more trivial or low-stakes events. Thus, investigating whether choice blindness can cause a change in how people appraise past events, and whether that change has long-term consequences for the individual, could prove to be a fruitful direction.

## **The Misinformation Effect**

Another psychological phenomenon that shares a number of characteristics with choice blindness is the misinformation effect. In a typical misinformation study, subjects first witness an event, like a simulation of a car running through a stop sign (Loftus et al., 1978). Later, subjects encounter misleading information regarding the event they witnessed; for example, subjects might read a narrative account of the event that describes the car running through a yield sign rather than a stop sign. Finally, subjects are tested on their memory for the original event they witnessed. The finding of interest is that subjects who are exposed to misleading information in this way often incorporate the misinformation into their memory for the original event, and consequently perform worse on the final memory test than subjects who are not exposed to misinformation in this way.

**Theoretical issues of the misinformation effect.** The misinformation effect has been studied for decades (for a review, see Loftus, 2005), and a number of theoretical issues have emerged from among the hundreds of studies investigating the malleability of memory. In particular, two important factors have been uncovered that play an important role in the misinformation effect: the timing of the misinformation and test, and source monitoring.

As with many memory processes, timing has been shown to be an important variable in the misinformation paradigm (Ebbinghaus, 1885; Loftus et al., 1978). There are two critical intervals that can be lengthened or shortened: the interval between the initial event and the misinformation, and the interval between the misinformation and the test (see Figure 1.1). Subjects tend to show the greatest memory distortion when the interval between the event and the misinformation is long, and when the interval between the misinformation and test is short (Loftus et al., 1978). This is likely because when the second interval is short, the misinformation

is fresh in the subjects' minds when they take the memory test, whereas when the first interval is long, the memory for the original event has begun to degrade through the ordinary process of forgetting. By contrast, when the first interval is short and the second interval is long, subjects may begin to forget the content of the misinformation when they take the memory test. In addition, the original event will be fresh in their memory when they are exposed to the misinformation, which might allow them to detect that the misinformation is not accurate.

A second important theoretical issue for the misinformation paradigm is called source monitoring (Lindsay, 2008). The source monitoring framework posits that the sources of memories are not explicitly tagged, but rather that people *infer* the sources of their memories based on qualities of the memories themselves. This framework parallels self-perception theory (Bem, 1972). Whereas in self-perception theory, people infer their attitudes based on their observable behavior, in the source-monitoring framework, people infer the sources of their memories based on characteristics of those memories. For example, a memory of a conversation that took place on a particular Tuesday is not labeled "Tuesday" in a person's mind. Rather, the person might deduce that the conversation took place on that Tuesday because they remember rain during the conversation and it rained on that Tuesday. In short, according to the source monitoring framework, "the distinction between content and source is a blurry one" (Lindsay, 2008, p. 326).

Research on source monitoring has shown that failing to properly identify the source of a cognitive product can influence behavior (Loersch & Payne, 2012). In this study, subjects were primed with either the concept of "fast" or the concept of "slow". Subjects were also led to believe that the priming procedure was either interrupting the formation of any thoughts (internal source) or inducing certain thoughts (external source). Subjects then completed a reading task.

The researchers found that subjects in the internal source group who were primed with “fast” subsequently read faster than subjects primed with “slow,” but there was no significant difference in reading speed between prime conditions for subjects in the external source group or subjects in a control group. When subjects were led to believe that the source of the prime was their own cognition, the prime influenced their behavior, but when they thought the source of the prime was external, the prime did not influence their behavior. This study demonstrated not only that people can misattribute the source of cognitive products based on contextual clues, but also that errors of source monitoring can have an influence on subsequent behavior.

An important concept linked to the source monitoring framework is called the discrepancy detection principle (Tousignant, Hall, & Loftus, 1986). In a series of experiments, Tousignant and colleagues found that when subjects read the misinformation narrative more slowly—either because they were naturally slower readers or because they were instructed to read more slowly—they were more likely to detect that the misinformation was not accurate, and they were less likely to be influenced by the misinformation at the final test. Put differently, an important aspect of the misinformation effect is that subjects do not detect the discrepancy between what actually occurred and the misinformation they receive (Cochran et al., 2016). As discussed above, subjects should be less likely to detect the discrepancy, and thus more likely to experience memory distortion, when the misinformation is presented long after the original event.

**The misinformation effect versus choice blindness.** Choice blindness and the misinformation effect share many characteristics in common. In both cases, subjects are exposed to some kind of initial event, be it a simple decision between two options or a video depicting an accident. Subjects in both cases also receive misleading information about that initial event; in

the choice blindness paradigm, they receive misinformation about their own decisions, whereas in the misinformation paradigm, they receive misinformation about what they witnessed. Finally, subjects are tested; in the misinformation paradigm, subjects usually are given a simple memory test assessing their memories for the original event. In the choice blindness paradigm, the test is usually implicit, and assesses only whether subjects detected the discrepancy between their original choice and the choice they are told they made. However, as discussed above, some studies have assessed whether being exposed to the choice blindness manipulation can have consequences for peoples' attitudes in the same way that being exposed to misinformation can have consequences for peoples' memories (Merckelbach et al., 2011; Johansson et al., 2013; Cochran et al., 2016). In sum, choice blindness and misinformation are similar paradigms, but the former tends to be used in investigations of attitudes and decision-making, whereas the latter is used in studies of eyewitness memory.

Another similarity between the choice blindness and misinformation paradigms is that in both cases, subjects tend to be misled in some domain that is expected to be malleable. For example, not only is memory prone to all sorts of errors and biases (Loftus, 2005), but people are generally quite familiar with making memory errors: people forget information, they misremember information, and two people might have incompatible memories for the same event. Hence, when subjects are exposed to misinformation, there might be a number of plausible explanations why their memories differ from the misinformation. In choice blindness, people are misled about their attitudes, but attitudes can be changed through processes like persuasion. Furthermore, people might have competing attitudes, or they might be apathetic about their choice. In addition, attitudes might be constructed *post-hoc*, through an active, reflective process (Johansson et al., 2006; Nisbett & Wilson, 1977; Bem, 1972). Some studies of

choice blindness examine how subjects respond to misinformation about less malleable characteristics, like subjects' own histories of criminal and norm-violating behavior, but these studies still investigate the generally malleable domain of memory (Sauerland et al., 2013). Thus, misinformation and choice blindness might be especially potent in domains where subjects have ambiguous or competing feelings, where subjects might have experience changing their feelings, or where subjects' feelings might otherwise be malleable, since this malleability lends credibility to the misinformation.

### **Malleability of Affect**

One domain that research on choice blindness and the misinformation effect could more closely examine is affect. In other words, can people be misled about their own affective responses to past events? Some theoretical frameworks for emotion suggest that this could be possible. In appraisal theories of emotion (Moors, Ellsworth, Scherer, & Frijda, 2013), emotions are seen “as processes, rather than states” (p. 119). Appraisal theories characterize emotional episodes as evolving, unfolding events that include a high degree of input from the person experiencing emotion. Indeed, appraisal theorists argue that it is not the event itself that causes an emotional response, but rather the person’s “appraisal [that] elicits or causes emotions” (p. 120). In other words, people are not passive recipients of emotions, but rather they are active parties in their own affective responses, and their current situations, goals, and attitudes might influence the way they appraise an event. Under appraisal theories, then, emotions are seen as malleable and subject to change, and not just over long timespans, but at numerous stages in a continuous affective process.

**Cognitive reappraisal.** One way in which a person’s affective response might be subject to change is through an active process called cognitive reappraisal. Cognitive reappraisal is “a

form of cognitive change that involves construing an emotion-eliciting situation in a way that changes its emotional impact” (Gross & John, 2003, p. 349). For example, a person feeling badly about a situation might try to look at it in a new light or see some new positive component of the event in order to change their emotional response. Reappraisal has been shown to be an effective strategy for regulating emotion (Brooks, 2014; Levine et al., 2012), and tends to be used to decrease negative emotion and increase positive emotion (Gross, 2013).

One study examined peoples’ performance on anxiety-inducing tasks when they reappraised their anxiety as excitement (Brooks, 2014). In the first experiment, subjects who stated aloud “I am excited” reported greater excitement after singing karaoke than subjects who said “I am anxious.” In addition, subjects who stated that they were excited actually sang better than those who said they were anxious. In another experiment, subjects who said they were excited before giving a short speech were rated as more persuasive, confident, competent, and persistent than subjects who said they were calm. In a final experiment, subjects who were instructed to get excited performed better on a math test than subjects who were instructed to remain calm or subjects who were not given any emotional instruction. These experiments demonstrate that emotional reappraisal can have important consequences for behavior and performance. What’s more, these studies show that some affective responses are fluid enough that a minimal reappraisal manipulation can have these consequences. Subjects weren’t instructed to engage in any sort of deep cognitive processing in which they were asked to find the benefits of these stressful tasks; they were merely asked to state out loud “I am excited” or they were instructed to “try to get excited,” and these minimally effortful interventions actually caused changes in performance.

Reappraisal has also been shown to create a bias in memory for emotion (Levine et al., 2012). This study investigated students preparing for a difficult and stressful exam. Three weeks before the exam, students were asked how much positive and negative emotion they were feeling, and also which emotion regulation strategies (i.e., reappraisal, distraction, or suppression) they were using. Three weeks after the exam, participants were asked about their current emotional states, and also to recall how they were feeling three weeks prior to the exam. The researchers found that students who regulated their emotions using reappraisal exhibited a positive bias in their memory for emotion: controlling for their reported pre-exam emotion, reappraisers remembered experiencing more positive emotion and less negative emotion before the exam. In other words, not only does reappraisal influence future performance (Brooks, 2014), but it also influences memory for past emotion, which itself is an important determinant of future behavior (Levine et al., 2012; Frederickson & Kahneman, 1993).

**Regulation, persuasion, & misinformation.** An interesting aspect of cognitive reappraisal, and of emotion regulation in general, is that it need not always be initiated by the person experiencing the affective episode. Extrinsic emotion regulation is the process of helping another individual regulate his or her emotions (Gross, 2013). For example, extrinsic emotion regulation is often employed to help children regulate their emotions: one might tell a child that they are silly when they fall down rather than act scared and protective so that the child laughs rather than cries. However, extrinsic emotion regulation can occur between adults as well, such as when one friend helps another emotionally process bad news (Zaki & Williams, 2013).

The process of extrinsic emotion regulation has some similarities to persuasion and misinformation. In extrinsic emotion regulation, there is an innate inconsistency between the emotional state of the target of the regulation and the goal of the person helping them to regulate



their emotions: by definition, the regulator is trying to make the target feel an emotion in response to an event that is different from what the target already feels. This process might sometimes resemble persuasion: the regulator might try to highlight the positive aspects of a negative event or reassure the target that the negative event wasn't as important as it seemed. Thus, the regulator might try to persuade or convince the target to feel a new emotion in response to the event, or at least to think of it in a new way in order to influence later affective judgments. The persuasive process of extrinsic emotion regulation is also similar to misinformation. Misinformation has been characterized as a type of persuasion in which a person has to reconcile their internal memory with some external report in conflict with their memory (Leding, 2012). Like persuasion and extrinsic emotion regulation, misinformation is a suggestive process (Loftus, 2005). In addition, all three processes can lead to memory distortion characterized by people remembering their past emotions, preferences, and memories as consistent with their present ones (Levine et al., 2012; Henkel & Mather, 2007). Since emotions are malleable, and since they might be influenced by external suggestions in the form of extrinsic emotion regulation, an interesting question is whether people can be misinformed about their own affective responses to events.

### **Previous Research on Affect and Misinformation**

Some literature exists investigating how people respond to misinformation about their own affective responses, but these studies typically employ fairly involved methods and apparatuses. In the 1960s, Stuart Valins conducted a series of studies involving exposing subjects to various stimuli while giving them false feedback about their own heart rates, which could serve as a proxy for affective arousal. For example, in one study, subjects viewed ten slides depicting sexually arousing images of women (Valins, 1966). In one condition, the experimenter placed a

microphone on the subjects' chests and told them they would be able to hear their own heartbeat during the study. In the control condition, subjects were told they would be hearing extraneous, irrelevant sounds during the study. In both conditions, the sounds they heard were actually a pre-recorded, simulated heartbeat. During the experiment, subjects heard their "heartbeats" quicken in response to five of the ten images. Next, subjects were asked to rate the attractiveness of the ten women, and were then told they could take prints of any five of the ten images home with them. The researchers found that subjects rated the women as more attractive when they believed their heart rate had increased in response to seeing the image versus when they did not believe their heart rate had increased. In addition, when subjects were allowed to select prints of the images to take home, subjects tended to select the images that corresponded with the supposed heart rate increase. In sum, misleading subjects about their arousal responses to images of attractive women actually led subjects' preferences to shift in reflection to this misinformation: subjects preferred the images they thought had made their hearts beat faster, and even selected those images to take home with them when given the opportunity.

Valins also examined how false heart rate feedback might influence negative affective responses (Valins & Ray, 1967). In this study, subjects were shown a slideshow consisting of twenty slides. Half of those slides depicted snakes, and the other half depicted the word "shock." When subjects saw the word "shock," they were actually given a shock by the experimenters. Subjects in the experimental condition heard their "heart rates" increase in response to the shock slides, but not in response to the snake slides. One week later, subjects were exposed to the same situation, except that they actually viewed a live snake in place of the snake slides. After this second session, subjects were brought into a room with a caged snake and were asked to try and touch the snake. Subjects who had been given false heartrate feedback were more willing to

touch the snake then subjects who had not been given this feedback. Both of these studies' results can be interpreted in relation to self-perception theory (Bem, 1972). Subjects' attitudes toward both the women and the snakes were ambiguous, so subjects used an external manifestation of their attitudes—their supposed heartbeats—to infer how they felt. When subjects' "heartbeats" quickened in response to sexual images, they inferred that they were aroused by those images; when their "heartbeats" quickened in response to snakes, they inferred that they were afraid.

Taken together, Valins's work on false heart rate feedback demonstrated that people can be misled about their physiological response to emotion-inducing stimuli, and that this misinformation can lead to important behavioral consequences (Valins, 1966; Valins & Ray, 1967). These findings open up avenues for future research. Subjects in these studies were misinformed about their physiological reaction, and it was up to them to determine what their supposed heart rate indicated. In one study, increased heart rate was supposed to indicate fear, but in another, it was supposed to indicate attraction. An interesting follow-up study might investigate misinforming subjects not about their physiological responses, but about their own appraisals of their affect; while a quickened heartbeat might indicate attraction in one context and fear in another, an appraisal is a less open to interpretation. Another question raised by these studies is whether people remembered the stimuli differently when they were exposed to false feedback. While it is interesting in its own right that people experienced the stimuli differently, it is memory for affective stimuli that is important in future decision making, not the experience *per se*, and thus an investigation of how people recall their emotion after receiving misinformation about it could be an important next step (Frederickson & Kahneman, 1993).

One other study has examined misinforming people about their own affect (Aucouturier et al., 2016). In this study, the experimenters developed a novel computer-mediated method to alter the sounds of participants' voices in real time to make them sound emotional. Participants read a story into a microphone, and heard their own voice through headphones. However, the researchers used audio software to covertly filter participants' voices to make them sound more happy, more sad, or more afraid than they actually were. The researchers found that the majority of participants did not detect this manipulation. Moreover, participants' emotions were shifted in ways consistent with the manipulation they received: for example, people who heard their voice filtered to sound happier reported higher levels positive affect. Participants' skin conductance response followed a similar pattern. This study used a novel manipulation to demonstrate that it is possible to mislead people about their own affective responses. However, like the previously discussed research, this study misinformed people about their affect, not their appraisals of it. Additionally, this study did not contain a memory component, leaving it to future researchers to examine how misinformation for affect might influence memory.

**“False” feedback studies.** A number of studies have provided subjects with false information about their own past experiences, and these studies are referred to as false feedback studies. In one study, subjects reported on their interest in various foods and completed a number of other scales. Later, they were told that their responses had been analyzed and had revealed a high likelihood that they had become sick after eating strawberry ice cream (Bernstein, Laney, Morris & Loftus, 2005a). Subjects who were seduced by this suggestion, developing a false belief or memory, subsequently exhibited decreased preference for strawberry ice cream. Analogous results were obtained in a series of studies in which subjects received suggestions that they had become sick after eating hard-boiled eggs or pickles (Bernstein, Laney, Morris, &

Loftus, 2005b), had loved eating asparagus as children (Laney, Morris, Bernstein, Wakefield, & Loftus, 2008), or had become sick after drinking rum or vodka (Clifasefi, Bernstein, Mantonakis, & Loftus, 2013). In one study, the researchers found that subjects who received the false suggestions that they had become sick after eating spoiled peach yogurt subsequently ate less yogurt than controls did when given the opportunity (Scoboria, Mazzoni, & Jarry, 2008). These studies demonstrate that misleading information about autobiographical events can influence peoples' subsequent attitudes and behavior.

One study employed false feedback to influence peoples' overall memories for painful, stressful, and uncomfortable procedures. This study examined children who had received their diphtheria pertussis tetanus shots (Bruck, Ceci, Francoeur, & Barr, 1995). Approximately 11 months after the inoculations, the children participated in three interviews in which they received either neutral or pain-denying feedback. Subjects who received the pain-denying feedback exhibited a greater reduction in their memories for pain and their memories for how much they had cried than subjects who received neutral feedback. This experiment employed a long retention interval between the initial event and the misinformation, and the misinformation was given to children around six years old. An important question for future research is whether similar results can be obtained on a shorter timescale and with adults.

Rather than providing subjects with false feedback, some studies have employed interventions that rather resemble reappraisal by focusing on reducing negative memory biases and bringing to mind the positive aspects of negative events. In one study, the researchers examined the behavior, fear, and pain that children exhibited over two dental visits (Pickrell et al., 2007). Prior to the second dental visit, the children participated in an intervention in they were given examples of how well they had behaved at the first visit, and asked to demonstrate

and share those examples with their parents. This intervention was designed to help the children “focus on positive information, emotions, and beliefs” from their first visit (Pickrell et al., 2007, p. 442). The researchers found that, compared to controls, the subjects in the intervention group improved their behavior more in the second visit. Subjects in the intervention group also had less negative biases for their memories of the pain and fear they experienced during the first visit. In another study, children with leukemia were observed during three lumbar punctures (Chen, Zeltzer, Craske, & Katz, 1999). At two timepoints—after the first lumbar puncture and prior to the second—the children participated in a memory intervention. The intervention involved reminding children about how well they had coped during the procedure and addressing areas in which the children’s memories of the procedure were negatively biased. Compared to controls who did not receive the intervention, subjects that did exhibited lower levels of distress across a variety of measures at the second and third lumbar punctures. These studies demonstrate that helping patients focus on the (true) positive aspects of a dental or medical procedure and confront any negative bias can lead to better behavioral, psychological, and physiological responses to similar procedures in the future.

Finally, one study investigated the physiological, psychological, and behavioral consequences of presenting subjects with feedback about their own past experiences that wasn’t truly false (Pezdek & Salim, 2011). In this experiment, subjects in the experimental condition were told that their responses to a questionnaire had been analyzed by a computer, which had suggested either that “they had experienced some positive public speaking experiences before the age of 10” (p. 1215). They were then asked to recall that experience and write down as many details about it as they could think of. They then completed a Trier Social Stress Test, hereafter TSST (Kirschbaum, Pirke, & Hellhammer, 1993; Kudielka, Hellhammer, & Kirschbaum, 2007).

Compared to control subjects who were told their questionnaire responses suggested they had positive experiences overcoming medical or animal phobias, subjects in the experimental group performed better on the TSST. They also exhibited an attenuated increase in self-report anxiety and salivary cortisol in response to the TSST. The results of this study indicate that positive memories of past experiences can influence anxiety and performance in response to similar future experiences.

**Misinformation and Stress.** An important consideration for research on affect and misinformation is the potentially confounding role that affect can play in moderating cognitive processes. For instance, some research has shown that subjects induced to feel sad just prior to exposure to misinformation about a prior event were less susceptible to the misinformation effect than controls or subjects induced to feel happy (Forgas, Laham, & Vargas, 2005). Of particular interest for the present studies is the influence that stress has on the misinformation effect. One misinformation study found that when subjects completed a TSST ten minutes prior to receiving misinformation, they subsequently exhibited an attenuated misinformation effect when compared to control subjects who did not receive a stress induction (Schmidt, Rosga, Schatto, Breidenstein, & Schwabe, 2013). The researchers suggest that this reduction in the misinformation effect could be due to the stress induction impairing the encoding of the misinformation. However, another experiment involved inducing stress in participants at the time of encoding the original event (a stressful slideshow), rather than the misinformation, and obtained similar results: for subjects who completed the TSST, higher self-reported arousal was related to a reduced misinformation effect for the stressful portion of the slideshow (Hoscheidt, LaBar, Ryan, Jacobs, & Nadel, 2013). Thus, research has found that stress can reduce the magnitude of the misinformation

effect both when it is induced prior to the initial event and when it is induced after the initial event but prior to the presentation of the misinformation.

## Summary

Research on choice blindness has demonstrated that people can often be misled about their own decisions, attitudes, and internal states. However, choice blindness research has largely ignored the lasting consequences of this effect (with some exceptions: Johansson et al., 2013; Cochran et al., 2016; Merckelbach et al., 2011). Choice blindness appears to be a more reliable finding when the subject of the decision is something people might feel ambivalent about (like their preferences between two likeable options), and when their decisions are made more ambiguous (such as when they are asked to report their decision on a scale rather than choosing between dichotomous options). Research on the misinformation effect has demonstrated that exposing people to misinformation can often distort their memories for an event, but this research generally focuses on *witnessed* events rather than peoples' own responses to *experienced* events (Loftus, 2005). This review of the literature yields three important questions: when people are exposed to altered versions of their own affective responses to events, will they detect the misinformation? Will that misinformation influence their memory for the event? And will this misinformation make them more willing or better able to experience similar events in the future? The choice blindness literature suggests that people might be misled about their own affective reactions to events; affective responses are malleable and sometimes ambiguous (Gross & John, 2003), and change naturally over time (Moors et al., 2013). And based on the misinformation literature, this suggestive influence could cause peoples' memories for their affective responses to change, which in turn could influence their future behavior (Redelmeier & Kahneman, 1996). The proposed studies were designed to address these questions.



# Study 1

## Overview and hypotheses

People's current appraisals of past events should be important determinants of their decisions about similar future events (Levine et al., 2012). Study 1 examined whether this is true even when current appraisals are based on misinformation. To that end, the present study investigated how people responded to misinformation about their own appraisal of a painful task: whether people detected the misinformation, whether the misinformation influenced their memories for the task, and whether misinformation made them more willing to complete the task again in the future.

Subjects completed a painful cold pressor task, and then rated how painful the experience was on a scale from 0 to 100. Later, they were asked to elaborate on their pain rating, but for some subjects, the pain rating they were shown was 20 points lower than the pain rating they actually made. Subjects returned to the lab approximately 1-2 days later for a second session in which they recalled how much pain they felt and reported how willing they would be to complete the cold pressor task a second time. Other researchers, noting how memories for painful procedures can be biased, have asked whether we should concern ourselves with minimizing the pain experienced or the pain remembered (Redelmeier & Kahneman, 1996). Study 1 poses a new possibility: minimize the pain experienced, and use misinformation to minimize the pain remembered.

*Hypothesis 1:* a large proportion of subjects would be blind to the manipulation.

*Hypothesis 2a:* subjects who received misinformation about how much pain they experienced would remember the task as less painful than subjects who experience no misinformation.

*Hypothesis 2b:* subjects who are blind to the misinformation would exhibit the largest misinformation effect, whereas subjects who are not blind would not differ greatly from control subjects.

*Hypothesis 3:* subjects who received misinformation about how much pain they experienced would be more willing to complete the cold pressor task in the future.

*Rationale:* Previous research on choice blindness has shown that people are often blind to changes in their own reports, even for important and self-relevant decisions (*Hypothesis 1*). False memory research has shown that when people receive misinformation, they often incorporate it into their memories for the event, even when they receive misinformation about their own reports (*Hypothesis 2a*). However, some studies have found that memory distortion occurs primarily for those subjects who fail to detect that the misinformation is erroneous (*Hypothesis 2b*). Finally, research has shown that it is our present memories and present appraisals for affective events, not the affect we experienced at the time, that determines our future decision-making. Therefore, our memories for a painful event—even false memories—should predict our willingness to complete similar events in the future (*Hypothesis 3*).

## **Method**

**Subjects.** Three-hundred three eligible subjects were recruited for participation. Ten subjects were excluded from analyses due to incomplete data, and 24 further subjects were excluded due to an equipment malfunction. Of the final sample of 269 subjects, 244 completed session 2 with full data.

The final sample of 269 subjects included 82.5% females. The ethnic breakdown was 50.2% Asian/Pacific Islander, 31.6% Hispanic/Latino, 8.9% White, 1.5% Black, 4.8% biracial, and 3% other. The mean age was 20.6.

## **Measures.**

***Pain ratings.*** Subjects rated how much pain they experienced during the cold pressor task on a sliding scale anchored at 0 (no pain) and 100 (the worst pain imaginable) (see Appendix B). In the second session, subjects used a similar scale to report how much pain they recalled experiencing during the task (see Appendix D).

***Misinformation.*** Roughly 25 minutes after reporting the amount of pain they experienced during the cold pressor task, subjects were exposed to the critical manipulation (see Appendix C). Subjects were reminded of their prior pain rating, and asked to elaborate on why they rated the task the way that they did. Subjects in the control group were shown their true prior pain rating, whereas subjects in the misinformation group were shown their prior pain rating minus 20 points.<sup>1</sup>

***Future willingness.*** In order to measure how willing subjects would be to participate in a similar task in the future, six questions were developed. The first 195 valid subjects to complete session 2 of the experiment were given two questions assessing their future willingness to participate (see Appendix E). These questions asked subjects how willing they would be to participate in a similar study in the future on a 5-point scale (Future Willingness 1), and how much they thought we should pay our subjects as a free response item (Future Payment 1).

After reviewing the data from the first set of subjects, the willingness measures were revised (see Appendix F). The final 49 valid subjects to complete session 2 of the experiment were asked four questions assessing their future willingness to participate. These questions asked subjects how willing they would be to complete *only* the cold pressor task in the future on a 5-point scale (Future Willingness 2), how much they thought we should pay our subjects to

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<sup>1</sup> The scale subjects reported their pain on ranged from 0 to 100, but due to a clerical error the critical manipulation reads “1 to 100”. It is not expected that this had any influence on the results of the present study.

complete *only* the cold pressor task on a 13-point scale anchored at \$0 and \$60 (Future Payment 2), how willing they would be to complete an entire study similar to the present one in the future on a 5-point scale (Future Willingness 3), and how much they thought we should pay our subjects to complete an entire similar study on a 13-point scale anchored at \$0 and \$60 (Future Payment 3).<sup>2</sup>

***Detection of misinformation.*** Four measures were devised to assess whether subjects detected the misinformation. To assess concurrent detection, subjects' free responses to the misinformation question were evaluated by three independent condition-blind coders for evidence of detection.<sup>3</sup> Subjects were considered detectors if at least two of the coders coded them as such. To assess retrospective detection, subjects' free responses to three of the debriefing prompts were evaluated (see Appendix G for the full debriefing prompts). The first prompt (Retrospective 1 Detection) was "Did you find any part of the study strange or surprising during either visit?" Subjects were coded as detectors if they mentioned anything about how the pain rating they were told they made was not accurate. The second prompt (Retrospective 2 Detection) was "the computer reminded you of [your pain] rating. Did you notice anything strange about this process?" Subjects were coded as detectors if they mentioned that they found something strange unless what they found strange was irrelevant to the study's hypotheses. The final prompt (Retrospective 3 Detection) was delivered after explaining to subjects the nature of the study, and asked, "Do you think you were in the group that was given the lower pain rating?" This measure was intended to be the broadest estimate of detection, so subjects were coded as detectors if they responded "yes," "probably," and even "maybe." They were coded as non-detectors if they responded "no," or "I don't think so." All measures were coded independently

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<sup>2</sup> The revision to this measure is discussed more fully in the discussion for Study 1.

<sup>3</sup> Detection was coded in two batches. For all measures of detection, three coders were used for the first batch of responses. One of those coders and two new coders were used for the second batch.

of each other. Thus, subjects could be coded as concurrent detectors and retrospective 2 detectors. In addition, when the coders were evaluating one measure of detection, they did not know how each subject had responded to the other measures of detection

### **Procedure**

The procedure for Study 1 is outlined in Figure 1.2. Subjects were recruited online via UC Irvine's SONA system for a two-part study. The second part of the study was scheduled 1-2 days after the first part. The study was advertised as an experiment on "affect and stress responses" in order to reduce participant suspicion of the study's main hypotheses. Once subjects agreed to participate in the study, they completed a screening questionnaire to ensure that they were not at risk for any complications with the cold pressor task (see Appendix A). If they met the criteria for the study, they were allowed to continue with the study procedures.

Subjects then completed some baseline measures. These measures were collected to bolster the cover story and as theoretically relevant variables for exploratory analyses. Measurements of subjects' height and weight were taken, as well as measurements of the circumference of subjects' waists, hips, and arms. Subjects then completed questionnaires assessing mood (Profile of Mood States Circumplex Variant, POMS; McNair, Lorr, & Droppleman, 1971), depressive symptoms (Center for Epidemiological Studies-Depression, CES-D; Radloff, 1977), anxiety (State-Trait Anxiety Inventory, STAI; Spielberger, Gorsuch, Lushene, Vagg, & Jacobs, 1983), pain sensitivity (Pain Sensitivity Questionnaire, PSQ; Ruscheweyh, Marziniak, Stumpfenhorst, Reinholz, & Knecht, 2009), pain attitudes (Pain Attitudes Questionnaire, PAQ; Yong, Gibson, Horne, & Helme, 2000), and sensation-seeking (Brief Sensation Seeking Scale, BSSS; Hoyle, Stephenson, Palmgreen, Lorch, & Donohew,

2002). Subjects then completed a 5-minute resting period, during which their blood pressures were measured four times in 90-second intervals.

Next, subjects completed the cold pressor task. Subjects immersed their non-dominant hand into cold water for 90 seconds. A water pump and thermometer were used to ensure that the water was continuously circulating at a temperature of 4 degrees Celsius. Participants were allowed to discontinue the cold pressor task at any time if it was too painful or if they felt ill, and a research assistant was present in the room to monitor the participant and discontinue the task in the event of an adverse reaction. One additional blood pressure measurement was taken 30 seconds into the cold pressor task

Immediately following the cold-pressor task, participants entered a 5-minute recovery period. During this time, four additional blood pressure measurements were taken at 90-second intervals. Subjects were also asked on scales ranging from 0 to 100 how much pain, distress, positive affect, and negative affect they felt during the task (see Appendix B). They then completed the POMS (McNair et al., 1971) for a second time, and completed a questionnaire gauging their thoughts on the cold pressor task adapted from Hovasapian & Levine (2015). Participants were asked to sit quietly for the remainder of the recovery period.

Next, participants completed a battery of questionnaires. The purpose of these questionnaires was threefold: first, completing the questionnaires took time which comprised a retention interval between the time of the cold pressor task and subjects' exposure to misinformation. Second, these questionnaires helped to bolster our cover story. Finally, these questionnaires measured theoretically relevant for exploratory analyses. These questionnaires included the Big Five Inventory (BFI, John & Srivastava, 1999), the 12-Item Grit Scale (Duckworth, Peterson, Matthews, & Kelly, 2007), the Perceived Stress Scale (PSS, Cohen,

Kamarck, & Mermelstein, 1983), the Life Orientation Test Revised (LOT-R, Scheier, Carver, & Bridges, 1994), the Affect Valuation Index (AVI, Tsai, Knutson, & Fung, 2006), and the Behavioral Inhibition/Behavioral Activation Scales (BIS/BAS; Carver & White, 1994).

Completing the questionnaires took roughly 20 minutes.

Next, subjects were exposed to the critical manipulation. Subjects were shown how they rated their pain for the cold pressor task and be asked to elaborate on why they rated their pain in that way. Subjects in the control condition were shown their true previous pain rating, whereas subjects in the misinformation condition were shown their true previous pain rating minus 20; this constituted the misinformation. Subjects were given space to respond in an open-ended way. Finally, subjects completed a demographic questionnaire. Following that, they were reminded of their appointment for the second session.

Session 2 was typically 1-2 days after Session 1, but due to scheduling difficulties, the exact timing varied. The time between the two sessions was recorded. Subjects first were asked to complete the POMS (McNair et al., 1971). They were then asked to recall how much pain, distress, positive affect, and negative affect experienced during the cold pressor task. Then subjects completed some additional questionnaire measures. These questionnaires assess traits and tendencies that could be theoretically interesting for exploratory analyses. They include the Rosenberg Self-Esteem scale (RSES; Rosenberg, 1965), the Marlow-Crowne Social Desirability Scale (SDS, Crowne & Marlowe, 1960), the Hypersensitive Narcissism Scale (Hendin & Cheek, 1997), the Reiss-Epstein-Gursky Anxiety Sensitivity Index (ASI, Reiss, Peterson, Gursky, & McNally, 1986), and the Revised Social Anhedonia Scale (Eckblad, Chapman, Chapman, & Mishlove, 1981). As subjects began this block of questionnaires, a research assistant interrupted them. They explained that we were planning another, similar cold pressor study with some

additional funding, and that we wanted to know the subjects' opinions on it. They asked subjects how willing they would be to complete a future cold pressor study and how much money the participants think we should pay future subjects for their participation (see Appendices E and F).

Finally, subjects were debriefed. Using a funneled debriefing procedure, a research assistant asked subjects increasingly specific questions querying their suspicion of the study's main hypotheses in order to assess if they detected the manipulation. They were asked what they thought the study was about, if they found anything strange or surprising about the study, and if they found anything strange about how they were reminded of their pain rating. After being told the purpose of the study, subjects were asked to guess which group (experimental or control) they thought they were in (see Appendix G).

## **Results**

**Detection.** To assess the reliability of the coders, Krippendorff's alphas were calculated for each of the measures of detection. As can be seen in Table 1.1, reliability was generally very high.

Table 1.2 displays the rates of detection across different measures. Of the 135 subjects who received misinformation, 15 (11.1%) detected it concurrently. For retrospective detection, 127 subjects in the misinformation group completed the second session. For retrospective 1 detection, 1 subject (0.8%) was coded as a detector. For retrospective 2 detection, 42 subjects (33.1%) were coded as detectors. For retrospective 3 detection, 85 subjects (66.9%) were coded as detectors.

In order to estimate the bias of the detection measures, the coders also evaluated the responses of control subjects. Of the 134 control subjects, 0 were coded as concurrent detectors, suggesting that this measure has a low false-positive rate. For retrospective detection, 117



control subjects completed the second session. For retrospective 1 detection, 0 were coded as detectors. For retrospective 2 detection, 8 subjects (6.9%) were coded as detectors<sup>4</sup>. For retrospective 3 detection, 23 subjects (19.7%) were coded as detectors. These percentages represent estimates of the bias of the detection measures, but they do not imply that control subjects actually detected the misinformation, as control subjects were never given any misinformation. In subsequent analyses, when referring to detectors, it is only subjects in the misinformation group who are considered.

**Temperature, duration, time, and pain:** The temperature of the water was recorded before and after the cold-pressor task. The initial temperature ranged from 3.6 to 4.4 degrees Celsius ( $M = 3.99$ ,  $SD = .12$ ). The temperature of the water after the task ranged from 3.8 to 5.0 degrees ( $M = 4.44$ ,  $SD = .23$ ). Neither the temperature of the water before nor the temperature after the cold pressor task was correlated with the amount of pain subjects reported experiencing or remembering all  $ps > .17$ .

Participants were allowed to discontinue the task at any time. The duration subjects kept their hand in the water ranged from 2 seconds to 93 seconds ( $M = 71.08$ ,  $SD = 27.12$ ). 166 participants (62.6%) kept their hand in the water for the full 90 seconds. For subjects with complete data for both sessions, the duration they kept their hand in the water was correlated with their initial pain ratings ( $r = -.13$ ,  $p = .041$ ) and with their recalled pain ratings ( $r = -.20$ ,  $p = .001$ ).

The number of days between sessions 1 and 2 varied due to scheduling. Of the 244 subjects who completed the second session, 233 (95.5%) did so one or two days following their initial session. The other 11 subjects completed the second session between 0 and 7 days after

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<sup>4</sup> One subject had missing data for retrospective 2 detection. This subject was in the control condition, so they are included in subsequent analyses.

their initial session. Excluding these 11 participants, there was no correlation between the number days between sessions and initial pain rating, recalled pain rating, difference in pain rating, or condition, all  $ps > .24$ .

Initial pain ratings ranged from 0 to 100 ( $M = 68.00$ ,  $SD = 19.84$ ). Recalled pain ratings ranged from 5 to 100 ( $M = 60.42$ ,  $SD = 21.46$ ). As all subjects in the misinformation group were shown lower pain ratings, 6 subjects (4.4%) were told they rated their pain as a negative number. However, none of them were coded as concurrently detecting the misinformation, so this is not expected to influence any of the results.

**Distortion in memory for pain:** In order to determine if the misinformation affected the degree of bias in subjects' memories for pain, a 2 (condition: misinformation vs control) by 2 (report type: experience vs memory) mixed ANOVA was conducted. The results of this analysis can be seen in Figure 1.3. There was a significant main effect for report type,  $F(1, 242) = 137.75$ ,  $p < .001$ ,  $\eta_p^2 = .363$ , and a significant report type by condition interaction,  $F(1, 242) = 40.49$ ,  $p < .001$ ,  $\eta_p^2 = .143$ . No significant between-subjects effect was found for condition,  $p = .40$ .

To examine the nature of this interaction, follow-up t-tests were conducted. There was no significant difference in initial pain rating between the experimental group ( $M = 69.20$ ,  $SD = 19.19$ ) and the control group ( $M = 67.21$ ,  $SD = 19.79$ ),  $t(242) = .80$ ,  $p = .43$ . However, there was a significant difference in recalled pain between the experimental group ( $M = 57.39$ ,  $SD = 21.82$ ) and the control group ( $M = 63.70$ ,  $SD = 20.64$ ),  $t(242) = 2.32$ ,  $p = .02$ . In addition, subjects in the control group exhibited a reduction in their pain ratings between the two time points, paired  $t(116) = 3.98$ ,  $p < .001$ , as did subjects in the experimental group, paired  $t(126) = 12.37$ ,  $p < .001$ . Subjects in both groups reduced their pain ratings between the two time points, and this effect was greater for subjects in the experimental group.

**Blindness and memory distortion<sup>5</sup>:** *Concurrent detection.* In order to determine if concurrent detection of the misinformation was associated with subjects' bias in their memories for their pain, a 3 (detection status: control vs non-detectors vs detectors) by 2 (report type: experience vs memory) mixed ANOVA was conducted. The results of this analysis can be seen in Figure 1.4. There was a significant main effect of report type,  $F(1, 241) = 32.10, p < .001, \eta_p^2 = .118$ , and a significant between-subjects effect of detection status,  $F(2, 241) = 5.11, p = .007, \eta_p^2 = .041$ . These main effects were qualified by a report type by detection status interaction,  $F(2, 241) = 34.74, p < .001, \eta_p^2 = .224$ .

To examine the nature of this interaction, follow-up tests were conducted. There was no significant difference in pain experienced between controls ( $M = 67.21, SD = 19.79$ ), detectors ( $M = 78.14, SD = 15.87$ ), and non-detectors ( $M = 68.09, SD = 19.33$ ),  $F(2, 241) = 2.00, p = .14$ . However, there was a significant difference in pain recalled between controls ( $M = 63.70, SD = 20.64$ ), detectors ( $M = 78.57, SD = 17.53$ ), and non-detectors ( $M = 54.77, SD = 20.91$ ),  $F(2, 241) = 11.16, p < .001, \eta_p^2 = .085$ . The differences between all three groups were significant (control vs. detector  $p = .011$ ; control vs. non-detector  $p = .001$ ; detector vs. non-detector  $p < .001$ ). In addition, the significant ANOVA interaction indicates a significant in the amount the three groups reduced their pain ratings between the two time points,  $F(2, 241) = 34.74, p < .001, \eta_p^2 = .224$ . Non-detectors ( $M = 13.32, SD = 10.31$ ) reduced their pain ratings significantly more than did detectors ( $M = -.43, SD = 4.75$ ) or controls ( $M = 3.50, SD = 9.52$ ),  $ps < .001$ . Detectors did not significantly reduce their pain rating, paired  $t(13) = .34, p = .74$ , and the amount detectors and controls reduced their pain ratings did not significantly differ from each other,  $p = .153$ .

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<sup>5</sup> The duration subjects kept their hand submerged in water was negatively correlated with both their experienced pain and their recalled pain. However, including a binary variable assessing whether subjects completed the full 90-second task as a covariate in the analyses of memory distortion did not change the inferences resulting from those analyses. Therefore, the analyses presented did not include this variable as a covariate.

*Retrospective 1 Detection.* Since only one subject was coded as a retrospective detector under this measure, no separate analyses were conducted.

*Retrospective 2 Detection.* In order to determine if retrospective detection of the misinformation was associated with subjects' bias in their memories for their pain, a 3 (detection status: control vs non-detectors vs detectors) by 2 (report type: experience vs memory) mixed ANOVA was conducted. The results of this analysis can be seen in Figure 1.5. There was a significant main effect of report type,  $F(1, 241) = 144.28, p < .001, \eta_p^2 = .374$ , and no significant between-subjects effect for condition,  $p = .36$ . This main effect was qualified by a report type by detection status interaction,  $F(2, 241) = 24.66, p < .001, \eta_p^2 = .170$ .

To examine the nature of this interaction, follow-up tests were conducted. There was no significant difference in pain experienced between controls ( $M = 67.21, SD = 19.79$ ), detectors ( $M = 70.31, SD = 21.49$ ), and non-detectors ( $M = 68.65, SD = 18.06$ ),  $F(2, 241) = .419, p = .66$ . However, there was a significant difference in pain recalled between controls ( $M = 63.70, SD = 20.64$ ), detectors ( $M = 62.02, SD = 23.87$ ), and non-detectors ( $M = 55.12, SD = 20.50$ ),  $F(2, 241) = 4.20, p = .016, \eta_p^2 = .034$ . The difference in recalled pain between the control group and the non-detectors was significant ( $p = .005$ ), while the differences between the control group and the detectors ( $p = .66$ ), and the detectors and the non-detectors ( $p = .085$ ), were nonsignificant. In addition, there was a significant difference in the reduction of pain ratings between the two time points between the controls ( $M = 3.50, SD = 9.52$ ), the non-detectors ( $M = 13.54, SD = 10.53$ ), and the detectors ( $M = 8.29, SD = 10.44$ ),  $F(2, 241) = 24.66, p < .001, \eta_p^2 = .170$ . The differences between all three groups were significant (control vs. detector  $p = .009$ ; control vs. non-detector  $p < .001$ ; detector vs. non-detector  $p = .006$ ). Subjects in all three groups reduced their pain ratings between the two time points, but this reduction was greatest for subjects who failed to

detect the misinformation, somewhat smaller for subjects who detected the misinformation, and smallest for subjects in the control group.

*Retrospective 3 Detection.* In order to determine if retrospective detection of the misinformation was associated with subjects' bias in their memories for their pain, a 3 (detection status: control vs non-detectors vs detectors) by 2 (report type: experience vs memory) mixed ANOVA was conducted. The results of this analysis can be seen in Figure 1.6. There was a significant main effect of report type,  $F(1, 241) = 196.12, p < .001, \eta_p^2 = .449$ , and no significant between-subjects effect for condition,  $p = .69$ . This main effect was qualified by a report type by detection status interaction,  $F(2, 241) = 27.04, p < .001, \eta_p^2 = .183$ .

To examine the nature of this interaction, follow-up tests were conducted. There was no significant difference in pain experienced between controls ( $M = 67.21, SD = 19.79$ ), detectors ( $M = 67.93, SD = 19.65$ ), and non-detectors ( $M = 71.76, SD = 18.17$ ),  $F(2, 241) = .863, p = .42$ . However, there was a marginally significant difference in pain recalled between controls ( $M = 63.70, SD = 20.64$ ), detectors ( $M = 58.26, SD = 21.96$ ), and non-detectors ( $M = 55.64, SD = 21.70$ ),  $F(2, 241) = 2.88, p = .058, \eta_p^2 = .023$ . The difference in recalled pain between the control group and the non-detectors was significant ( $p = .036$ ), while the differences between the control group and the detectors ( $p = .074$ ) and the detectors and the non-detectors ( $p = .515$ ) were nonsignificant. In addition, there was a significant difference in the reduction of pain ratings between the two time points between the controls ( $M = 3.50, SD = 9.52$ ), the non-detectors ( $M = 16.12, SD = 10.68$ ), and the detectors ( $M = 9.67, SD = 10.19$ ),  $F(2, 241) = 27.04, p < .001, \eta_p^2 = .183$ . The differences between all three groups were significant (control vs. detector  $p < .001$ ; control vs. non-detector  $p < .001$ ; detector vs. non-detector  $p = .001$ ). Subjects in all three groups reduced their pain ratings between the two time points, but this reduction was greatest for

subjects who failed to detect the misinformation, somewhat smaller for subjects who detected the misinformation, and smallest for subjects in the control group.

**Willingness to complete the task again:** Subjects who experienced or remembered the cold pressor task as less painful were hypothesized to be more willing to complete a similar study again in the future. To test this hypothesis, correlations were computed between pain experienced, pain recalled, the six measures of future willingness both for subjects in the control group only and for all subjects. For control subjects, pain experienced was not significantly correlated with any measure of future willingness, all  $ps > .14$ . Pain recalled was significantly correlated only with Future Payment 1 (see appendix E),  $r = .21, p = .045$ . For all subjects, pain experienced was not significantly correlated with any measure of future willingness, all  $ps > .06$ . Pain recalled was significantly correlated only with Future Payment 1,  $r = .18, p = .013$ .

An independent samples  $t$ -test was performed to assess whether subjects who received misinformation about their pain ratings would recommend less compensation for subjects in a similar study. The results of this test can be seen in Figure 1.7. Because Levene's test for equality of variances was significant ( $F = 5.95, p = .016$ ), an unpooled  $t$ -test was used. Results indicated a marginal effect for condition,  $t(143.44) = 1.83, p = .070$ . Subjects who received misinformation ( $M = 12.17, SD = 8.26$ ) recommended marginally less compensation than control subjects ( $M = 15.23, SD = 14.08$ )<sup>6</sup>.

Another question is whether subjects who failed to detect the misinformation would be more willing to undergo a similar task in the future than subjects who detected the misinformation or control subjects. Because of heterogeneity of variances and unequal group

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<sup>6</sup> Some outliers existed for Future Payment 1. When the data were 90% winsorized, the difference in Future Payment 1 between the misinformation group and control group was no longer marginally significant,  $M_{\text{control}} = 13.5, SD = 8.25, M_{\text{misinformation}} = 11.92, SD = 7.19, t(193) = 1.43, p = .155$ . This was true despite the correlation between pain recalled and Future Payment 1 remaining significant both for controls only ( $r = .212, p = .043$ ) and for both groups combined ( $r = .155, p = .031$ ).

sizes, Kruskal-Wallis H tests were used to assess this question. No significant results were found for concurrent detection, retrospective 2 detection, or retrospective 3 detection, all  $ps > .42$ .

**Exploratory analyses of detectors:** In order to better understand the process of detecting misinformation, as well as the methods of measuring detection, point-biserial correlations were calculated between each level of detection and a subset of the individual difference measures included as filler tasks. These correlations can be seen in Table 1.3. Concurrent detection was positively correlated with hypersensitive narcissism ( $r = .198, p = .026$ ), and negatively correlated with social desirability ( $r = -.269, p = .002$ ) and conscientiousness ( $r = -.196, p = .027$ ). Retrospective 2 detection was negatively correlated with sensation seeking ( $r = -.204, p = .022$ ). Retrospective 3 detection was positively correlated with the fun-seeking subscale of the behavioral activation scale ( $r = .200, p = .024$ ), and negatively correlated with openness to experience ( $r = -.245, p = .005$ ). Interestingly, no two measures of detection shared a significant correlation with any individual difference measure. However, retrospective 2 detection exhibited a marginally significant negative correlation with social desirability ( $r = -.174, p = .050$ ), which was also negatively correlated with concurrent detection. No significant correlations with detection were found for social anhedonia, depression, the drive subscale of the BAS, the reward subscale of the BAS, the BIS, self-esteem, the life orientation test, the perceived stress scale, anxiety sensitivity, state anxiety, trait anxiety, neuroticism, agreeableness, extraversion, or grit, all  $ps > .12$ .

## **Discussion**

The results from Study 1 demonstrate that people can be misled about their own reports of the pain they experienced for a cold-pressor task. Subjects who received misinformation suggesting they reported less pain than they actually reported later exhibited a greater positive

memory bias (i.e., they reduced their pain ratings more) than control subjects who didn't receive misinformation (Hypothesis 2a). This was especially true for subjects who retrospectively failed to detect that the pain rating they were shown had been falsified: retrospective detectors (with respect to both retrospective 2 and retrospective 3) exhibited a greater reduction in their pain ratings than control subjects did, but a lesser reduction than subjects who failed to detect the misinformation retrospectively. However, subjects who concurrently detected the misinformation did not exhibit a reduction in their pain ratings (Hypothesis 2b). These findings are consistent with past research demonstrating that choice blindness can have lasting effects for memory (Cochran et al., 2016; Stille et al., 2017), and that when people detect the discrepancy between the misinformation and the facts, they are less likely to be seduced by misinformation (Tousignant et al., 1986). These findings also extend the literature on memory blindness by demonstrating that memory blindness can be found even for peoples' own appraisals of their experiences.

One interesting finding from Study 1 was how the different measures of detection performed with regard to both the misinformation group and control group (Hypothesis 1). Only 11.1% of subjects in the misinformation group were coded as concurrent detectors, which is low in comparison to a number of other choice blindness studies (e.g., Hall et al., 2012). One potential reason why this rate was so low was because of the nature of the manipulation. Subjects were "moved" 20 points on a scale from 0 to 100. Rather than choosing between binary options (e.g., Hall et al., 2010), subjects made a rating on a continuous scale, which may have caused their decisions to be more ambiguous. Furthermore, the degree that subjects' responses were "moved" was 20% of the total scale, which is lower than some other studies (Cochran et al., 2016, Experiment 1). The number of subjects coded as retrospective detectors by both the



retrospective 2 and retrospective 3 measures was more in line with the findings from previous research (Hall et al., 2012).

The number of subjects in the control condition who were coded as detectors can be a useful metric in understanding the bias in these various measures of detection. No control subjects were coded as concurrent detectors or retrospective 1 detectors, suggesting that these measures have low false-positive rates. For retrospective 2, 8 control subjects (6.9%) were coded as detectors, while for retrospective 3, 23 control subjects (19.7%) were coded as detectors. These rates suggest that whereas retrospective 2 exhibits a moderate false-positive rate, retrospective 3 exhibits a substantial false positive rate. Nevertheless, because retrospective 3 involves explaining the nature of the study to subjects and asking whether they thought they received misinformation, it is useful in bounding the estimate of the number of subjects who truly detected the misinformation without any further prompting from the research assistants. Eighty-five misinformation subjects (66.9%) were coded as retrospective 3 detectors, and 42 (33.1%) were coded as non-detectors. Thus, at a bare minimum, 33.1% of subjects failed to detect the misinformation.

Another curious result from Study 1 was that subjects who remembered experiencing less pain were not more willing to participate in a similar study in the future by any measure except Future Payment 1 (Hypothesis 3). To understand this result, it is worthwhile to review the willingness measures that were used (Appendices E and F). Several of the measures used (Future Willingness 1, Future Willingness 3, and Future Payment 3) ask subjects how willing they would be to complete an entire similar study in the future. Since the study lasted approximately 90 minutes over two days, and the cold pressor task only lasted 90 seconds, it could be the case that subjects were more concerned with the total time commitment than with the degree of pain they

remembered experiencing. Future Payment 1 tacitly asked about completing an entire similar study, but did not say so explicitly. The reason for redesigning the willingness questionnaire was to distinguish between participants' willingness to complete only the cold pressor task again and their willingness to complete an entire similar study. However, in redesigning the questionnaire, several other changes were made. Rather than allowing subjects to write in any dollar amount they wanted, subjects were instead asked to select a dollar amount from a 13-point scale. It is possible that the selections available on the scale communicated to subjects information about how much money they were expected to suggest. Future Willingness 2 and Future Payment 2 were also on the same page as Future Willingness 3 and Future Payment 3, which may have also influenced their responses: subjects should have reported greater willingness to participate in the cold pressor task only than to participate in an entire study that included a cold pressor task as one element. Despite these limitations, subjects who received misinformation about their pain ratings did suggest marginally lower payment than control subjects. Future research should take into consideration exactly what subjects are hypothesized to be more or less willing to do when designing these types of questionnaires.

Exploratory analyses examined what individual difference variables were correlated with different measures of detection. Interestingly, no two measures of detection were correlated with the same individual difference variable. Concurrent detection was positively correlated with hypersensitive narcissism, and negatively correlated with conscientiousness and social desirability. Theoretically, these correlations make sense: concurrent detection requires the subject to spontaneously indicate that the score they are shown is different from the one they intended to report. Retrospective 2 detection was negatively correlated with sensation seeking. This finding is somewhat less intuitive to understand, but perhaps subjects high in sensation

seeking were more willing to accept the novel experience of a manipulated pain rating. Retrospective 3 detection was positively correlated with the fun-seeking subscale of the behavioral activation scale (Carver & White, 1994), and negatively correlated with openness to experience. Being a subject in the experimental group of a study could be considered the more interesting role, which might explain the correlation between fun-seeking and retrospective 3 detection. Subjects who were highly open to experience might have more readily integrated the misinformation into their memories, explaining the correlation between openness and retrospective 3 detection. Still, these analyses were all exploratory in nature, and future research should continue to evaluate the individual difference measures associated with each level of detection.

In sum, Study 1 found some support for all hypotheses. Only 11.1% of subjects concurrently detected the misinformation, and no more than 66.9% of subjects detected the misinformation by any measure used (Hypothesis 1). Subjects who were shown misinformation about their prior pain ratings later exhibited a greater reduction in their pain scores than control subjects (Hypothesis 2a), but this was not true for concurrent detectors, and somewhat attenuated for retrospective detectors (Hypothesis 2b). Subjects given misinformation about their pain ratings suggested marginally less payment for subjects in a similar future study (Hypothesis 3), although many of the measures of future willingness that were used were not correlated with how much pain subjects remembered.

## **Study 2**

### **Overview and hypotheses**

Study 1 investigated how people respond to misinformation about their own appraisals of their affective responses to a painful task, and whether this misinformation made people more

willing to complete similar painful tasks in the future. However, the results of Study 1 raise additional questions. While many people failed to detect misinformation about their prior pain ratings for a cold pressor task and subsequently remembered the task as less painful, does this finding generalize to misinformation for discrete emotions? Appraisal theories of emotion (Moors et al., 2013) characterize emotions as processes that can evolve and develop as goals shift, and emotion regulation strategies operate on the fact that emotions can be malleable (Gross, 2013). Because of this malleability, people might be persuaded by misinformation about their own emotional responses to events (Leding, 2012).

Another important question raised by Study 1 concerns the downstream consequences of false memories. While subjects in Study 1 weren't significantly more willing to participate in a cold pressor study in the future, other consequences of false memories could be investigated. For example, in a domain in which performance is important, could misinformation about prior appraisals of an event influence performance on a similar subsequent event? This question is reminiscent of Brooks' (2014) study demonstrating that reappraising anxiety as excitement can lead to enhanced performance on a variety of tasks. The present study pushes these findings one step further by asking not whether reappraising anticipatory anxiety can influence performance, but whether manipulating prior memories of responsive anxiety can influence performance. Another potential consequence of misinformation for prior affective ratings is a disparity in appraisals for future events. People who remember or believe that they experienced less anxiety and more excitement on a prior similar task might experience less anxiety and more excitement on a subsequent task (Pezdek & Salim, 2011). Thus, misinformation could enhance the subjective emotions people feel in response to events.

In Study 2, subjects completed a TSST (Kudielka et al., 2007). Immediately following the TSST, subjects rated the anxiety and excitement that they felt during the task. After some filler tasks lasting approximately 30 minutes, subjects were reminded of their prior emotion ratings and asked to elaborate on them. Subjects in the control group were shown their true prior ratings, while subjects in the misinformation group were shown anxiety ratings that were reduced by 20 points and excitement ratings that were increased by 20 points. After another retention interval lasting approximately 15 minutes, subjects were asked to recall the anxiety and excitement that they felt during the first TSST. Then, subjects completed a second TSST, and reported how much anxiety and excitement they felt during the task. Finally, subjects completed a funneled debriefing.

*Hypothesis 1:* a large proportion of subjects will be blind to the manipulation.

*Hypothesis 2a:* subjects who receive misinformation about how much anxiety and excitement they experienced will remember feeling more excitement and less anxiety than subjects who experience no misinformation.

*Hypothesis 2b:* subjects who are blind to the misinformation will exhibit the largest misinformation effect, whereas subjects who are not blind will not differ greatly from control subjects.

*Hypothesis 3:* subjects who receive (and are blind to) misinformation about how much anxiety and excitement they experienced will perform better on the second TSST.

*Hypothesis 4:* subjects who receive (and are blind to) misinformation about how much anxiety and excitement they experienced will experience more excitement and less anxiety on the second TSST.

*Rationale:* Previous research on choice blindness has found that people are often blind to changes in their own reports, even when those reports are highly self-relevant (*Hypothesis 1*). Research on the misinformation effect has found that people often incorporate misleading post-event information into their memories (*Hypothesis 2a*), but that people who detect the discrepancy between the misinformation and the actual event are less susceptible to this memory distortion (*Hypothesis 2b*). People make predictions about how they will feel in novel situations based on how they have felt in similar situation in the past, and feeling more excitement and less anxiety has been shown to enhance performance on a variety of tasks (*Hypothesis 3*). Finally, a more positive expectation about an event and a more positive performance on a task should lead to a more positive affective response (*Hypothesis 4*).

## **Method**

**Subjects.** One hundred fourteen eligible subjects were recruited for participation. Four subjects did not have time to complete the study procedures, three subjects had missing data, and 13 subjects withdrew from the study or asked that their data be withheld from analyses. The final sample included 94 subjects. The ethnic breakdown was 47.9% Asian/Pacific Islander, 27.7% Hispanic/Latino, 8.5% White, 3.2% Black, 8.5% biracial/multiracial, and 4% other. The mean age was 21.0.

## **Measures.**

***Emotion ratings.*** Subjects rated how much sadness, anxiety, happiness, and excitement they experienced during the TSST on a sliding scale anchored at 0 (none) and 100 (the most imaginable) (see Appendix H). Subjects used a similar scale to report how much sadness, anxiety, happiness, and excitement they recalled experiencing during the task (see Appendix J).

Sadness and happiness ratings were not manipulated for any subjects, but were included as filler items to reduce subject suspicion of the misinformation.

***Misinformation.*** Roughly 25 minutes after reporting on their emotional responses to the TSST, subjects were exposed to the critical manipulation (see Appendix I). Subjects were reminded of their prior emotion ratings, and were asked to elaborate on why they rated the task the way that they did. Subjects in the control group were shown their true prior emotion ratings, whereas subjects in the misinformation group were shown their prior anxiety rating minus 20 points, and their prior excitement rating plus 20 points.<sup>7</sup>

***Performance.*** Performance on the TSST was coded by independent evaluators. Evaluators were trained on pilot data by coding with the lead researcher, independently, and with each other. After training, the five evaluators all coded the first 19 videos to estimate inter-rater reliability. Once those videos had been coded, the remaining videos were coded by two independent evaluators, whose scores were averaged to create a single score for each subject on each dimension. In order to ensure that the evaluators were as unbiased as possible, evaluators did not code any videos of subjects that they had run through the study.

Performance on the TSST was evaluated on a number of dimensions adapted from Brooks (2014) (see Appendix L). On 7-point scales, coders evaluated how persuasive (“the speaker was persuasive,” “the speaker deserves the scholarship/leadership role”), confident (“the speaker was confident,” “the speaker was self-assured”), anxious (“the speaker was anxious”), excited (“the speaker was excited”), competent (“the speaker was intelligent,” “the speaker knew what s/he was talking about,” “the speech made sense”), and persistent (“the speaker was persistent”) the speaker was, and then rated the speaker’s overall performance. These measures

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<sup>7</sup> The scale subjects reported their pain on ranged from 0 to 100, but due to a clerical error, the first 41 subjects received a critical manipulation that read “1 to 100”. This error was discovered and corrected for the remaining subjects.

exhibited high within-coder reliability (see Table 2.3), and were therefore combined into a single index of speech quality. The coders also evaluated subjects on the arithmetic portion of the TSST as a more objective measure of performance. The coders recorded the lowest number subjects reached on their first attempt at the arithmetic task, and the lowest number they reached throughout the entire arithmetic task. The coders also rated the subjects' overall performance for the math task on a 7-point scale.

***Detection of misinformation.*** Four measures were devised to assess whether subjects detected the misinformation. To assess concurrent detection, subjects' free responses to the misinformation questions were evaluated by three independent condition-blind coders for evidence of detection. Subjects were labeled as detectors if two coders agreed they had detected the misinformation to either the anxiety item or the excitement item. Retrospective detection was coded by 2 of 5 condition-blind raters. To assess retrospective detection, subjects' free responses to three of the debriefing prompts were evaluated (see Appendix K for the full debriefing prompts). The first prompt (Retrospective 1 Detection) was "Did you find any part of the study strange or surprising during your session today?" Subjects were coded as detectors if they mentioned anything about how the pain rating they were told they made was not accurate. The second prompt (Retrospective 2 Detection) was "the computer reminded you of [your anxiety and excitement] ratings. Did you notice anything strange about this process?" Subjects were coded as detectors if they mentioned that they found something strange unless what they found strange was irrelevant to the study's hypotheses. The final prompt (Retrospective 3 Detection) was delivered after explaining to subjects the nature of the study, and asked, "Do you think you were in the group that was given the different emotion ratings?" This measure was intended to be the broadest estimate of detection, so subjects were coded as detectors if they responded "yes,"



“probably,” and even “maybe.” They were coded as non-detectors if they responded “no,” or “I don’t think so.” Subjects who responded “I don’t know” were initially coded into a third category, but were later recoded as non-detectors.<sup>8</sup>

The coding process for Study 2 was modified slightly from the process for Study 1. The coders assessed detection for each subject after coding their performance on the two tasks. This modification was not expected to influence the coding. However, in Study 1, the coders evaluated each level of detection independently from other levels of detection. In Study 2, the coders evaluated each level of detection sequentially, and thus were privy to how subjects had responded on prior detection measures. This may have influenced the coders’ responses, however, the coding instructions were specific to each debriefing question, which should have minimized the influence of this prior information on the coders’ evaluations. Disagreements between the coders on whether the subject was a detector or not were resolved by discussion.

### **Procedure**

The procedure for Study 2 is outlined in Figure 2.1. Subjects were recruited online via SONA systems for a study lasting 90 minutes. The study was advertised as an experiment on skills and motivation in order to reduce participant suspicion of the study’s main hypotheses. When subjects arrived in the lab, they first reviewed a document describing the types of tasks they would be asked to complete. If they agreed to participate in the study, they began the study procedures. First, subjects completed baseline measures of overall mood (POMS; McNair, Lorr, & Droppleman, 1971), anxiety (STAI; Spielberger et al., 1983), and depression (CES-D; Radloff, 1977). These measures acted both as filler tasks and as potential constructs that could relate to the detection of the misinformation.

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<sup>8</sup> To measure retrospective detection and TSST speech quality, all responses were coded by 2 of 5 coders, except for the 19 subjects who were used to establish reliability, who were coded by all 5 coders, and 1 subject who was coded by 3 coders.

Next, subjects began the first TSST. The TSST is a standardized laboratory task that has been shown to induce stress in experimental subjects. The present study used a modified procedure. Subjects were told that they had two minutes to prepare a five minute speech about why they were ideal candidates for a scholarship (or, in the second TSST, for a leadership position at their workplace). Subjects were given a pen and paper during this period to write out preparations for their speech. After two minutes, the pen and paper were removed, and subjects were asked to deliver their speech to a panel of two judges. The judges wore lab coats, and were trained to remain completely neutral and unresponsive to the subject. The judges were either both female or one male and one female<sup>9</sup>. As the subjects delivered their speeches, the evaluators interrupted them every 45 seconds with distracting feedback such as “please refrain from hand gestures.” This speech was recorded, which may have contributed to the stress induced by the task, and allowed for coders to evaluate the quality of the speech. After five minutes, the evaluators abruptly ended the task and instructed the subject to begin a second task by counting backwards from 1022 by intervals of 13 (or, for the second TSST, intervals of 17). Whenever subjects made a mistake or took too long between numbers, the evaluators instructed them to begin again at 1022. The evaluators continued to interrupt subjects every 45 seconds with distracting feedback such as “please go as fast as possible”. After five minutes, the evaluators ended the task and instructed the subject to continue with the survey.

Immediately after the first TSST, subjects were asked to rate their sadness, anxiety, happiness, and excitement in response to the task. These ratings were made on 100-point scales (see Appendix H). Of primary interest were the subjects’ ratings of their anxiety and excitement. Following these ratings, subjects began a retention interval similar to the retention intervals in Study 1, which lasted approximately 30 minutes. Subjects completed a variety of questionnaires,

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<sup>9</sup> One subject had a panel of two male judges.

including the POMS (McNair, Lorr, & Droppleman, 1971), the BSS (Hoyle et al., 2002), the BFI (John & Srivastava, 1999), the 12-Item Grit Scale (Duckworth et al., 2007), the PSS (Cohen et al., 1983), the LOT-R, (Scheier et al., 1994), the AVI (Tsai et al., 2006), and the BIS/BAS (Carver & White, 1994), which helped to lengthen the interval between the initial ratings and the misinformation.

Next, subjects entered the misinformation phase of the experiment (see Appendix I). Subjects in the control group were given their initial ratings of their sadness, anxiety, happiness, and excitement, and asked to elaborate on those ratings in a free-response format. Subjects in the misinformation group were asked the same questions, but the ratings shown were manipulated: their rating of anxiety was decreased by 20 points, and their rating of excitement was increased by 20 points. Following this procedure, participants entered a second retention interval during which they completed more personality questionnaires, including the RSES (Rosenberg, 1965), the SDS, (Crowne & Marlowe, 1960), the Anxiety Sensitivity Inventory (Reiss et al., 1986), and the Balanced Inventory of Desired Responding (BIDR, Paulhus, 1991).

Subjects were then tested on their memories for their sadness, anxiety, happiness, and excitement in response to the first TSST (see Appendix J). Following this test, they began the second TSST. The second TSST was identical to the first except in two ways. For the speech task, subjects were asked to prepare a speech about why they qualify for a leadership position at their workplace. This prompt was designed to be similar to the first TSST so that subjects' emotional responses to the first TSST were relevant to their mindset leading up to the second TSST. However, this task was designed not to be identical to the first TSST so that subjects do not simply recite the same speech they did for the first TSST. The second way in which the second TSST differed from the first was that in the arithmetic portion, subjects were asked to

count backwards from 1022 by intervals of 17. This modification was made for similar reasons: subjects still had to perform mental arithmetic under stressful conditions, but by instructing them to count backwards by intervals of 17, practice effects from the first TSST should have been mitigated. This TSST was also videotaped, which may have increased the stress induced by the task, and allowed coders to evaluate the quality of the speech for a comparison with the first speech.

Immediately after the second TSST, subjects rated their sadness, anxiety, happiness, and excitement in response to the task. This allowed for a test to see whether misinformation about past emotional responses would influence subsequent emotional responses. Subjects then completed a final POMS (McNair et al., 1971), and a demographic questionnaire. Finally, subjects were debriefed using a funneled debriefing procedure similar to that of Study 1 (see Appendix K).

## Results

**Detection:** To assess the reliability of the coders, reliability statistics were calculated for each of the measures of detection. As can be seen in Table 2.1, reliability was acceptably high. For retrospective 1, the coders agreed unanimously for 18/19 (94.7%) of responses. For retrospective 2, the coders agreed unanimously for 14/19 (73.7%) of responses.<sup>10</sup> For retrospective 3, the coders agreed unanimously for 13/19 (68.4%) of responses.<sup>11</sup>

Table 2.4 displays the rates of detection across different measures. For concurrent detection, subjects had two opportunities to detect: in response to the misinformation about their

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<sup>10</sup> This proportion takes into account one response that had missing data. In Table 2.1, that response is excluded in order to compute Krippendorff's alpha.

<sup>11</sup> For coding retrospective 3 detection, the coders had three responses: detector, non-detector, or that the participant had said something like "I don't know." Because saying "I don't know" does not give any evidence that the subject may have detected the manipulation, those ratings were re-coded as non-detectors prior to calculating the reliability statistics.

anxiety scores, and in response to the misinformation about their excitement scores. No subject detected the misinformation about their excitement scores. Of the 46 subjects who received misinformation about their anxiety scores, 4 (8.7%) detected it concurrently. For retrospective 1, no subject was coded as a detector. For retrospective 2, 10 subjects (22.2%) were coded as detectors. For retrospective 3, 16 subjects (37.2%) were coded as detectors.

In order to estimate the bias of the detection measures, the coders also evaluated the responses of control subjects. Of the 48 control subjects, 1 (2.1%) was coded as a concurrent detector, suggesting that this measure has a low false-positive rate. For retrospective 1 detection, 0 were coded as detectors. For retrospective 2 detection, 3 subjects (6.4%) were coded as detectors. For retrospective 3 detection, 10 subjects (21.3%) were coded as detectors. These percentages represent estimates of the bias of the detection measures, but they do not imply that control subjects actually detected the misinformation, as control subjects were never given any misinformation.

Considering the proportions of detectors for each measure and between the experimental and control groups, and considering the same proportions with regards to Study 1, one composite measure of detection was created for the analyses for Study 2. In Study 1, the same pattern of results emerged when examining retrospective 2 detection and retrospective 3 detection. However, the proportion of control subjects coded as retrospective 3 detectors suggests that this measure is too broad. Therefore, for Study 2, subjects were considered detectors if they were coded as detectors for concurrent detection, retrospective 1 detection, or retrospective 2 detection (see Table 2.4). By this measure, 11/46 (23.9%) subjects in the misinformation group and 4/48

(8.3%) subjects in the control group were coded as detectors. In subsequent analyses, it is only detectors in the misinformation group who are considered detectors.<sup>12</sup>

**TSST Coding Reliability:** To assess the reliability of the coders, all five coders evaluated the first 19 videos. Intraclass correlations were calculated for each dimension the coders evaluated. Results can be seen in Table 2.2. As can be seen, reliability was generally high.<sup>13</sup> In addition, following Landers (2015), when the dimensions were combined within each coder and reliability was assessed for the total scale, reliability was also high (see Table 2.3). Because the dimensions exhibited high reliability with each other within coders, this was the method used for subsequent analyses. In other words, speech quality was assessed by averaging the 11 dimensions (see Appendix L) for each coder for each subject, and then averaging the coders' composite scores for each subject.

The coders also recorded the lowest number subjects reached on their first attempt in the arithmetic portions of the TSST, and the lowest number subjects reached overall in the arithmetic portions. For subjects' first attempts, all five coders agreed perfectly for 16 (84%) responses.<sup>14</sup> For subjects' overall best attempts, all five coders agreed perfectly for 15 (79%) responses.<sup>15</sup>

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<sup>12</sup> Two subjects were missing data from the retrospective detection variables due to misunderstandings between the research assistants and the subjects or errors on the part of the research assistants. One of these subjects was in the control group, and thus their detection status was not relevant to subsequent analyses. The other subject was in the misinformation group, and was not coded as a concurrent detector, so they were coded as a non-detector for subsequent analyses.

<sup>13</sup> Single measures ICCs for consistency failed to reach the .6 threshold for three of the constructs coders evaluated: that the speaker deserved the scholarship, excitement, and anxiety. However, average measures ICCs for consistency for these constructs was acceptably high. Both of these measurements may be misleading in assessing the reliability of the measurement because for all but the first 19 subjects, the ratings of two coders were averaged together. Results are presented for these constructs with the caution that they be interpreted with this in mind.

<sup>14</sup> For one disagreement, one coder's response was one interval different from the other 4 coders' responses. A difference of one interval may be due to the way the measure was devised. The measure asked what the lowest number a subject reached before making their first mistake was, but could be misinterpreted to mean "what number was the subject attempting to reach when they made their first mistake."

<sup>15</sup> For one disagreement, one coder's response was one interval different from the other 4 coders' responses. For a second disagreement, the subject successfully counted backwards by intervals of 13 all the way to 0, and the coders all reported this, but in slightly different ways. For a third disagreement, 2 coders reported ambiguity in the lowest number achieved. When the potential numbers they reported were averaged, all coders agreed perfectly.

### **Distortion in Memory for the TSST**

To determine if the misinformation caused subjects to remember experiencing less anxiety and more excitement in the first TSST, two 2 (condition: control vs misinformation) by 2 (rating type: experience vs recall) mixed ANOVAs were conducted. The results for the ANOVA for anxiety can be found in Figure 2.2. For anxiety, a significant effect was found for rating type,  $F(1, 92) = 33.22, p < .001, \eta_p^2 = .27$ . No significant between-subjects effect for condition was found,  $p = .81$ . This main effect was qualified by a marginally significant interaction between rating type and condition,  $F(1, 92) = 3.90, p = .051, \eta_p^2 = .04$ . To examine the nature of this interaction, follow-up *t*-tests were conducted. There was no significant difference in anxiety experienced between the control group ( $M = 59.56, SD = 28.11$ ) and the misinformation group ( $M = 63.93, SD = 32.20$ ),  $p = .48$ . There was also no significant difference in anxiety recalled between the control group ( $M = 54.08, SD = 30.47$ ) and the misinformation group ( $M = 52.74, SD = 30.98$ ),  $p = .83$ . Subjects significantly reduced their anxiety ratings between the two time points in both the control group (paired  $t(47) = 2.42, p = .019$ ) and the misinformation group (paired  $t(45) = 6.30, p < .001$ ). Thus subjects in both groups reduced their anxiety rating, but this reduction was marginally greater in the misinformation group than in the control group.

The results of the ANOVA for excitement are shown in Figure 2.4. For excitement, a significant effect was found for rating type,  $F(1, 92) = 5.86, p = .017, \eta_p^2 = .06$ . No significant between-subjects effect was found for condition,  $p = .20$ . This main effect was qualified by a significant interaction between rating type and condition,  $F(1, 92) = 4.64, p = .034, \eta_p^2 = .05$ . To examine the nature of this interaction, follow-up *t*-tests were conducted. There was no significant difference in excitement experienced between the control group ( $M = 14.73, SD = 21.13$ ) and the misinformation group ( $M = 15.87, SD = 20.98$ ),  $p = .79$ . However, there was a significant

difference in excitement recalled between the control group ( $M = 15.23$ ,  $SD = 21.68$ ) and the misinformation group ( $M = 24.46$ ,  $SD = 21.91$ ),  $t(92) = 2.05$ ,  $p = .043$ . Subjects in the control group did not significantly increase their excitement ratings between the two time points,  $p = .88$ , while subjects in the misinformation group did significantly increase their excitement ratings, paired  $t(45) = 5.98$ ,  $p < .001$ . Thus subjects in the misinformation group recalled more excitement than they experienced, whereas subjects in the control group did not.

To determine if detection of the misinformation influenced subjects' memories of their anxiety and excitement, two 3 (condition: controls vs detectors vs non-detectors) by 2 (rating type: experienced vs recalled) mixed ANOVAs were conducted. The results of the ANOVA for anxiety are shown in Figure 2.3. For anxiety, a significant effect for rating type was found,  $F(1, 91) = 20.11$ ,  $p < .001$ ,  $\eta_p^2 = .18$ . No significant between-subjects effect for condition was found,  $p = .31$ . This main effect was qualified by a significant interaction,  $F(2, 91) = 3.72$ ,  $p = .028$ ,  $\eta_p^2 = .08$ . These findings were also qualified by a significant Levene's test for equality of error variances for experienced anxiety ratings,  $F(2, 91) = 6.18$ ,  $p = .003$ . To examine the nature of this interaction, follow-up tests were conducted. There were no significant differences in anxiety experienced between the controls ( $M = 59.56$ ,  $SD = 28.11$ ), non-detectors ( $M = 68.69$ ,  $SD = 27.34$ ), and detectors ( $M = 48.82$ ,  $SD = 42.34$ ), Welch statistic (2, 25.57) = 1.69,  $p = .21$ . Nor were there differences between in anxiety recalled between the controls ( $M = 54.08$ ,  $SD = 30.47$ ), non-detectors ( $M = 55.37$ ,  $SD = 28.48$ ), and detectors ( $M = 44.36$ ,  $SD = 38.22$ ),  $F(2, 91) = .56$ ,  $p = .57$ . The significant ANOVA interaction indicates a significant difference between the groups in how much they reduced their anxiety ratings. There was a significant difference between the controls and the non-detectors,  $p = .01$ , but no significant difference between the controls and the detectors,  $p = .83$ , or the detectors and the non-detectors,  $p = .07$ . In addition, there was a



significant reduction in anxiety ratings for the controls, paired  $t(47) = 2.42, p = .02$ , and the non-detectors, paired  $t(34) = 6.59, p < .001$ , but not the detectors, paired  $t(10) = 1.46, p = .175$ . Thus the non-detectors exhibited a reduction in their anxiety ratings, and this reduction was significantly greater than the control group, but not the detectors. The reductions exhibited by controls and detectors were not significantly different from each other.

The results of the ANOVA for excitement can be found in Figure 2.5. For excitement, a significant effect for rating type was found,  $F(1, 91) = 4.00, p = .048, \eta_p^2 = .04$ . No significant between-subjects effect was found for condition,  $p = .31$ . This main effect was qualified by a significant interaction,  $F(2, 91) = 3.11, p = .049, \eta_p^2 = .06$ . To examine the nature of this interaction, follow-up tests were conducted. There was no significant difference in excitement experienced between the controls ( $M = 14.73, SD = 21.13$ ), the non-detectors ( $M = 13.60, SD = 16.65$ ), and the detectors ( $M = 23.09, SD = 31.01$ ),  $F(2, 91) = .89, p = .41$ . Nor was there a significant difference in excitement recalled between the controls ( $M = 15.23, SD = 21.68$ ), non-detectors ( $M = 24.06, SD = 19.07$ ), and the detectors ( $M = 25.73, SD = 30.35$ ),  $F(2, 91) = 2.12, p = .13$ . However, there was a significant difference in the amount the three groups increased their excitement ratings,  $F(2, 91) = 3.11, p = .049, \eta_p^2 = .06$ . Follow-up tests indicated a significant difference between the controls and the non-detectors,  $p = .02$ , but no significant difference between the controls and the detectors,  $p = .73$ , or the detectors and the non-detectors,  $p = .22$ . Thus, while the non-detectors exhibited a greater positive bias in their recall for their excitement than controls, this bias was not greater than the detectors', and the biases of the controls and detectors did not significantly differ.

## Appraisals of the second TSST

To determine if the misinformation caused subjects to have a more positive appraisal of the second TSST, two 2 (condition: control vs misinformation) by 2 (time: first TSST vs second TSST) mixed ANOVAs were conducted. The results of the ANOVA for anxiety are displayed in Figure 2.6. For anxiety ratings, a significant effect for time was found,  $F(1, 92) = 132.62, p < .001, \eta_p^2 = .59$ . No significant between-subjects effect for condition was found,  $p = .95$ . This main effect was qualified by a significant interaction between time and condition,  $F(1, 92) = 5.27, p = .024, \eta_p^2 = .05$ . To examine the nature of this interaction, follow-up tests were conducted. There was no significant difference between the groups for anxiety experienced for the first TSST (see above). For anxiety experienced for the second TSST, there was no significant difference between the control group ( $M = 40.38, SD = 31.92$ ) and the misinformation group ( $M = 35.20, SD = 28.78$ ),  $p = .41$ . For the control group, there was a significant difference in the anxiety experienced after each TSST, paired  $t(47) = 7.30, p < .001$ . This was also true for the misinformation group, paired  $t(45) = 8.85, p < .001$ . Thus, both groups reported lower anxiety after the second TSST than after the first, and this reduction was greater for the misinformation group than for the control group. When the same analysis was conducted for excitement ratings, no significant effects were found, all  $ps > .12$  (see Figure 2.8).

These analyses were also replicated distinguishing between detectors and non-detectors. To examine the role of detection, two 3 (condition: control, detectors, non-detectors) by 2 (time: first TSST vs second TSST) ANOVAs were conducted. The results for anxiety are displayed in Figure 2.7. For anxiety, a significant main effect for time was found,  $F(1, 91) = 89.79, p < .001, \eta_p^2 = .50$ . No significant between-subjects effect for condition was found,  $p = .34$ . This main effect was qualified by a significant interaction,  $F(2, 91) = 3.89, p = .02, \eta_p^2 = .08$ . These findings

were also qualified by a significant Levene's test for equality of error variances for anxiety ratings for the first TSST,  $F(2, 91) = 6.18, p = .003$ . To examine the nature of this interaction, follow-up tests were conducted. There was no significant difference between the groups in anxiety experienced for the first TSST (see above). Nor was there a significant difference between the controls ( $M = 40.38, SD = 31.92$ ), non-detectors ( $M = 37.37, SD = 29.10$ ), and detectors ( $M = 28.27, SD = 27.90$ ) in anxiety experienced for the second TSST,  $F(2, 91) = .71, p = .49$ . The significant ANOVA interaction indicates a significant difference between the groups in how much they reduced their anxiety ratings. The difference between the controls and the non-detectors was significant,  $p = .008$ . However, the differences between the controls and the detectors ( $p = .84$ ) and between the detectors and the non-detectors ( $p = .123$ ) were not statistically significant. Thus, while subjects generally exhibited lower anxiety ratings for the second TSST than the first, this reduction was significantly greater for the non-detectors than the controls.

A similar 2x3 ANOVA was conducted for excitement, the results of which are shown in Figure 2.9. No significant effect was found for time,  $p = .73$ . There was also no significant between-subjects effect for condition,  $p = .50$ . However, a significant interaction between time and detection was found,  $F(2, 91) = 3.31, p = .041, \eta_p^2 = .07$ . To examine the nature of this interaction, follow-up tests were conducted. There was no significant difference in excitement experienced on the first TSST between the conditions,  $p = .41$  (see above). There was also no significant difference in excitement experienced on the second TSST between the controls ( $M = 13.85, SD = 20.37$ ), the non-detectors ( $M = 21.89, SD = 20.27$ ), and the detectors ( $M = 18.09, SD = 19.22$ ),  $F(2, 91) = 1.61, p = .21$ . The significant ANOVA interaction indicates a significant difference in the amount subjects increased their excitement ratings between the groups. Follow-

up tests indicated a significant difference between the controls and the non-detectors,  $p = .03$ , and a significant difference between the detectors and the non-detectors,  $p = .04$ . However, there was no significant difference between the controls and the detectors,  $p = .51$ . In addition, the non-detectors increased their excitement ratings between the two TSSTs, paired  $t(34) = 2.87$ ,  $p = .01$ . However, no differences in excitement ratings between the two TSSTs were found for controls ( $p = .75$ ) or detectors ( $p = .46$ ). In sum, whereas neither the controls nor the detectors changed their excitement ratings between the two TSSTs, non-detectors significantly increased their excitement ratings.

### **Performance on the second TSST**

To determine if misinformation for subjects' appraisals of the first TSST influenced performance on the second TSST, three 2 (condition: misinformation vs control) by 2 (time: first vs second TSST) were conducted. For the speech portion, a significant effect for time was found,  $F(1, 90) = 18.85$ ,  $\eta_p^2 = .17$ . No other significant effects were found, all  $ps > .85$ . Subjects in both conditions improved their performance from the first TSST ( $M = 4.16$ ,  $SD = 1.05$ ) to the second ( $M = 4.48$ ,  $SD = 1.09$ ). For the arithmetic portion, two measures were evaluated. For the coders' subjective assessments of the subjects' performance, a significant effect of time was found,  $F(1, 91) = 7.59$ ,  $p = .01$ ,  $\eta_p^2 = .08$ . No other significant effects were found, all  $ps > .14$ . Subjects performed better on the second task than the first. A second measure of the quality of the arithmetic task was how many correct responses subjects recorded prior to making a mistake on their best attempt throughout the task. A 2 by 2 ANOVA examining this measure was conducted, but no significant effects were found, all  $ps > .06$ .<sup>16</sup>

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<sup>16</sup> Another measure of the quality of the arithmetic task was devised: how many correct responses subjects recorded prior to making a mistake on their first attempt for each task. However, in the first task, 71% of subjects recorded 0 or 1 correct responses before making a mistake on their first attempt, and in the second task, that proportion was

Changes in performance were also examined between controls, non-detectors, and detectors. For the speech portion, a 3 (condition: controls, non-detectors, detectors) by 2 (time: first TSST or second) ANOVA was conducted. A main effect was found for time,  $F(1, 89) = 10.23, p = .002, \eta_p^2 = .10$ . A significant between-subjects main effect was also found for condition,  $F(2, 89) = 4.03, p = .021, \eta_p^2 = .08$ . No significant interaction effect was found,  $p = .87$ . Subjects' performance generally improved from the first TSST ( $M = 4.16, SD = 1.06$ ), to the second TSST ( $M = 4.48, SD = 1.09$ ). In addition, detectors (adjusted  $M = 3.62, SE = .31$ ) performed worse than non-detectors (adjusted  $M = 4.61, SE = .17$ )  $p = .007$ , and marginally worse than controls (adjusted  $M = 4.27, SE = .14$ ),  $p = .064$ , while controls and non-detectors did not significantly differ from each other,  $p = .12$ . In examining performance on the arithmetic task as rated by the coders, a significant effect for time was found,  $F(1, 90) = 5.88, p = .017, \eta_p^2 = .06$ . No other significant effects were found, all  $ps > .27$ . Subjects performed better on the second task than on the first. In examining the number of correct responses before making a mistake, no significant effects were found, all  $ps > .23$ . In sum, while subjects' performance on the TSST generally improved between the first task and the second, this improvement was moderated neither by exposure to misinformation about their prior emotion ratings, nor by a failure to detect such misinformation.

## Discussion

The results from Study 2 demonstrate that misinformation for *specific emotions* can influence peoples' subsequent memories for a stressful task, and their future appraisals of a similar future task. When exposed to misinformation about their prior anxiety and excitement ratings, 76.1% of subjects failed to detect this misinformation (Hypothesis 1). In addition, these

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58.1%. Because of this pattern, analyses on this measure were not conducted. This potential floor effect is discussed further in the discussion for Study 2.

subjects later exhibited a greater positive memory bias (i.e., their memories for anxiety and excitement changed more) than controls not exposed to misinformation, although for anxiety, this difference was only marginally significant (Hypothesis 2a). As predicted by the discrepancy detection principle (Tousignant et al., 1986), this positive memory bias was greatest for subjects who failed to detect the misinformation, whereas detectors did not differ significantly from either controls or non-detectors (Hypothesis 2b). While subjects overall performed better on the second TSST by several measures, subjects exposed to misinformation did not improve their performance more than control subjects, and subjects who failed to detect the misinformation did not improve their performance more than controls or detectors (Hypothesis 3). Finally, subjects who received misinformation about their anxiety ratings for the first TSST exhibited a greater reduction in their anxiety ratings for the second TSST than controls. In particular, subjects who failed to detect the misinformation exhibited the greatest reduction in their anxiety ratings. Furthermore, only non-detectors exhibited an increase in their excitement ratings between the two TSSTs (Hypothesis 4).

The concurrent detection rate in Study 2 was surprisingly low. Compared to Study 1, subjects in Study 2 received a second exposure to misinformation, which constituted a second opportunity to detect the discrepancy. However, no subjects concurrently detected the excitement manipulation. The detection rates for the different measures of retrospective detection were also somewhat lower. One potential explanation for this finding concerns the appraisal questions subjects in both studies were asked. In both studies, subjects were asked four appraisal questions regarding the tasks they completed, but in Study 1, subjects were only reminded of their prior pain rating. By contrast, in Study 2, subjects were reminded of all four ratings. This modification was made to make the study procedures more consistent and to lure subjects into accepting the

misinformation, and it appears to have worked: subjects detected the misinformation at a relatively low rate.

Study 2 also demonstrated that misinformation about specific emotional appraisals for a stressful task can influence peoples' future memories for that task. Exposing subjects to misinformation about their prior emotion ratings influenced their subsequent memory for the TSST. These findings build on a wealth of literature concerning the malleability of memory (Loftus, 2005) and the malleability of affect (Moors et al., 2013). In particular, they generalize the memory blindness phenomenon (Cochran et al., 2016) to a new context: memories of emotional responses to events people have experienced.

Subjects who received misinformation about their emotional responses to the first TSST subsequently experienced less anxiety on the second TSST, and subjects who were blind to the misinformation exhibited the greatest reduction in anxiety and also exhibited an increase in excitement. These findings represent an important new contribution to the literature on choice blindness. Misinformation did not merely lead to a subsequent distortion in subjects' memories for the first TSST, but actually improved their appraisals for the second TSST. Building on prior literature (e.g., Merckelbach et al., 2011; Cochran et al., 2016; Johansson et al., 2013), these results demonstrate that choice blindness can have important downstream consequences: subjects blind to misinformation about their appraisals of the first TSST actually had an improved experience on the second TSST. These findings echo similar research that has shown that interventions designed to improve memory for an unpleasant event can lead to lower levels of distress for an analogous subsequent event (Chen et al., 1999). However, the present study specifically used misinformation to achieve this end, whereas past research has employed interventions to combat negative memory biases (Chen et al., 1999).

Despite this pattern of findings, the results of the present study did not indicate that exposure to misinformation for appraisals of the first TSST – or failure to detect it – led to improved performance on the second TSST. This is striking considering that subjects who failed to detect the misinformation subjectively reported improved levels of anxiety and excitement for the second TSST. One potential explanation for this null result is that performance on the TSST is difficult to measure. For the speech portion, a subjective measure of performance was employed, but ideally a more objective measurement could be designed. The arithmetic portion included two objective measures, but it is not clear that they were the optimal choices. The number of consecutive correct responses on subjects' initial and best attempts at the task is a reasonable measure of performance, but it doesn't take into account the total number of attempts subjects make, how quickly subjects reach each correct answer, or how the evaluators might moderate performance. For example, an interruption from the evaluators in the middle of a promising attempt is perhaps a greater obstacle than an interruption at the beginning of an attempt. Perhaps if performance were measured in a more objective, optimized way, the results would have been different. Another potential explanation for the null results with regard to performance is in the nature of the TSST. The task is intended as a stress induction (Kirschbaum et al., 1993), and is quite difficult. In fact, one measure of performance for the arithmetic portion was left out of analyses for Study 2 because of a potential floor effect. Perhaps differences in performance between the groups might have been found for a less difficult, but still anxiety-inducing task. Other researchers have developed more moderate versions of the TSST (Wiemers, Schoofs, & Wolf, 2013), and perhaps these tasks would be less difficult while still inducing moderate anxiety. However, evaluations of performance on these tasks might still be subjective. Future research investigating the influence of misinformation on subsequent performance quality



should consider the difficulty of the performance, and should choose a task with an objective method of grading performance.

## **General Discussion**

Two studies investigated the downstream consequences about misinformation for subjects' own appraisals of unpleasant tasks. In Study 1, subjects who received misinformation suggesting that they experienced less pain than they truly reported during a cold pressor task often failed to detect the discrepancy between their prior appraisal and the misinformation. Subjects who received misinformation subsequently remembered the task as less painful, especially if they failed to detect it. However, retrospective detectors still exhibited a greater change in memory than control subjects, though it was smaller than that of non-detectors. Despite this change in memory, subjects who received misinformation were only marginally more willing to participate in a similar study again in the future as measured by the amount of money they suggested future subjects be compensated for their participation. Similarly, in Study 2, subjects who received misinformation suggesting they experienced less anxiety and more excitement than they truly reported during a TSST often failed to detect the misinformation. Subjects exposed to misinformation later exhibited a greater positive memory bias for the first TSST than control subjects. Misinformation subjects also exhibited a greater reduction in their anxiety appraisals between the two TSSTs than control subjects, and non-detectors exhibited the greatest reduction in their anxiety ratings, and also an increase in their excitement ratings. However, misinformation about prior affective responses to the TSST did not lead to improved performance on the second TSST for either the speech portion or the arithmetic portion. In sum, two studies demonstrated that people often fail to notice alterations to their own reports of their appraisals for unpleasant tasks, that exposure to (and failure to notice) these alterations can

influence future memory for the tasks, and in Study 2, that exposure to (and failure to notice) these alterations can influence subjective appraisals for a subsequent unpleasant task.

In Study 1, only a marginally significant difference was found between the groups for future willingness to participate in a similar study. In addition, no differences were found between the groups in Study 2 in performance on the second TSST. These results could indicate that misinformation in this form has little influence on future behavior, even if it does influence memory. However, alternative explanations should also be considered. Subjects in Study 1 were only misinformed about their pain ratings for the cold pressor task, but this task represented 90 seconds out of an experiment lasting 90 minutes spanning two days. Subjects' willingness to participate in a future study may have been more closely related to the overall time commitment than to their memories of how painful the task was. Several other measures were devised to specifically assess subjects' willingness to complete only a cold pressor task in the future, but these measures were not correlated with how painful subjects recalled the task to be. Therefore, future research should devise a measure of willingness that is related to the amount of pain subjects remember. With regard to Study 2, subjects who were exposed to (or failed to detect) misinformation about their appraisals for a prior TSST did not improve in their performance on a subsequent TSST more than control subjects. However, the TSST is a difficult task, and potential floor effects were observed for one measure of performance. Moreover, several of the measures of performance employed in the present study were subjective, and the objective measures were themselves imperfect. Thus, future research should employ an anxiety-inducing task for which performance can be objectively and precisely measured.

## **Limitations and Future Directions**

The present studies are limited in a number of ways. They were both laboratory experiments employing undergraduates as research subjects. In addition to the biases inherent in using “WEIRD” samples (Henrich, Heine, & Norenzayan, 2010), laboratory experiments might create demand characteristics that field studies avoid. Specifically, many choice blindness studies are field experiments (e.g., Hall et al., 2010; Hall et al., 2012), the differences between how subjects behave in these two contexts could be especially important for measures of detection – subjects who know they are participating in an experiment may try to behave in a way consistent with their perception of the experiment’s goals. And indeed, in Study 1, social desirability was negatively related to concurrent detection. One factor that partially alleviates this worry is that both studies included multiple measures of detection, and retrospective detection was not significantly correlated with social desirability. Nevertheless, future research might attempt to design measures of detection that are less sensitive to demand characteristics. For instance, researchers might observe subjects’ facial expressions when they receive misinformation to determine if subjects appear confused or taken aback. Another unobtrusive measure might be the amount of time subjects take before responding, or the amount of time they use in their responses. Subjects might require additional time to generate explanations for choices or ratings that they didn’t truly make than those that they did.

Another limitation to the present studies involves the use of computer software in delivering misinformation. Early studies on choice blindness involved manipulations and apparatuses adapted from magicians (E.g., Hall et al., 2013; Hall et al., 2010; Johansson et al., 2005). When confronted with a choice that was not truly theirs, subjects in these experiments had few alternative explanations (Johansson, Hall, Gulz, Haake, & Watanabe, 2007). However, when

misinformation is delivered via computer, subjects might have a ready explanation: a computer error. Perhaps, then, in their responses to the misinformation questions, subjects weren't truly describing why they rated their pain, anxiety, or excitement the way the computer suggested they did, but rather simply reporting why they made the ratings that they did. However, this possibility seems unlikely because in their explanations, many non-detectors repeated the misinformation they were shown. In addition, when asked "did you find anything strange or surprising during the study," almost no subjects were coded as detectors, but if they had reported an error that the computer had made regarding their ratings, they would have been. A second limitation in using computers to deliver the misinformation is that it denies subjects the opportunity to have a conversation about their affect ratings. When interacting with another person, people have the opportunity to ask additional questions or seek elaboration, but when interacting with a computer, their choices are more limited.

An additional limitation to the present studies was the nature of the control conditions. Subjects in the misinformation conditions were given manipulated ratings of their pain, anxiety, and excitement. However, subjects in the control condition were given their true prior ratings. It is possible that this procedure acted as a rehearsal opportunity for subjects in the control condition and artificially reduced the amount that they changed their ratings. In other words, future research should employ a control condition that receives no feedback about their ratings in order to better isolate the effect of the misinformation. It should be noted, however, that another study of memory blindness has employed a no-feedback control, and found no differences in the effect of the misinformation between the no-feedback control group and the consistent feedback control group (Cochran et al., 2016, Study 2).

A number of alternative or moderating explanations for the present findings should be considered. Research has shown that when people believe information has been saved on a computer, they may be less likely to remember it (Sparrow, Liu, & Wegner, 2011). In both of the present studies, subjects reported their appraisals on computers. When presented with the misinformation, subjects may have been less likely to detect it because, confident that their ratings had been saved on the computer, they allowed their memories for their appraisals to diminish. Perhaps a stronger test of the present studies' hypotheses would be if subjects had reported their initial appraisals in a form that was unlikely to be saved. A future study might incorporate a computer that apparently is known to malfunction, and subjects might be asked to report whenever it does so that the experimenters can make a note of it. This could reduce subjects' confidence that their scores would be stored accurately, while simultaneously producing an environment in which subjects indicating that they detected the misinformation would be considered socially desirable.

A second potentially moderating explanation for the present findings concerns retrieval-enhanced suggestibility (RES; Thomas, Bulevich, & Chan, 2010). RES is the finding that after people recall the details of an event, they are subsequently more susceptible to misinformation about that event. In the present studies, perhaps reporting on their initial appraisals for the tasks increased subjects' likelihood of being lured by the misinformation. On the other hand, subjects reported their appraisals for the tasks immediately after the tasks concluded. While these types of ratings blur the line between experience and recall, subjects were asked about the "average" levels of pain, anxiety, and excitement for tasks that lasted between 90 seconds and 10 minutes, so it is likely they were recalling their prior feelings, even if those feelings only took place 90 seconds earlier. Unfortunately, it is difficult to conceive of a study of memory blindness that

does not involve subjects making any initial report in some way. One method that future research might employ is not to have subjects make an initial rating, but to nevertheless give them misinformation as though they had. This might be especially effective if subjects answer a variety of similar questions so that they are less likely to detect that they were never asked about the critical items. Another option could be to use false feedback approaches (e.g., Valins, 1966; Valins & Ray, 1967). Still, for the present studies, RES can't be ruled out as a potential influence.

A final alternative explanation for the present findings concerns the distinction between subjects' memories for the tasks and their memories for their prior ratings. When subjects were asked to recall how much pain, anxiety, and excitement they felt for the initial tasks, did they recall their feelings during the tasks, or did they instead recall their prior ratings? This is a concern for research on memory blindness, but the methodology of the present studies may have helped to mitigate its influence. In both studies, subjects made four ratings after the initial task, and were asked to recall those four ratings later on (pain, distress, positive emotion, and negative emotion in Study 1; sadness, anxiety, happiness, and excitement in Study 2). Subjects may have had a difficult time recalling their ratings for all four questions, so they may have been more likely instead to rely on their memories for the tasks (Hovasapian & Levine, 2015). In addition, the instructions for the memory questions asked subjects to "think back to how you felt during the cold water task" (Study 1) or informed them that "we are interested in how you recall [the TSST] now" (Study 2), so subjects should have been responding based on their memories for the tasks. However, the distinction between memories for the tasks and memories for the ratings may be less important than it seems. If subjects *believe* they made more positive ratings for the pain, anxiety, or excitement that they experienced than they truly did, or they *remember* doing so, that

fact itself should influence their appraisals of the tasks (Bem, 1972; Moors et al., 2013). In other words, the memory of rating the tasks as more pleasant might initiate a reappraisal process in a manner predicted by self-perception theory: subjects might interpret their observable behavior (their pain, anxiety, and excitement ratings) in order to infer how they felt during the tasks themselves.

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## Appendix A

### Screening Questions for Cold Pressor, Study 1

\* indicate questions that will result in exclusion if YES is indicated.

\*\* indicate questions that will result in exclusion if NO is indicated.

\*1. Please indicate if you have EVER been diagnosed with or currently have any of the following:

(Yes / No) A psychological disorder for which you are being treated (medication or therapy)?  
(e.g., clinical depression, anxiety disorder, bipolar disorder)

(Yes / No) Cardiovascular disease (e.g., a heart condition, hypertension)

(Yes / No) Chronic pain conditions (e.g., arthritis)

(Yes / No) Reynaud's disease or a similar blood vessel disorder and/or circulatory problems

(Yes / No) Do you have a condition or disorder causing you to faint frequently?

(Yes / No) Are you currently pregnant, or is there any possibility you may be pregnant?

(Yes / No) Do you have a disorder called cardiovascular syncope or neurocardiogenic syncope?

(Yes/No) Have you ever become faint/light headed, pale, dizzy, been told you had an extreme drop in heart rate or blood pressure, fainted, or had a seizure because of one of the following: seeing blood or something frightening, being exposed to cold temperature or hot temperature, laughing too hard, not eating, exercise, being surprised, or after suddenly changing positions.

(Yes / No) \*\*2. Is English your first language or are you very fluent in English?

(Yes / No) 3. \*Have you taken any pain medication today (e.g., Tylenol, Aspirin)?

4. Please list in the space below ALL medications (both prescribed and over the counter) that you are currently taking (e.g., birth control, antibiotics) and note which ones you have taken today. *\*Participants will be excluded if they regularly take mood altering medications (e.g., SSRIs), cardiovascular system altering medications, pain medications, or blood thinners (e.g. Aspirin):*

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5. When is the last time today that you had any caffeine (e.g., soda, coffee, energy drink) and how much did you consume when you did have some?

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6. When is the last time that you ate any food today? \_\_\_\_\_

7. FOR WOMEN ONLY:

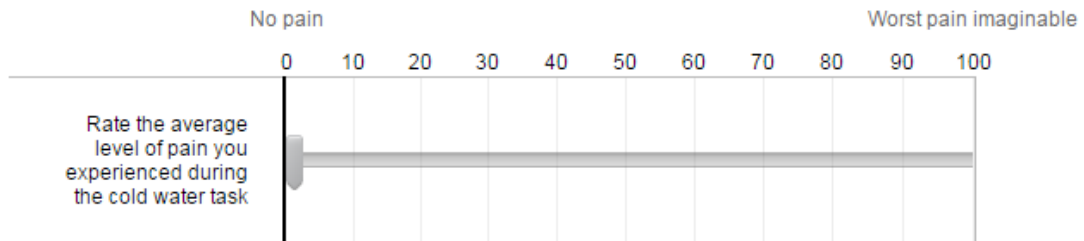
7a. When was your last menstrual period? \_\_\_\_\_  
(include day and month)

7b. Often, women's cycles are about 28 days, but this widely varies. Approximately how many days is your cycle? Please count the number of days between the first day of your last period to the first day of your most recent period. \_\_\_\_\_ days.

## Appendix B

### Initial Pain Rating, Study 1

Rate the average level of pain you experienced during the cold water task (0 to 100)



## Appendix C

### Choice Blindness Manipulation, Study 1

We are interested in understanding what the experience of putting your hand in cold water was like for you. Earlier in the study, on a scale from 1 to 100, you rated your pain as a [**X, X-20**].

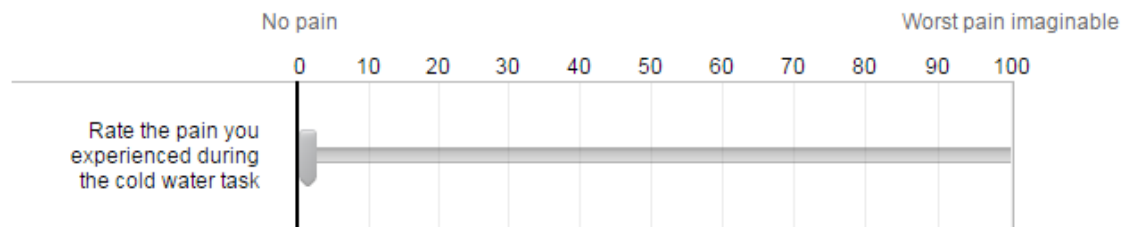
What made you rate the task in that way? Please be detailed in your response.

## Appendix D

### Pain Memory Question, Study 1

Think back to how you felt during the cold water task.

Rate the average level of pain you experienced during the cold water task (0 to 100)



## Appendix E

### Participant Feedback Questionnaire

You have received this questionnaire because you have recently completed a study involving a cold water task, and we would like to know your opinions about that task.

Recently, our lab has received a grant to use to run some similar studies. We want to run a new study with a cold water task just like the one you completed, and pay our research subjects for participating. The study will last 90 minutes over two sessions, and will involve the same cold water task you completed.

How willing would you be to participate in an experiment similar to this one in the future?

Not at all willing				Extremely willing
1	2	3	4	5

How much do you think we should pay our subjects?

\$\_\_\_\_\_

We greatly appreciate your feedback. Thank you for your participation and your opinions.

## Appendix F

### Participant Feedback Questionnaire [Revised]

You have received this questionnaire because you have recently completed a study involving a cold water task, and we would like to know your opinions about that task.

Recently, our lab has received a grant to use to run some similar studies. We want to run a new study with a cold water task just like the one you completed, and pay our research subjects money for participating instead of course credit. **The future experiment will include the same tasks you have completed for this experiment (e.g., questionnaires, cold water task) and will last approximately 90 minutes over 2 sessions.**

- 1) How willing would you be to do **JUST** the cold water task (like the one you did in this study) in a future study?

Not at all willing				Extremely willing
1	2	3	4	5

- 2) How much do you think we should pay our subjects **JUST** for completing the cold water task (and not for other study tasks)?

\$0	\$5	\$10	\$15	\$20	\$25	\$30	\$35	\$40	\$45	\$50	\$55	\$60
-----	-----	------	------	------	------	------	------	------	------	------	------	------

- 3) How willing would you be to participate in a whole experiment similar to this one in the future (*including* the cold water task and various questionnaires)?

Not at all willing				Extremely willing
1	2	3	4	5

- 4) How much do you think we should pay our subjects for participating in a whole experiment similar to this one in the future (*including* the cold water task and various questionnaires)?

\$0	\$5	\$10	\$15	\$20	\$25	\$30	\$35	\$40	\$45	\$50	\$55	\$60
-----	-----	------	------	------	------	------	------	------	------	------	------	------

We greatly appreciate your feedback. Thank you for your participation and your opinions.

## **Appendix G**

### **Funneled Debriefing, Study 1**

You have completed all the tasks of the study. First, what do you think the study was about?

[Record answer.]

Did you find any part of the study strange or surprising during either visit?

[Record answer.]

I'm sure you remember the cold-pressor task from the previous study session. Do you remember reporting how much pain you felt during the cold-pressor task on the computer? Later in the same session, the computer reminded you of this rating. Did you notice anything strange about this process?

[Record answer.]

Okay, thank you. The true purpose of this study was to better understand how different aspects of your memory for a painful experience would influence your willingness to undergo a future painful experience. During the first session of this study, you were asked to report why you rated your pain experienced in the cold-pressor task the way that you did. But for some subjects the pain rating we told you was yours was actually lower than what you had really said. I do not personally know whether your rating was changed by the computer or not. Do you think you were in the group that was given the lower pain rating?

[Record answer.]

Why or why not?

[Record answer.]



## Appendix H

### Emotion Questionnaire, Study 2

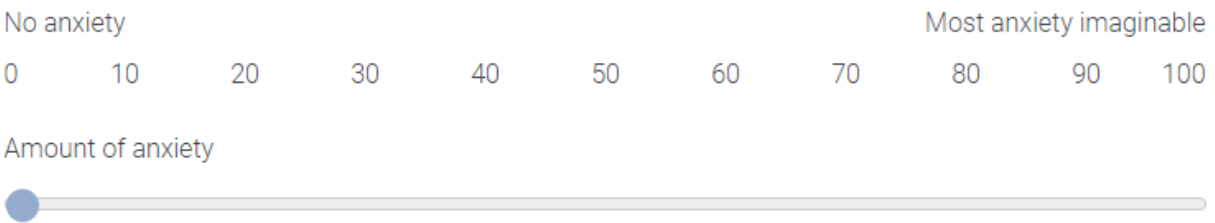
[Note: Questions were displayed on separate pages.]

Rate the average level of sadness you experienced during the speech and math tasks (0 to 100)



[Page Break]

Rate the average level of anxiety you experienced during the speech and math tasks (0 to 100)



[Page Break]

Rate the average level of happiness you experienced during the speech and math tasks (0 to 100)



[Page Break]

Rate the average level of excitement you experienced during the speech and math tasks (0 to 100)

No excitement

Most excitement imaginable

0 10 20 30 40 50 60 70 80 90 100

Amount of excitement



## Appendix I

### Choice Blindness Manipulation, Study 2

We are interested in understanding what the experience of the speech and math tasks were like for you.

Earlier in the study, on a scale from 0 to 100, you rated your sadness as a [X]. What made you rate the task in that way? Please be detailed in your response.

[Page Break]

We are interested in understanding what the experience of the speech and math tasks were like for you.

Earlier in the study, on a scale from 0 to 100, you rated your anxiety as a [X, X-20]. What made you rate the task in that way? Please be detailed in your response.

[Page Break]

We are interested in understanding what the experience of the speech and math tasks were like for you.

Earlier in the study, on a scale from 0 to 100, you rated your happiness as a [X]. What made you rate the task in that way? Please be detailed in your response.

[Page Break]

We are interested in understanding what the experience of the speech and math tasks were like for you.

Earlier in the study, on a scale from 0 to 100, you rated your excitement as a [**X**, **X+20**]. What made you rate the task in that way? Please be detailed in your response.

--

## Appendix J

### Emotion Memory Questionnaire, Study 2

[Note: Questions were displayed on separate pages.]

Please think back to the speech and math tasks you completed earlier in the study. We are interested in how you recall those tasks now.

Rate the average level of sadness you experienced during the speech and math tasks (0 to 100)

No sadness  
0      10      20      30      40      50      60      70      80      90      100  
Most sadness imaginable

Sadness experienced



[Page Break]

Rate the average level of anxiety you experienced during the speech and math tasks (0 to 100)

No anxiety  
0      10      20      30      40      50      60      70      80      90      100  
Most anxiety imaginable

Anxiety experienced



[Page Break]

Rate the average level of happiness you experienced during the speech and math tasks (0 to 100)

No happiness  
0      10      20      30      40      50      60      70      80      90      100  
Most happiness imaginable

Happiness experienced



[Page Break]

Rate the average level of excitement you experienced during the speech and math tasks (0 to 100)

No excitement

Most excitement imaginable

0 10 20 30 40 50 60 70 80 90 100

Excitement experienced



## **Appendix K**

### **Funneled Debriefing, Study 2**

- You have completed all the tasks of the study. First, what do you think the study was about?
- Did you find any part of the study strange or surprising?
- Think back to the first speech task you had to do. Do you remember reporting how much anxiety and excitement you felt during the task on the computer? Later on, the computer reminded you of this rating. Did you notice anything strange about this process?
- Okay, thank you. The true purpose of this study was to better understand how different aspects of your memory for a stressful experience would influence your performance in a future stressful experience. Earlier in this study, you were asked to report why you rated your anxiety and excitement for the speech task the way that you did. But for some subjects the emotion ratings we told you were yours were actually different from what you had really said. I do not personally know whether your rating was changed by the computer or not. Do you think you were in the group that was given different emotion ratings? [wait for response] Why or why not?
- Have you ever participated in a study before that included a speech task like the one you did for this experiment? [wait for response] Are you familiar at all with that task? [wait for response] Did you take 103H, Health Psychology in the Fall [Last quarter]?

## Appendix L

### TSST Evaluation Rubric

	Strongly Disagree	Disagree	Somewhat disagree	Neither agree nor disagree	Somewhat agree	Agree	Strongly Agree
The speaker was persuasive	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
The speaker deserves the scholarship[/leadership role]	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
The speaker was confident	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
The speaker seemed self-assured	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
The speaker was anxious	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
The speaker was excited	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
The speaker was intelligent	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
The speaker knew what s/he was talking about	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
The speech made sense	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
The speaker was persistent	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

What is your overall rating of the speech?

Very poor	Poor	Somewhat poor	Neither good nor poor	Somewhat good	Good	Very good
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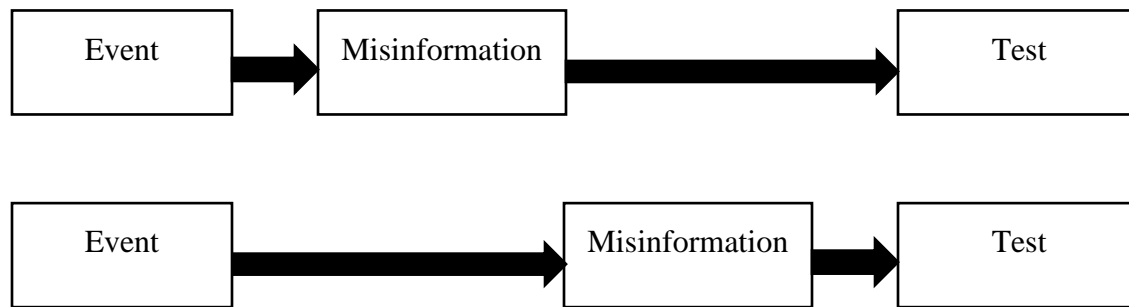


What is the lowest number the participant reached before making their first mistake? \_\_\_\_\_

What is the lowest number the participant reached during the exercise? \_\_\_\_\_

What is your overall rating of the math task?

Very poor	Poor	Somewhat poor	Neither good nor poor	Somewhat good	Good	Very good
-----------	------	---------------	-----------------------	---------------	------	-----------



*Figure 1.1.* Retention intervals in a misinformation study. When the interval between the event and the misinformation is short and the interval between the misinformation and the test is long (top), the misinformation effect is somewhat weaker. When the interval between the event and the misinformation is long and the interval between the misinformation and test is short (bottom), the misinformation effect is somewhat stronger.

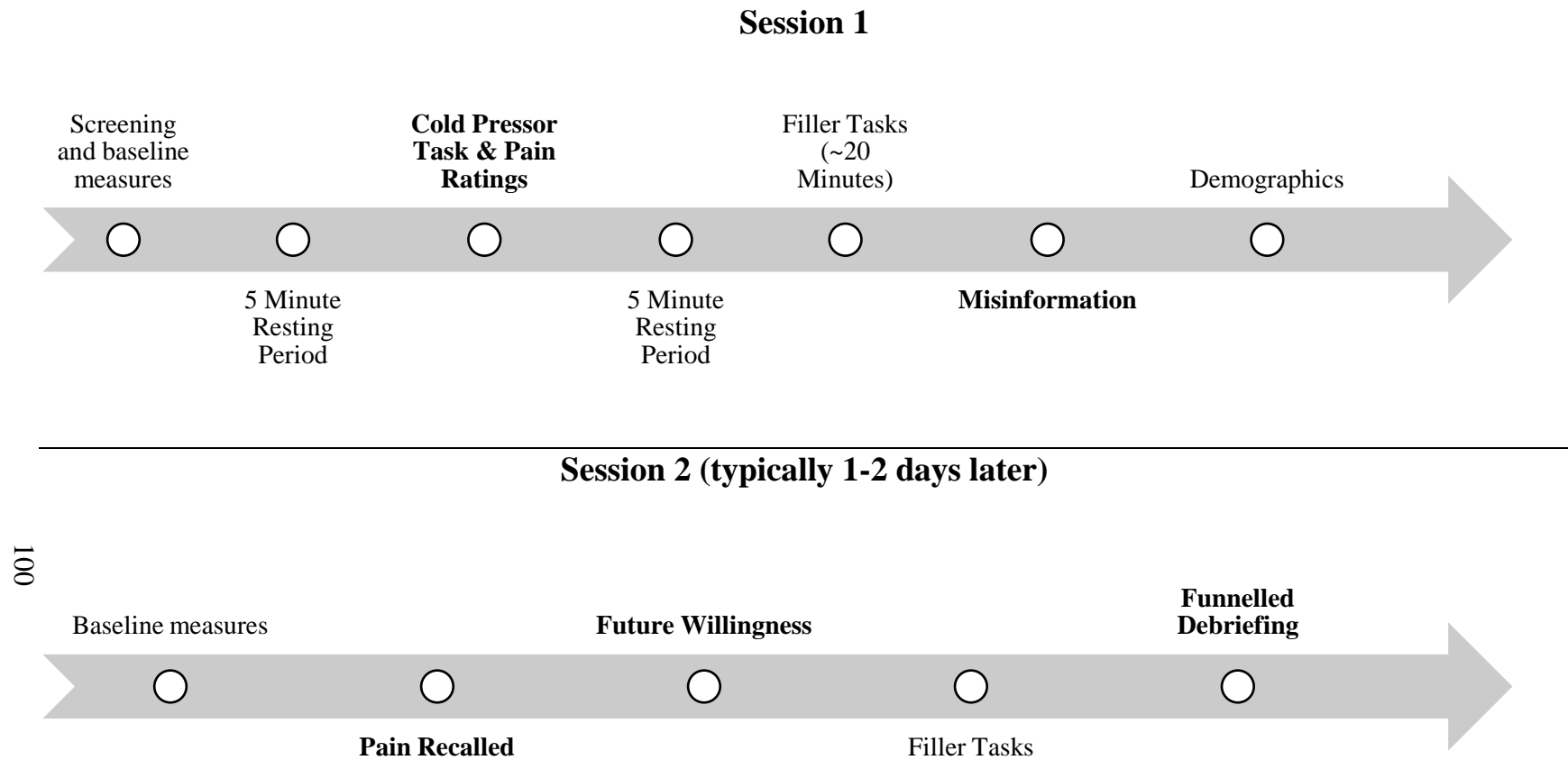
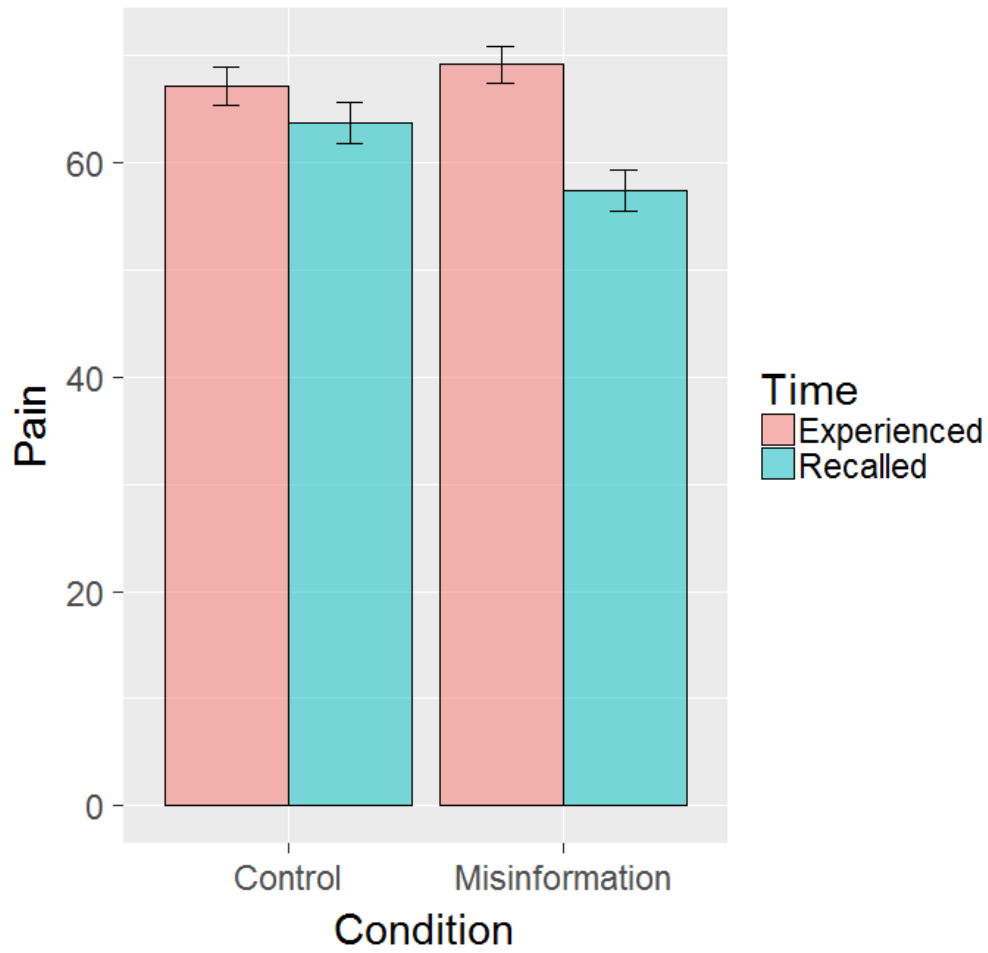
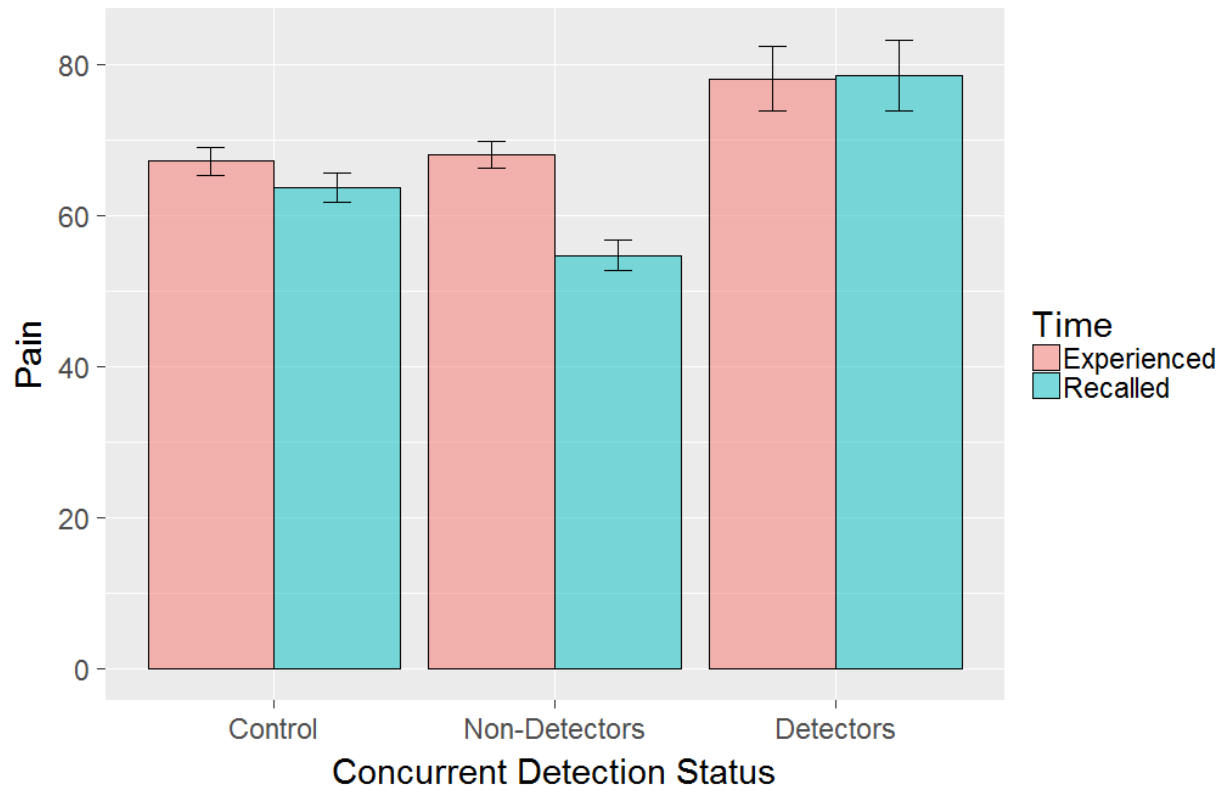


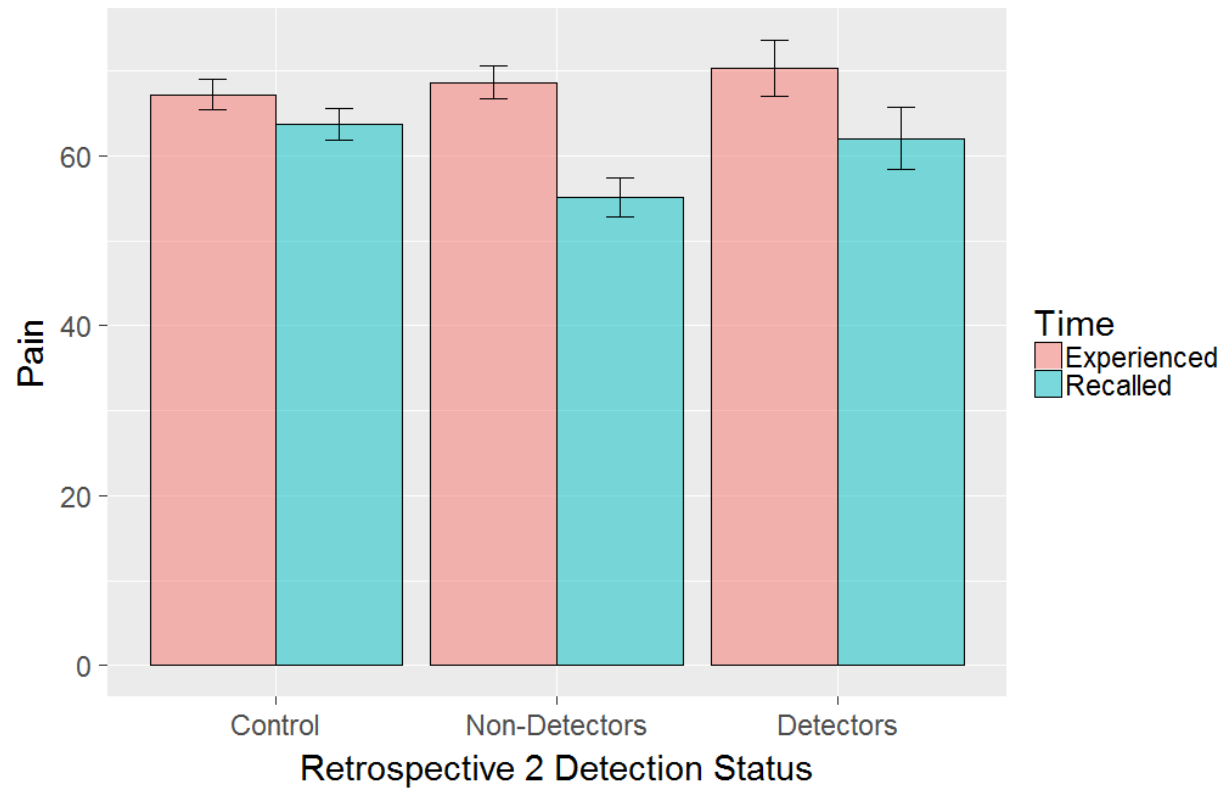
Figure 1.2. Procedural flowchart for Study 1



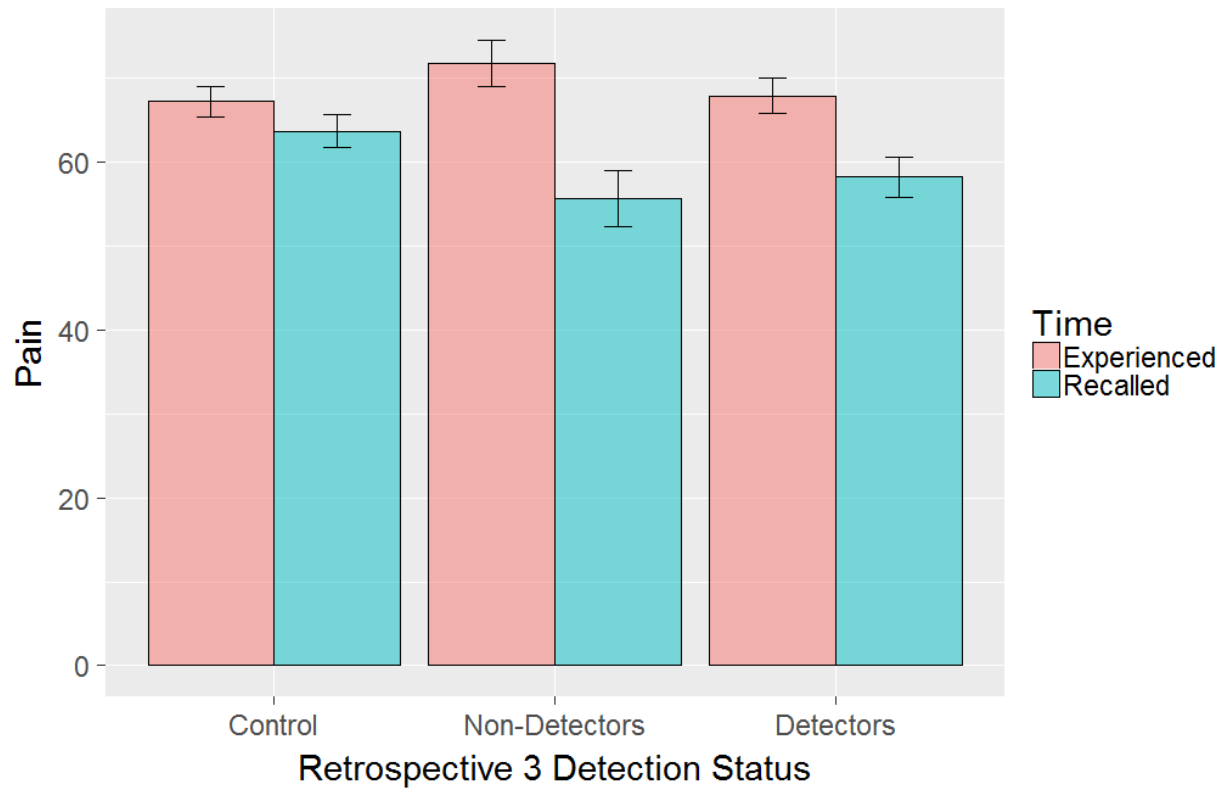
*Figure 1.3.* Pain experienced and recalled by condition in Study 1. Error bars represent  $\pm 1$  S.E.M.



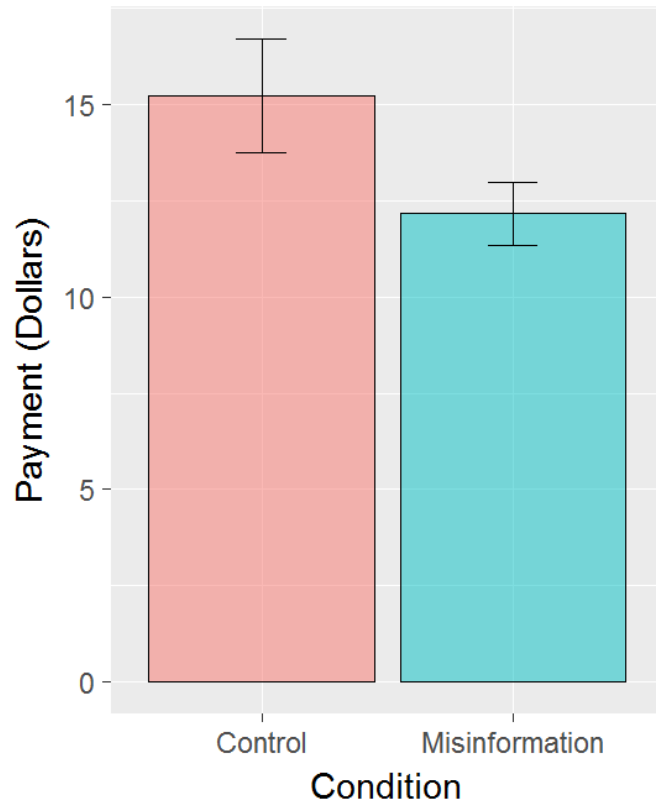
*Figure 1.4.* Pain experienced and recalled by concurrent detection status in Study 1. Error bars represent +/- 1 S.E.M.



*Figure 1.5.* Pain experienced and recalled by Retrospective 2 detection status in Study 1. Error bars represent +/- 1 S.E.M.

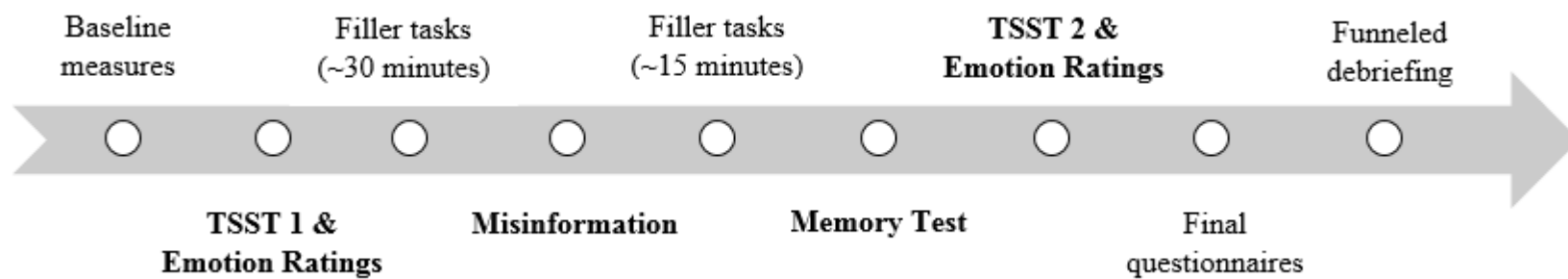


*Figure 1.6.* Pain experienced and recalled by Retrospective 3 detection status in Study 1. Error bars represent +/- 1 S.E.M.



*Figure 1.7.* Suggested compensation for a similar future study by condition in Study 1. Error bars represent  $\pm 1$  S.E.M.





*Figure 2.1.* Procedural flowchart for Study 2.

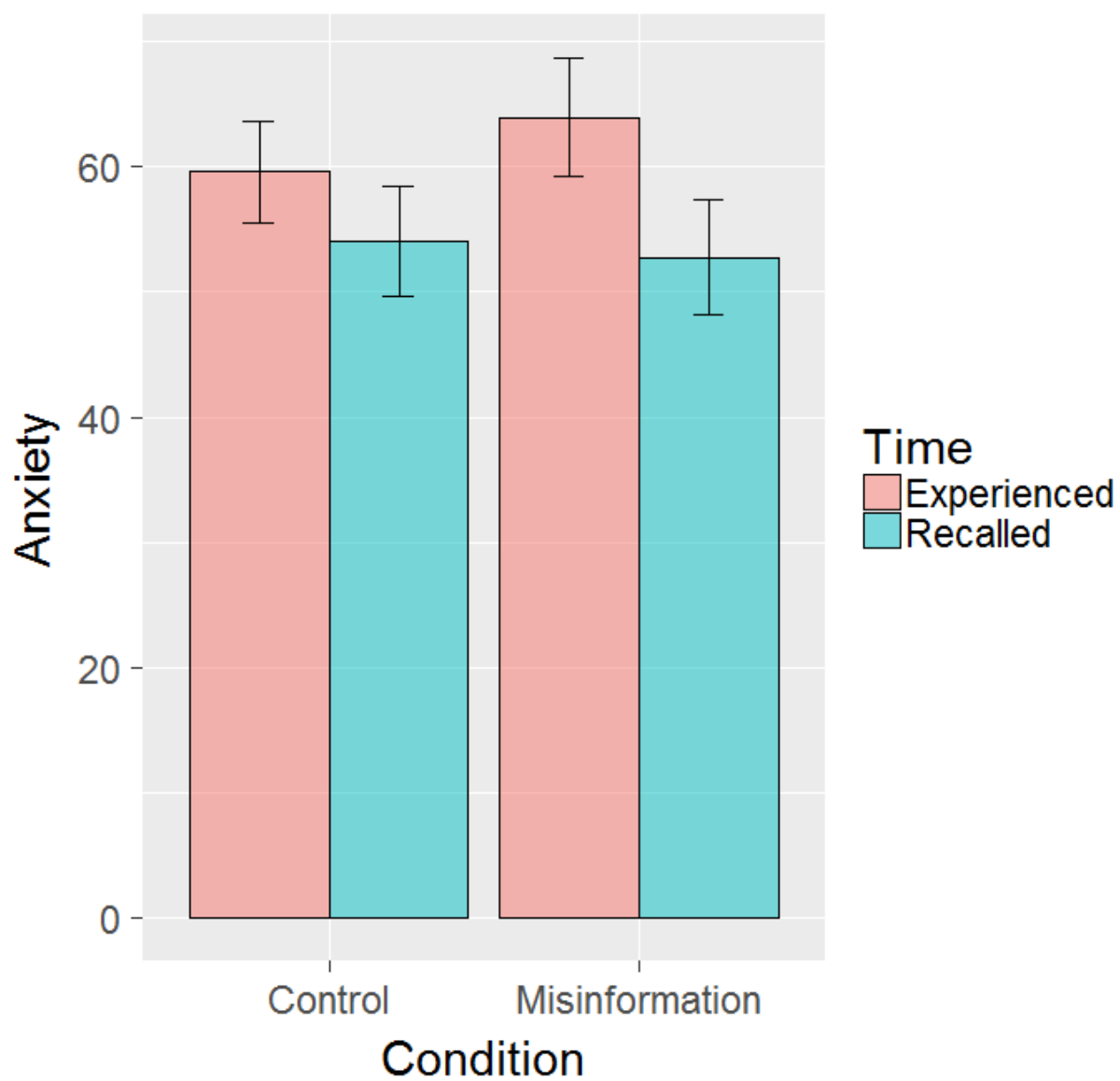
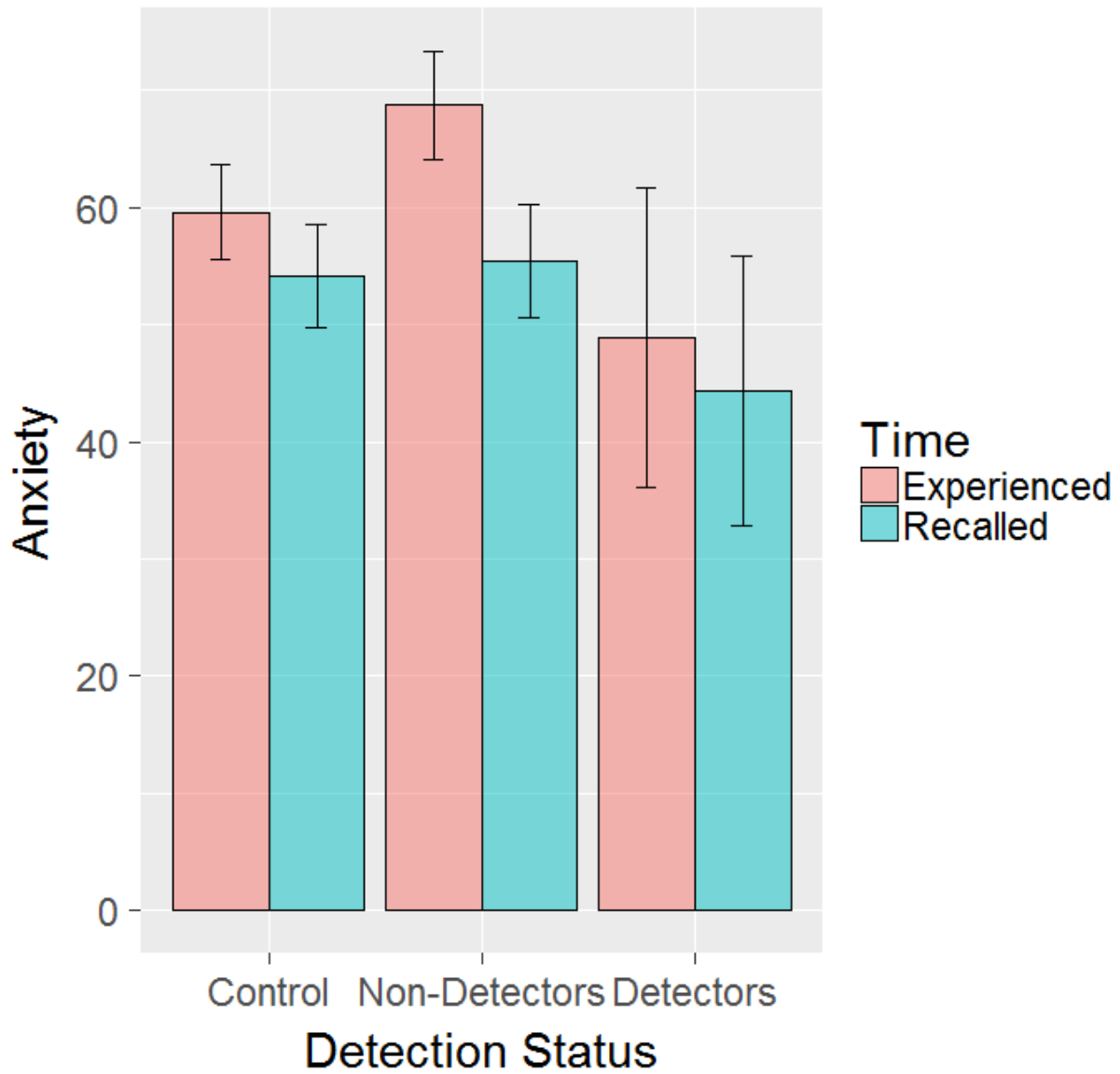


Figure 2.2. Experienced and recalled anxiety ratings by condition in Study 2. Error bars represent  $\pm 1$  S.E.M.



*Figure 2.3.* Experienced and recalled anxiety ratings by detection status in Study 2. Error bars represent +/- 1 S.E.M.

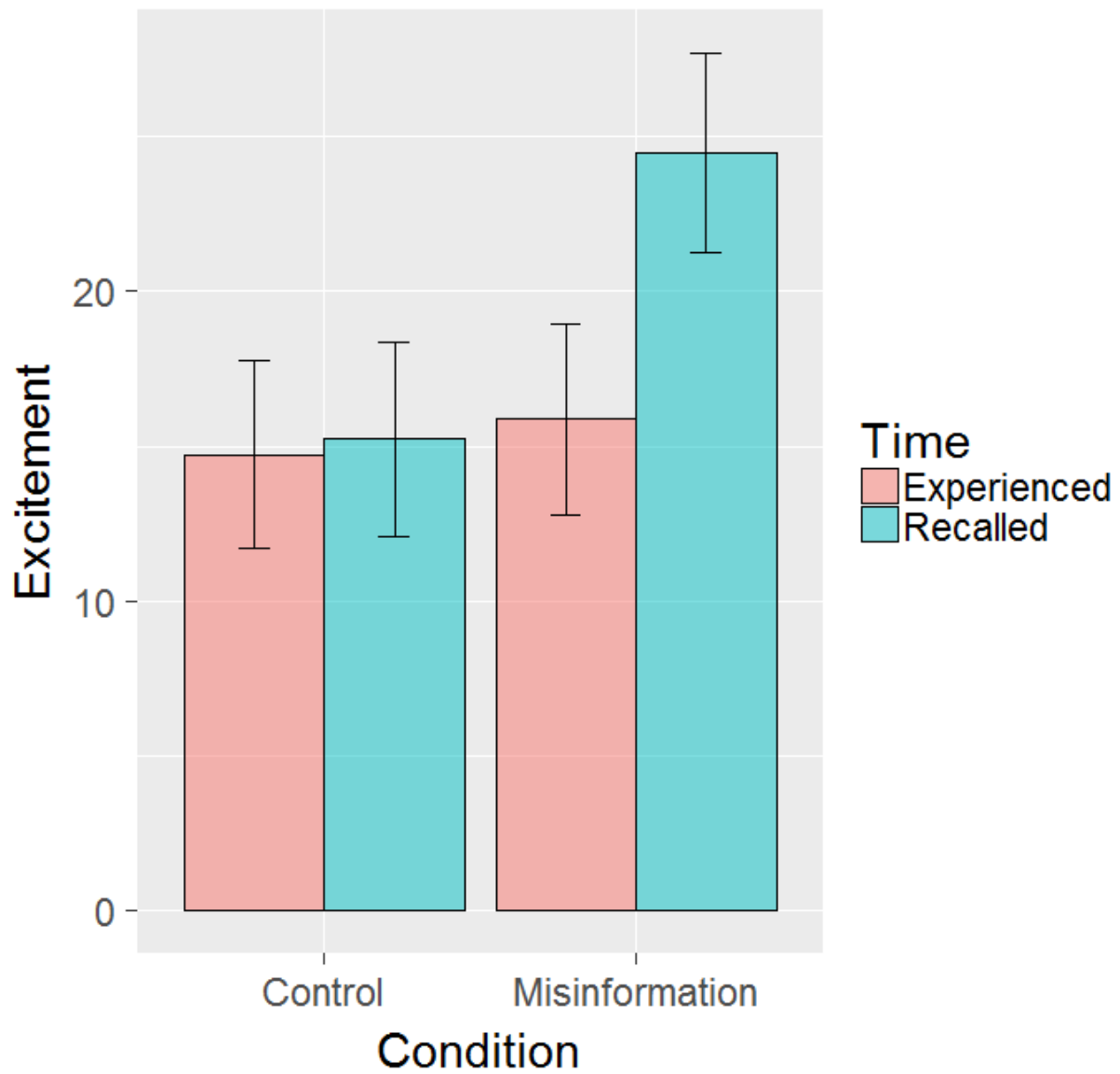
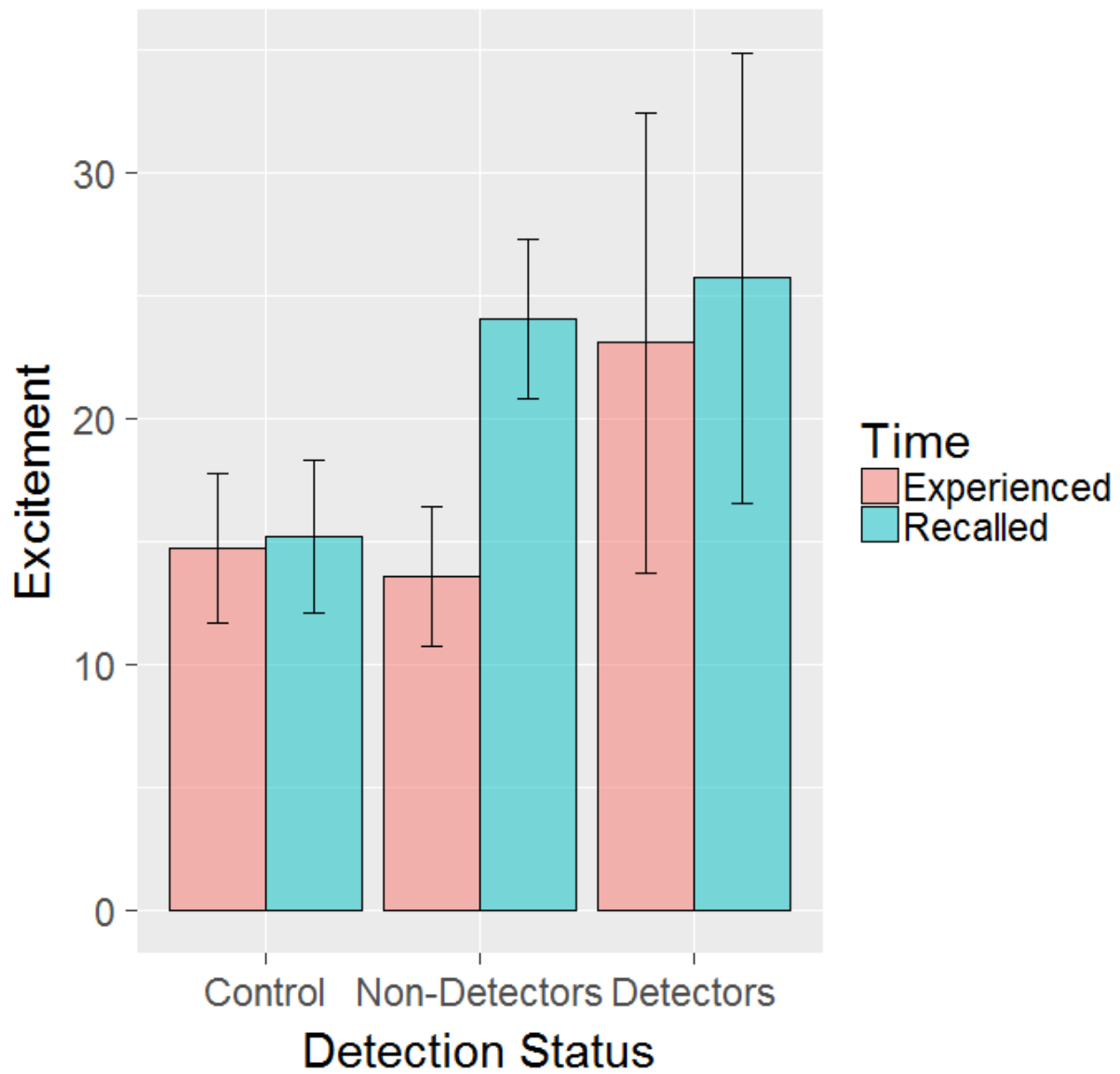


Figure 2.4. Experienced and recalled excitement ratings by condition in Study 2. Error bars represent  $\pm 1$  S.E.M.



*Figure 2.5.* Experienced and recalled excitement ratings by detection status in Study 2. Error bars represent +/- 1 S.E.M.

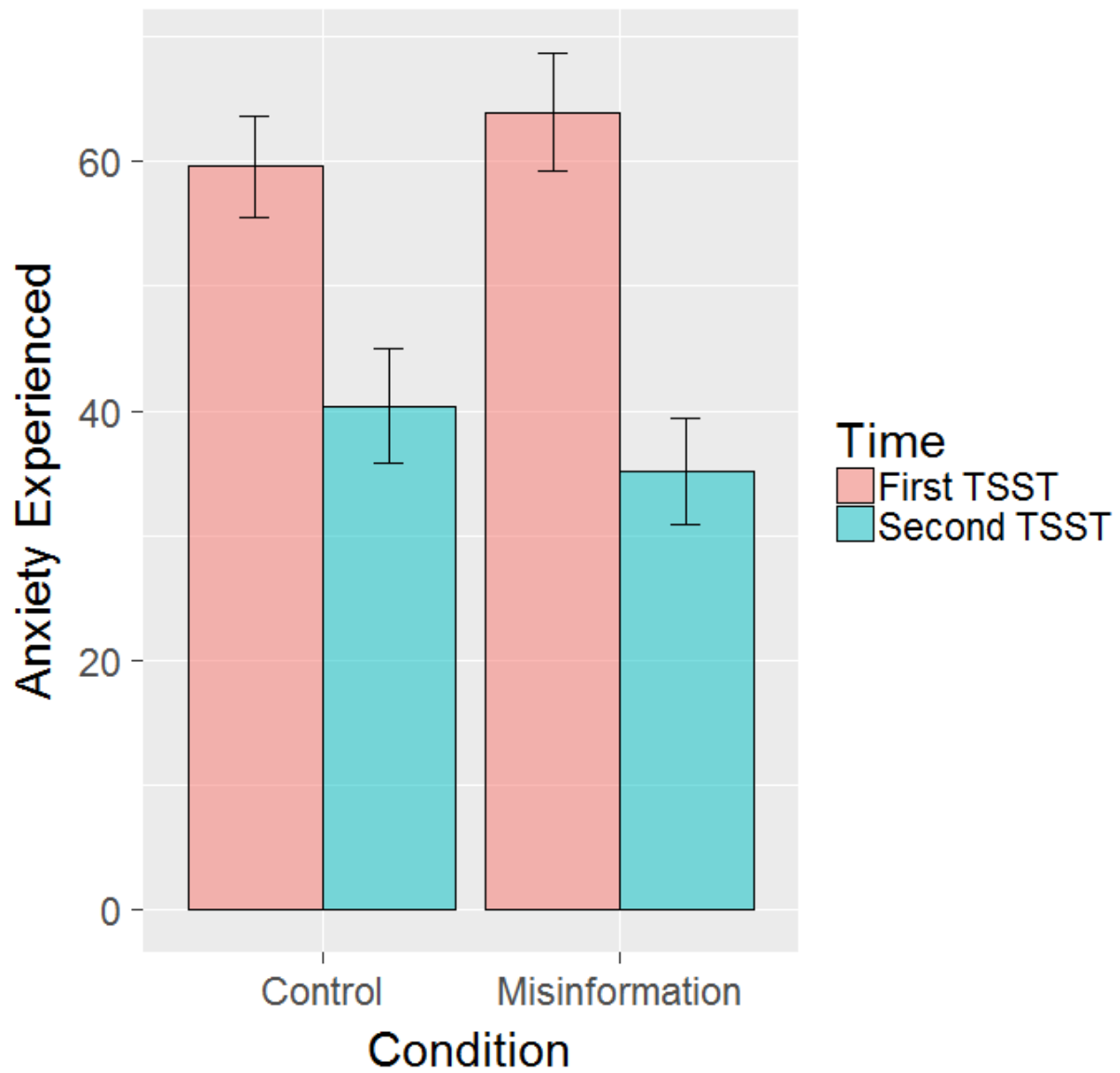


Figure 2.6. Anxiety experienced by TSST and condition in Study 2. Error bars represent  $\pm 1$  S.E.M.

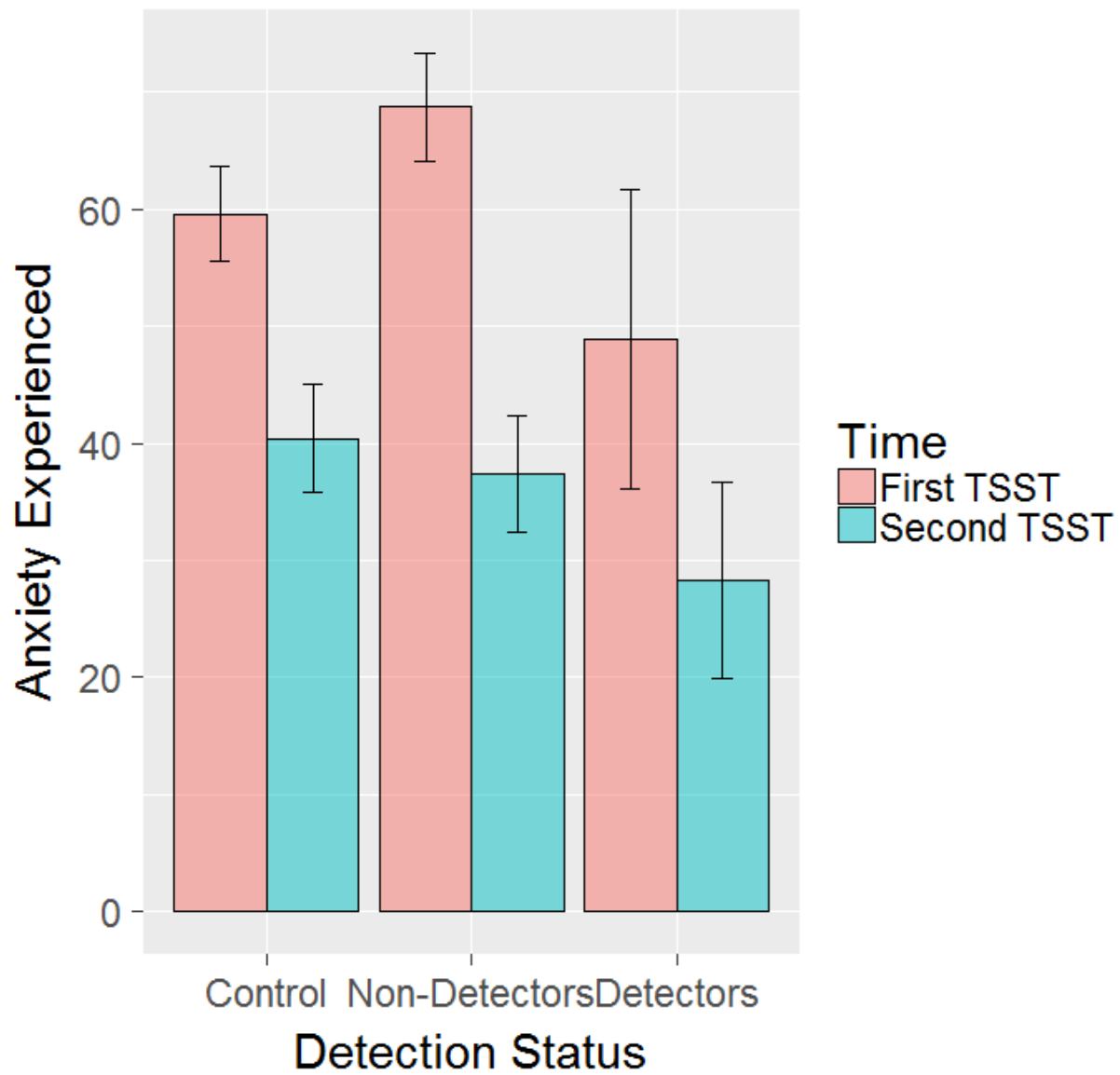


Figure 2.7. Anxiety experienced by TSST and detection status in Study 2. Error bars represent  $\pm 1$  S.E.M.

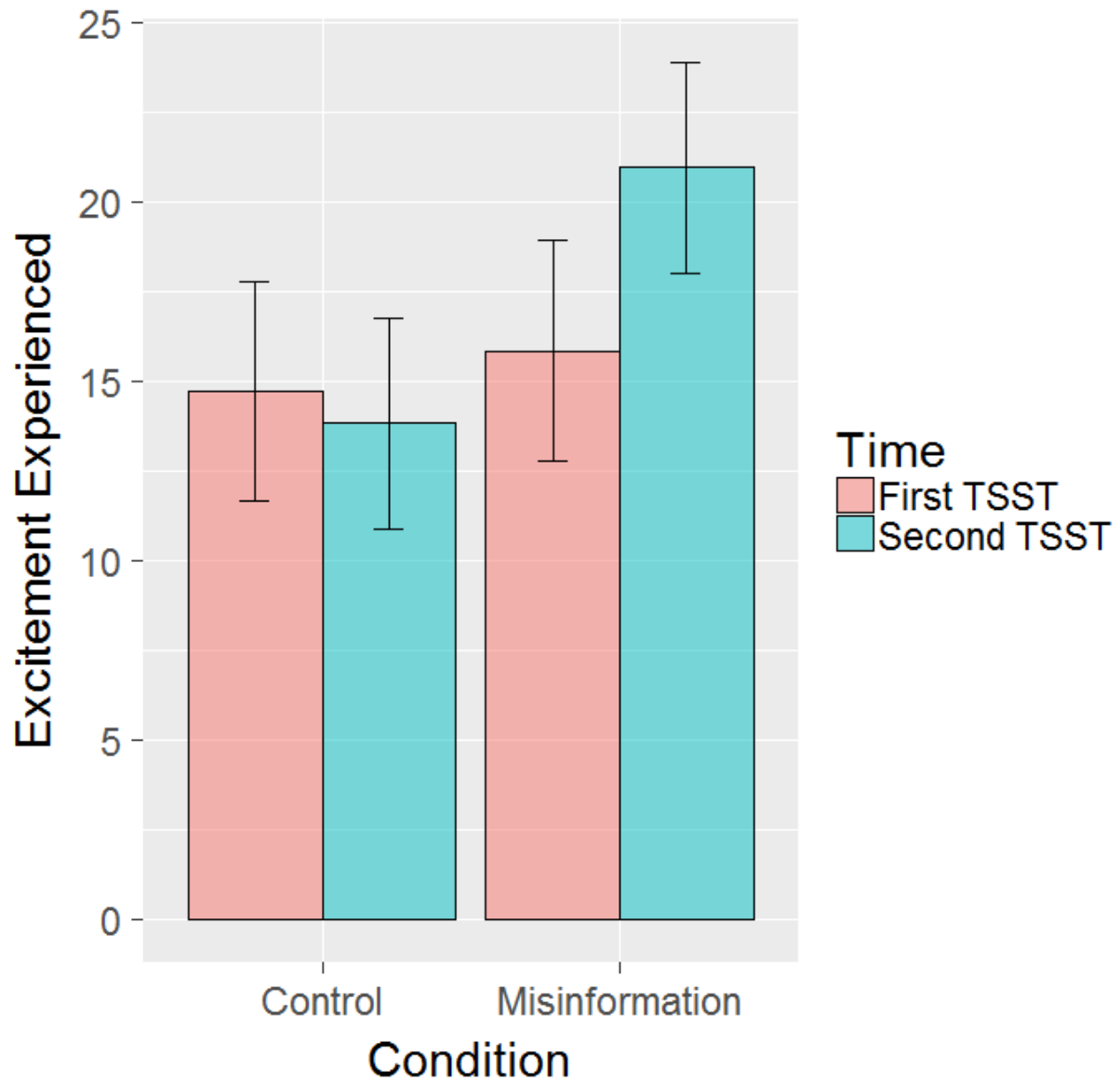


Figure 2.8. Excitement experienced by TSST and condition in Study 2. Error bars represent  $\pm 1$  S.E.M.



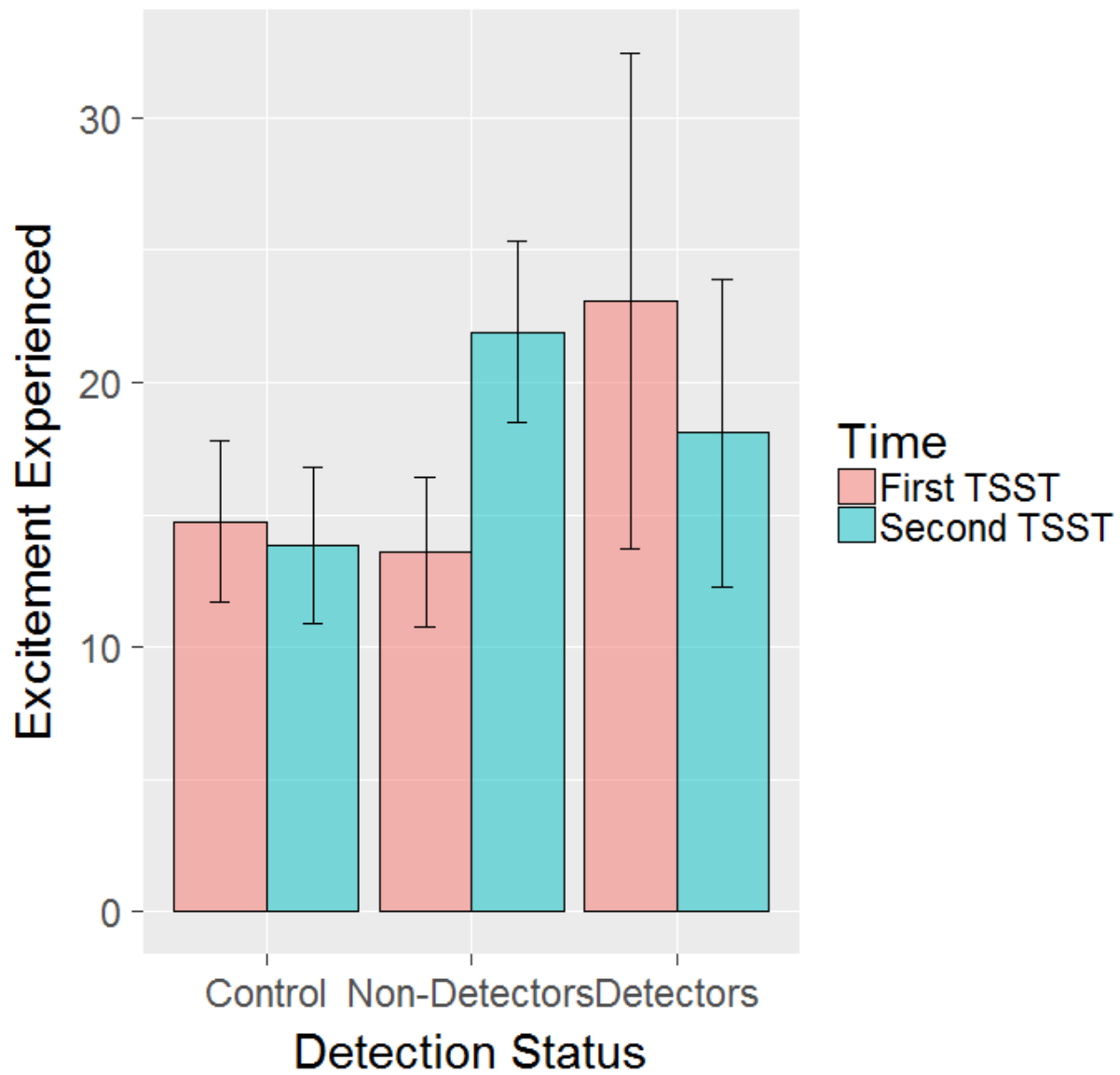


Figure 2.9. Excitement experienced by TSST and detection status in Study 2. Error bars represent  $\pm 1$  S.E.M.

Table 1.1

*Reliability measures for coding subjects as detectors for Study 1.*

	<i>n</i>	Average Pairwise Percent Agreement	Krippendorff's Alpha
Batch 1			
Concurrent	210	97.78	0.75
Retrospective 1	188	99.29	0.33
Retrospective 2	187	95.01	0.85
Retrospective 3	188	98.23	0.96
Batch 2			
Concurrent	59	98.87	0.92
Retrospective 1	56	98.81	0.00
Retrospective 2	56	98.81	0.96
Retrospective 3	56	98.81	0.98

*Note.* Each response was coded by three independent coders. Reliability statistics are presented separately for Batch 1 and Batch 2 because they employed different sets of coders. Subjects in both the misinformation group and the control group are included. Alpha values are low for Retrospective 1 due to extremely low rates of detectors. One subject had missing data for Retrospective 2.

Table 1.2

*Rates of subjects coded as detectors for different measures for Study 1.*

	<i>n</i>	Number of Detectors	Detection Rate
Experimental			
Concurrent	135	15	11.1%
Retrospective 1	127	1	.8%
Retrospective 2	127	42	33.1%
Retrospective 3	127	85	66.9%
Control			
Concurrent	134	0	0%
Retrospective 1	117	0	0%
Retrospective 2	116	8	6.9%
Retrospective 3	117	23	19.7%

*Note.* Detection rates are shown for each measure of detection for both conditions. The control condition is included to demonstrate the amount of bias each measure includes. One subject had missing data for Retrospective 2.

Table 1.3

*Point-biserial correlations between detection status and individual difference measures for Study 1.*

	Concurrent ( <i>n</i> = 14 detectors)	Retrospective 2 ( <i>n</i> = 42 detectors)	Retrospective 3 ( <i>n</i> = 85 detectors)
BAS fun-seeking	-.042	.059	<b>.200*</b>
Hypersensitive Narcissism	<b>.198*</b>	.139	-.009
Social Desirability	<b>-.269**</b>	-.174 <sup>+</sup>	-.101
Openness to Experience	.007	-.047	<b>-.245**</b>
Conscientiousness	<b>-.196*</b>	-.124	.064
Sensation Seeking	-.052	<b>-.204*</b>	-.106

*Note.* +  $p < .10$ , \*  $p < .05$ , \*\*  $p < .01$ .  $n = 127$ . Point-biserial correlations between each level of detection and individual difference measures. Retrospective 1 is omitted due to too few detectors being present.

Table 2.1

*Reliability measures for nominal coding in Study 2.*

		Average Pairwise	Percent Perfect	Krippendorff's
	<i>n</i>	Percent Agreement	Agreement	Alpha
Concurrent	188	98.94	98.40	.78
Anxiety	94	98.58	97.87	.84
Excitement	94	99.29	98.94	0
Retrospective 1	19	97.90	94.74	0
Retrospective 2	18	88.89	77.78	.56
Retrospective 3 <sup>1</sup>	19	85.26	68.42	.71

*Note.* For concurrent detection, responses were coded by three independent coders. For all other measures, responses were coded by five independent coders. Subjects in both the misinformation group and the control group are included. Alphas are low for Concurrent Excitement and Retrospective 1 due to an extremely low rate of detectors.

<sup>1</sup>For coding retrospective 3 detection, the coders had three responses: detector, non-detector, or that the participant had said something like "I don't know." Because saying "I don't know" does not give any evidence that the subject may have detected the manipulation, those ratings were re-coded as non-detectors prior to calculating the reliability statistics.

Table 2.2

*Reliability statistics for constructs in Study 2.*

	<i>n</i>	ICC(2,1)	ICC(2,5)
Persuasiveness <sup>1</sup>	19	.675	.912
Persuasive	19	.701	.922
Deserved scholarship	19	.582	.875
Deserved leadership position <sup>1</sup>	19	.783	.947
Confidence	19	.780	.947
Confident	19	.764	.942
Self-assured	19	.714	.926
Anxious	19	.561	.864
Excited	19	.418	.782
Competence	19	.788	.949
Intelligent	19	.728	.930
Knows what s/he was talking about	19	.736	.933
Made sense	19	.731	.932
Persistent	19	.826	.960
Overall speech	19	.771	.944
Overall math	19	.921	.983

*Note.* 2-way random intraclass correlations for consistency are displayed. Both single measures and average measures are shown. These values are calculated from the first TSSTs from the first 19 subjects. For the computed measures of persuasiveness, confidence, and competence, following Landers (2015), averaged measures were computed first for each coder, and ICCs were computed for those averaged measures.

<sup>1</sup>For persuasiveness, one item in the averaged measure was different between the two TSSTs. This item had to do with whether the participant deserved the accolade they were advocating for (the scholarship or the leadership position). These items were designed to function the same way but to be tailored to the two different TSSTs. To compute the ICC for persuasiveness, only the scholarship item was averaged with the persuasive item. The leadership position ICCs are also displayed.

Table 2.3

*Reliability statistics for coders in Study 2.*

	<i>n</i>	Cronbach's $\alpha$	ICC(2,1) [95% CI]	ICC(2,5) [95% CI]
Coder 1	19	.946		
Coder 2	19	.976		
Coder 3	19	.952		
Coder 4	19	.959		
Coder 5	19	.967		
Across coders	19		.831 [.708, .920]	.961 [.924, .983]

*Note.* For each coder, Cronbach's  $\alpha$  is shown for the 11 items assessing the quality of the speech task. The item assessing anxiety was reverse-coded. Across all 5 coders, 2-way random intraclass correlations for consistency are displayed. Both single measures and average measures are shown. These values are calculated from the first TSSTs from the first 19 subjects. Following Landers (2015), averaged measures were computed first for each coder, and ICCs were computed for those averaged measures.

Table 2.4

*Rates of subjects coded as detectors for different measures in Study 2.*

	<i>n</i>	Number of Detectors	Detection Rate
Experimental			
Concurrent	46	4	8.7%
Retrospective 1	45	0	0 %
Retrospective 2	45	10	22.2%
Retrospective 3	43	16	37.2%
Detectors	46	11	23.9%
Control			
Concurrent	48	1	2.1%
Retrospective 1	48	0	0%
Retrospective 2	47	3	6.4%
Retrospective 3	47	10	21.3%
Detectors	48	4	8.3%

*Note.* Detection rates are shown for each measure of detection for both conditions. The control condition is included to demonstrate the amount of bias each measure includes.