

Do people keep believing because they want to? Preexisting attitudes and the continued influence of misinformation

Ullrich K. H. Ecker · Stephan Lewandowsky ·
Olivia Fenton · Kelsey Martin

© Psychonomic Society, Inc. 2013

Abstract Misinformation—defined as information that is initially assumed to be valid but is later corrected or retracted—often has an ongoing effect on people’s memory and reasoning. We tested the hypotheses that (a) reliance on misinformation is affected by people’s preexisting attitudes and (b) attitudes determine the effectiveness of retractions. In two experiments, participants scoring higher and lower on a racial prejudice scale read a news report regarding a robbery. In one scenario, the suspects were initially presented as being Australian Aboriginals, whereas in a second scenario, a hero preventing the robbery was introduced as an Aboriginal person. Later, these critical, race-related pieces of information were or were not retracted. We measured participants’ reliance on misinformation in response to inferential reasoning questions. The results showed that preexisting attitudes influence people’s use of attitude-related information but not the way in which a retraction of that information is processed.

Keywords Misinformation · Continued influence effect · Attitudes · Beliefs · Motivated reasoning

Misinformation—defined as information that is initially believed to be valid but is subsequently retracted or corrected¹—has an ongoing impact on people’s memory and inferential reasoning, even after unambiguous and clear retractions. For

¹This is in contrast to another common usage of the term *misinformation* in the literature on source memory, and in particular on eyewitness memory, where the term is used in a more general way to refer to erroneous information, and in particular postevent suggestive misinformation (cf. Loftus, 2005).

Electronic supplementary material The online version of this article (doi:10.3758/s13421-013-0358-x) contains supplementary material, which is available to authorized users.

U. K. H. Ecker (✉) · S. Lewandowsky · O. Fenton · K. Martin
School of Psychology, University of Western Australia, Crawley,
WA 6009, Australia
e-mail: ullrich.ecker@uwa.edu.au

example, when people make inferences regarding the causal chain leading up to an event (e.g., the circumstances of a fire), misinformation (e.g., an initial suspicion of arson that is later corrected) is often relied upon, even when people accurately remember its retraction (Ecker, Lewandowsky, & Apai, 2011; Ecker, Lewandowsky, Swire, & Chang, 2011; Ecker, Lewandowsky, & Tang, 2010; H. M. Johnson & Seifert, 1994; Lewandowsky, Ecker, Seifert, Schwarz, & Cook, 2012; Wilkes & Leatherbarrow, 1988).

In studies on this topic, participants typically read a news report about a fictional event, in which a piece of causal information is first given and then retracted for one group of participants. Participants are subsequently given a questionnaire asking them to make inferences about the event in response to indirect questions (e.g., in the present example, regarding the cause of the fire or the response from authorities). References to the initial piece of misinformation are then tallied and compared to those of another group that did not receive a retraction. The typical result is that a retraction at most halves the number of references to a piece of misinformation, but that it does not eliminate the misinformation’s influence altogether (cf. Lewandowsky et al., 2012, for a review).

Previous research has offered some suggestions why this *continued-influence effect* of misinformation (H. M. Johnson & Seifert, 1994) arises. Most of these theoretical accounts refer to failures of strategic memory processing. They argue that retracted or outdated information remains available in memory, despite retractions or attempts to update memory (cf. Ayers & Reder, 1998; Bjork & Bjork, 1996; Ecker, Lewandowsky, Oberauer, & Chee, 2010; Kendeou & O’Brien, *in press*; Oberauer & Vockenberg, 2009). If this retracted but available information is automatically activated, it might be accepted as valid at face value; in particular, when its processing appears fluent, people might use a heuristic that fluency implies veracity (cf. Ecker, Swire, & Lewandowsky, *in press*; M. K. Johnson, Hashtroudi, & Lindsay, 1993; Lewandowsky et al., 2012; Schwarz, Sanna, Skurnik, & Yoon, 2007). Hence, any automatic activation of outdated or invalidated information will require some strategic

memory processing to counteract the potential impact of the automatically retrieved but invalid information. This strategic memory processing could involve the recollection of contextual details such as the source of the information or the details of the correction, or it could rely on a strategic monitoring process that determines the validity of an automatically retrieved piece of information (cf. Ecker, Lewandowsky, Apai, 2011 and Ecker, Lewandowsky, Swire, et al. 2011; Gilbert, Krull, & Malone, 1990; M. K. Johnson et al., 1993). The fact that explicit warnings about the misinformation effect reduce people's reliance on misinformation substantially (but not completely) has been taken as evidence for such a dual-process account of automatic and strategic retrieval processing (Ecker, Lewandowsky, & Tang, 2010).

Yet, this purely cognitive explanation does not consider motivational factors. A person processing information (including misinformation and retractions) is not a *tabula rasa*: People have preexisting opinions and attitudes and process information in relation to what they already know and believe. Hence, in many real-world circumstances, people will have a motivation to believe one event version over another; that is, people will often have an intrinsic motivation to resist a retraction.

Consequently, survey research suggests that in the real world, attitudes play a major role in how people process misinformation and retractions. For example, Kull, Ramsay, and Lewis (2003) investigated misperceptions regarding the 2003 Iraq war, including the belief that weapons of mass destruction (WMD) had been found in Iraq after the invasion. During this period, many suspected WMD findings were reported in the media, all of which were later retracted. Despite the extensive media coverage of the *failures* to find WMDs, a substantial proportion of the US public continued to believe that WMDs had been found (see also Jacobson, 2010), and these people also showed much stronger support for the war than did people who correctly believed that no WMDs had been discovered. Similarly, Travis (2010) reported that ongoing belief in the clearly refuted assertion that President Obama was born outside the US was much more widespread among Republicans than Democrats. In a different context, a study carried out in the UK demonstrated that concerns about the refuted link between a common vaccine and autism (cf. Ratzan, 2010) covaried with distrust in the public health system and the government's role in regulating health risks (Casiday, Cresswell, Wilson, & Panter-Brick, 2006). In Australia, Pedersen, Attwell, and Heveli (2005) reported that false beliefs about asylum seekers were predicted by political position and strength of national identity.

Although these results demonstrate how attitudes determine (or at least covary with) people's beliefs in common misconceptions,² they do not directly address a very important question:

Do attitudes affect the processing of retractions directly? Surveys can shed light on people's belief in misinformation after its retraction, but surveys are rarely administered both before and after a retraction of misinformation. It therefore remains unclear whether an attitude-congruent belief after a retraction (a) simply mirrors the attitude-congruent belief *before* the retraction or (b) reflects the ineffectiveness of an attitude-incongruent retraction. For example, possibility (a) suggests that a person mistrusting the public health system might have believed speculations about a vaccine–autism link more than a person who trusted the public health system, both before and after the retraction (with the retraction thus potentially having the same quantitative effect on both people). Alternatively, possibility (b) suggests that both people might have believed the initial suggestion to a similar degree, but that the retraction may have reduced misbelief only in the trusting person.

Both of these possibilities are plausible. Misinformation that supports one's attitudes will be consistent with existing personal knowledge and other beliefs, will be familiar and therefore easy to process and more readily believed (cf. Dechêne, Stahl, Hansen, & Wänke, 2010; Schwarz et al., 2007), and will often come from a trusted source and be shared by others in one's social network. These factors may lead to attitude-dependent acceptance of misinformation (cf. Lewandowsky et al., 2012). On the other hand, retractions that contradict one's worldview will be less consistent with existing beliefs, less familiar, and more likely to come from an untrustworthy source and not be shared by peers. These factors may undermine a retraction's effectiveness (cf. Lewandowsky et al., 2012).

Lewandowsky, Stritzke, Oberauer, and Morales (2005) found that people who were skeptical about the official motives for the 2003 Iraq war were better able to discount war-related misinformation more generally. In their study, participants read actual news items, some of which had been publicly retracted, and their belief in the items as well as memory for the retractions was measured. Lewandowsky et al. (2005) found that people who were skeptical about the official *casus belli* (i.e., they thought that the war was launched for reasons unrelated to WMDs) showed reduced belief in pieces of information if they remembered a piece's retraction, whereas memory for a retraction did not reduce belief in less skeptical participants. In other words, people who accepted the official reason for the war would continue to believe a retracted news story, despite being able moments earlier to state explicitly that the story was false. This result suggests that skepticism may be a mediating factor in the processing of retractions. Yet, arguably, people who were skeptical about the official reasons for the invasion of Iraq would have also had a different attitude regarding the war and war-related information (i.e., a more "anti-war" attitude). This implies that to the degree that the retracted pieces of information used in the Lewandowsky et al. study were "pro-war," skeptical people would have been

² For discussion of the reverse effects of memory on attitudes, see Bizer, Tomala, Rucker, and Petty (Bizer et al. 2006), Hastie and Park (1986), and Loken and Hoverstad (1985).

more likely to accept the retractions because they were more in line with their general “anti-war” attitude. (All four “false-retracted” news items used in the Lewandowsky et al., 2005, study in fact portrayed the Allied forces as being strong and successful and/or the “enemy” as being weak, cruel, and fragmented; hence, their retraction would have been in line with an “anti-war” attitude.) Thus, Lewandowsky et al.’s (2005) results could be taken to suggest that people are willing to accept retractions only to the degree that the retractions are attitude-congruent.

Nyhan and Reifler (2010) addressed the issue in the field of political science. They presented Republicans and Democrats with a variety of political misperceptions (e.g., claims made by the George W. Bush administration that tax cuts in the early 2000s had increased government revenue). Some of their participants were also given factual retractions of these misperceptions (e.g., a statement that government revenues had actually decreased as a result of the tax cuts). Retractions were found to be effective only when they were attitude-congruent. For example, Democrats relied less on the misinformation that revenue had increased after reading a retraction. In cases of attitude incongruence, however, retractions actually backfired. That is, Republicans became even *more* likely to believe in the incorrect assertion of increased revenues after reading an attitude-incongruent retraction [this could be described as an extreme manifestation of option (b) discussed above]. In a similar study, Nyhan, Reifler, and Ubel (2013) demonstrated that a correction of Sarah Palin’s “death panel” assertions was effective in participants not supporting Palin, but backfired in Palin supporters (at least in those who were politically knowledgeable; see also Hart & Nisbet, 2012, who reported that Republicans became *less* supportive of climate mitigation policies when confronted with the potential health impacts of climate change). The occurrence of such extreme, backfiring effects is quite surprising, considering how counterintuitive it seems at first glance that people would modify their belief in a direction *counter* to evidence. Some experiments have found more subtle attitude effects, in which corrections did not actually backfire, but were nonetheless relatively ineffective when they were attitude-incongruent (Nyhan & Reifler, 2010; see also Nyhan & Reifler, 2011). In sum, these findings suggest that attitudes have a major impact on the processing of retractions.

However, the results of Nyhan and Reifler (2010) and Nyhan et al. (2013) stand in contrast to some data from our own lab. In a reanalysis of data reported in Ecker, Lewandowsky, and Apai (2011)—a study designed to investigate the effect of emotiveness on misinformation processing, using a plane crash scenario—we found that participants high in islamophobia relied more on the misinformation that a terrorist attack had caused the plane crash (relative to participants with low islamophobia scores). In contrast to the results of Nyhan and colleagues, however, we found that this group difference was present and

of comparable magnitudes both before and after a retraction. In other words, the retraction had the same effect, independent of its attitude congruence, reducing the number of references to a terrorist attack in both groups to an equal extent [this is a manifestation of option (a) discussed above]. This finding must be considered provisional, because it was based on a post-hoc analysis of data collected for a different purpose that were available only for a subsample of participants. Nonetheless, other researchers have also failed to find support for attitude-driven backfire effects (Berinsky, 2012; Garrett, Nisbet, & Lynch, 2013). For example, Berinsky reported that corrections of the rumor that US health care changes would promote euthanasia were effective in both Democrats and Republicans.

The present study was designed to further investigate the interplay of attitudes and the processing of misinformation and retractions. Participants scoring higher and lower on a measure of racial prejudice read fictional accounts of crimes supposedly involving indigenous Australians; this aspect of the events—that is, the race of the protagonists—was subsequently retracted for one group of participants. Continued reliance on the retracted misinformation during a subsequent inference task was then compared to the responses in another group that had received no retraction. We expected the racial prejudice factor to have a substantial effect on the overall number of references to the critical race-related information. We also expected a retraction to significantly reduce the number of references to the critical information. We had no strong expectation regarding the question of whether racial attitudes would have an impact on the effectiveness of the retraction (i.e., the reduction in references to the critical information caused by the retraction), given the contradictory results obtained in previous research. Unlike most of the previous (survey-based) research, our experiment was designed to shed light on this issue, as it featured both nonretraction and retraction conditions.

Experiment 1

Method

In Experiment 1, participants scoring higher versus lower on a racial prejudice questionnaire were presented with a fictitious news report about a liquor store robbery. Three versions of the report were presented, which differed in the description of the suspects. Two versions initially described the suspects as Aboriginal Australians; this information was later retracted in one version of the report (the retraction condition) but not in the other (the no-retraction control condition). The third version (the no-misinformation control condition) described the suspects as Caucasian and also contained no retraction. The two control conditions provided a ceiling and baseline, respectively, against which to assess the effects of the retraction. The no-misinformation condition was included because any

description of a liquor store robbery in Australia may lead some people to assume that the suspects were Aboriginal on the basis of common stereotypes (cf. Wimshurst, Marchetti, & Allard, 2004); we thus expected this condition to yield a nonzero baseline. The experiment employed a 2 (racial prejudice: high vs. low) \times 3 (retraction condition: no-misinformation, no-retraction, retraction) between-subjects design.

The racial prejudice scale used was the Attitudes Toward Indigenous Australians scale (ATIA; Pedersen, Beven, Walker, & Griffiths, 2004), an 18-item questionnaire with good validity and reliability (reported internal consistency $\alpha = .93$; Pedersen et al., 2004), measuring racial prejudice toward indigenous Australians on 7-point Likert scales.

Participants A priori power analysis (G*Power 3; Faul, Erdfelder, Lang, & Buchner, 2007) suggested that in order to detect a medium-size effect of $\eta_p^2 = .1$ at $\alpha = .05$ and $1 - \beta = .80$, the minimum sample size should be at least 90. In line with precedents (see, in particular, Ecker, Lewandowsky, & Apai, 2011), we decided to test a total of $N = 144$ participants, all undergraduate students from the University of Western Australia (97 females, 47 males; age range 17–46, mean age 19 years).³ Approximately a third of these participants ($n = 47$) were randomly sampled from the upper and lower quartiles of a population of students prescreened with the ATIA ($N = 379$; the internal consistency of the ATIA in this population was $\alpha = .91$). The remaining two thirds were not prescreened for pragmatic reasons (i.e., lack of access to a prescreened population). All participants completed the ATIA (again) during the experimental session (test–retest reliability on the subsample that completed the ATIA twice was high, $\rho = .90$). Participants were randomly assigned to the different retraction conditions and divided into higher and lower racial prejudice groups on the basis of median splits on their (more recent) ATIA score ($n = 24$ per cell).

Stimuli Participants were given a fictitious news report, consisting of a series of 14 messages, each printed on a separate page, which provided an account of a liquor store robbery in Australia’s Northern Territory. Across conditions, the stories differed only at Message 5, when the critical information about the race of the suspects was introduced, and Message 11, in which that piece of critical information either was or was not retracted (see the Appendix in the supplemental materials).

In the no-misinformation condition, Message 5 stated that “police . . . believed the three suspects were Caucasian,” and Message 11 gave the neutral piece of repeated information that “Police . . . confirm[ed] that the owner of the store was the sole person in the store.” In the retraction and no-retraction conditions, Message 5 stated that “police . . . believed the three suspects were Aboriginal.” In the retraction condition, Message 11 then stated that “Police . . . no longer believed the suspects were . . . Aboriginal.” Message 11 in the no-retraction condition was identical to the neutral Message 11 of the no-misinformation condition.

Participants’ understanding of the story, and in particular their reliance on misinformation, was assessed using an open-ended questionnaire. The questionnaire contained ten inference questions, nine fact-recall questions, and two retraction-awareness questions (always given in this order, to prevent any impact from fact retrieval on people’s inferences; see the Appendix).

The inference questions required participants to infer something about the circumstances surrounding the incident and were designed to elicit responses indirectly related to the critical information—that is, the race of the suspects. For example, the inference question “Why did the shop owner have difficulty understanding the conversation between the attackers?” could be answered by relying on the critical information (e.g., that the intruders were speaking in their native Aboriginal language), although other explanations were possible (e.g., the attackers had their mouths covered or were intoxicated). The final inference question (“Who do you think the attackers were?”) was placed at the end of the fact-recall questions so that it appeared to be a recall question, but it was scored as an inference question.

The nine fact-recall questions were used to assess participants’ memory for the factual details of the story (e.g., “What sort of car was found abandoned?”). These questions did not relate to the race of the suspects. Finally, the two retraction-awareness questions tested participants’ awareness of the retraction (e.g., “Was any of the information in the story subsequently corrected or altered?”). The fact-recall and retraction-awareness questions were included to control for insufficient encoding, allowing for the potential exclusion of participants who did not recall the event sufficiently well or who may not have noticed the retraction at all.

Our analyses thus focused on (a) the accuracy of recall (fact-recall score), (b) memory for the retraction (retraction-awareness score), and most importantly, (c) reliance on the critical information (inference score).

Procedure Participants read the report at their own pace without backtracking; they were informed that their memory for and understanding of the report would be tested (to ensure adequate encoding). Reading was followed by a 30-min retention interval that was filled with an unrelated memory-updating computer

³ The participants’ race or ethnicity was neither considered nor recorded in the participant selection process, and from publicly available information we estimated that about 80 % of the participants were Caucasian and 20 % from culturally diverse (mainly Asian) backgrounds; only about 1 % could be expected to identify as Aboriginal.

task. Participants were then given the open-ended questionnaire and instructed to answer all questions in the order given. Finally, participants were given a bundle of three questionnaires to complete, the last of which was the ATIA. The ATIA was administered at the end of the procedure so as not to prime Aboriginal-related responses on the open-ended questionnaire; the two other questionnaires were unrelated to the study. The entire experiment took approximately 1 h.

Results

Racial prejudice scores ATIA scores ranged from 0 to 4.56 (the maximum possible score was 6). Across experimental conditions, the mean racial prejudice scores were 1.33 ($SE = 0.07$) and 2.91 ($SE = 0.08$) in the lower and higher racial-prejudice groups, respectively. We acknowledge that the ATIA score of the “high racial-prejudice” group in this experiment was only moderate.⁴ The group difference was nonetheless significant, $F(1, 142) = 220.63$, $MSE = 0.41$, $p < .001$, $\eta_p^2 = .61$. The mean racial prejudice scores for all cells are given in the upper half of Table 1.

Questionnaire coding All open-ended questionnaires were scored by a trained scorer who was blind to the experimental conditions, following a standardized guide. Another trained scorer scored five questionnaires from each condition in order to assess interrater reliability, which was found to be very high ($r > .95$ for inference, fact-recall, and retraction-awareness questions).

Fact-recall questions were scored 1 for *correct* responses and 0 for *incorrect* responses. For certain questions, it was possible to receive partial marks of .5 or .33 for partially correct responses, as determined a priori in the scoring guide. Since nine fact-recall questions were presented, the maximum fact-recall score was 9. The retraction-awareness questions were given a score of 1 if *participants remembered the retraction* and a score of 0 if *they did not*. The maximum retraction-awareness score was 2.

The inference questions were scored 0 or 1. Any uncontroverted mention of Aboriginal people, Aboriginal culture or

⁴ The ATIA score of the high-prejudice group was on par with the population mean (2.85 on a 0–6 scale) reported in Pedersen et al. (2004). The participants in Pedersen et al. (2004) came from the same city (Perth) but were on average much older (49.7 years) and less educated (with less than half attending or having attended a tertiary institution) than were the participants of the present study. Pedersen et al. (2004) reported correlations of both age and education with racial prejudice, with younger and more educated people being on average less prejudiced. This means that our high-prejudice group cannot be described as extremely high in racial prejudice, but that the mean prejudice score was probably above average for a student population.

Table 1 Racial prejudice (ATIA) scores, Experiments 1 and 2

Condition	Low-Prejudice Group		High-Prejudice Group	
	<i>M</i>	<i>SE</i>	<i>M</i>	<i>SE</i>
Experiment 1				
No misinformation	1.14	0.09	2.77	0.14
No retraction	1.60	0.14	3.11	0.15
Retraction	1.25	0.11	2.85	0.12
Experiment 2				
No retraction	1.45	0.12	4.18	0.23
Retraction	1.64	0.15	4.26	0.21

ATIA, Attitudes Toward Indigenous Australians Scale (Pedersen et al., 2004).

communities, or anything that directly implied that Aboriginal persons were the suspects of the robbery was counted as a reference to the critical information and given an inference score of 1. Examples of inferences scoring a 1 would be the response “The robbers were speaking in their Aboriginal language” to the inference question “Why did the shop owner have difficulty understanding . . . the attackers?,” or the response “Because police rarely solve crimes involving Aboriginals” to the inference question “Why do the police fear the case will remain unsolved?” In contrast, the response “First they thought the robbers were Aboriginals, but apparently not” to the inference question “Who do you think the attackers were?” would have been considered a controverted statement and given a score of 0. The maximum inference score was 10.

Accuracy of recall Mean fact-recall accuracy rates (out of a maximum of 1) for the high- and low-prejudice groups are given in the upper half of Table 2. A two-way analysis of variance (ANOVA) with the factors Retraction Condition and Racial Prejudice revealed no significant effects, $F_s < 2.84$, $p_s > .05$.

Awareness of retraction The mean rates of retraction awareness (in the retraction condition, out of a maximum of 1) were .79 ($SE = .08$) and .85 ($SE = .06$) for the high- and low-prejudice groups, respectively. This difference was not significant, $F < 1$.

Inferential reasoning The mean inference scores for both the high- and low-prejudice groups across all conditions are shown in Fig. 1.

Not surprisingly, the number of references to the critical information was lowest [but not zero: $M = 0.40$, $SE = 0.08$; $t(47) = 4.86$, $p < .001$] in the no-misinformation condition, when the critical piece of information was never explicitly given. Also as expected, the number of references to the critical information was highest in the no-retraction condition, when this information was introduced but never challenged.

We ran a two-way between-subjects ANOVA on mean inference scores, with the factors Retraction Condition and Racial

Table 2 Recall accuracy, Experiments 1 and 2

Condition	Low-Prejudice Group		High-Prejudice Group	
	<i>M</i>	<i>SE</i>	<i>M</i>	<i>SE</i>
Experiment 1				
No misinformation	.62	.04	.63	.03
No retraction	.69	.05	.74	.03
Retraction	.64	.04	.63	.05
Experiment 2				
No retraction	.61	.03	.57	.04
Retraction	.63	.02	.56	.04

Prejudice. The analysis revealed reliable main effects of retraction condition, $F(2, 138) = 43.81$, $MSE = 1.35$, $p < .001$, $\eta_p^2 = .39$, and racial prejudice, $F(1, 138) = 6.67$, $MSE = 1.35$, $p = .01$, $\eta_p^2 = .05$. The interaction between retraction condition and racial prejudice was not significant, $F(2, 138) = 1.58$, $p = .21$. These effects were confirmed in an ANOVA excluding the no-misinformation control condition, yielding significant effects of retraction type, $F(1, 92) = 16.21$, $MSE = 1.87$, $p < .001$, $\eta_p^2 = .15$, and racial prejudice, $F(1, 92) = 4.36$, $MSE = 1.87$, $p = .04$, $\eta_p^2 = .05$, but no significant interaction, $F(1, 92) = 2.01$, $p = .16$.

A number of planned contrasts, again with mean inference scores as the dependent variable, were conducted, so as to assess the overall effectiveness of the retraction and to further examine the relationship between retraction type and racial prejudice. These results are reported in Table 3. First, we assessed the difference between the no-retraction and retraction conditions: The retraction significantly reduced the number of references to the critical information in both racial-prejudice groups (Contrasts 1 and 2). We then investigated whether a significant reliance on misinformation would emerge after a retraction (i.e., a continued-influence effect) by contrasting the retraction condition with the no-misinformation baseline condition. Contrasts 3 and 4 showed significant continued reliance on misinformation in both racial-prejudice groups, despite the retraction. Finally, the effects of racial prejudice were investigated and shown only to be significant in the no-retraction condition (Contrasts 5–7), meaning that people who scored relatively high on racial prejudice mentioned the race of the suspects more often than did people in the low-prejudice group, but mainly when this information was explicitly supplied without being subsequently retracted.⁵

⁵ Repeating these analyses excluding participants who scored below 2 on the fact-recall questions ($n=3$) and participants from the retraction condition with a retraction-awareness score of 0 ($n=6$) did not substantially alter the pattern of results.

Discussion

In Experiment 1, we examined the relationship between racial attitudes and the continued influence of racial misinformation. In line with previous research, Experiment 1 revealed that a simple retraction significantly reduced but did not eliminate reliance on misinformation (cf. Ecker, Lewandowsky, & Tang, 2010). This means that even participants in the low-prejudice group failed to fully discount the Aboriginal misinformation after a retraction; they continued to make a significant number of references to the misinformation. This suggests that strategic memory processes failed to suppress automatic activation of race-related misinformation, even in people who arguably are not predisposed toward maintaining a belief in the misinformation (i.e., Aboriginal robbers).

In terms of racial prejudice effects, we found that people with relatively high levels of racial prejudice made more references to attitude-congruent racial information, as long as this information was explicitly given and not retracted. However, relatively high racial prejudice did not lead to a failure to discount attitude-incongruent misinformation. On the contrary, the retraction of racial misinformation was equally effective in both prejudice groups. This finding is in line with the reanalysis of the Ecker, Lewandowsky, and Apai (2011) data reported at the outset, and it suggests that racial attitudes do not influence how people process a retraction of racial misinformation. If anything, the present retraction tended to be numerically more effective in the high-prejudice group (i.e., when it was attitude-incongruent), resulting in both groups making equivalent numbers of references to Aboriginal misinformation after a retraction. In sum, the results provide evidence against the notion that people generally seek to reinforce their preexisting attitudes by dismissing the retraction of misinformation.

One factor that may have influenced these results—in particular, the lack of interaction between racial prejudice and the effectiveness of the retraction—is the use of a stereotypical scenario. The liquor-store scenario in Experiment 1 was congruent with negative stereotypes about Aboriginal people (Wimshurst et al., 2004), and strong innuendo (setting the incident in Australia's Northern Territory, a region with a large indigenous population, the use of Aboriginal place names, etc.) was intentionally used to boost the number of references to the critical information, in an attempt to avoid floor effects.⁶ Since the knowledge and use of stereotypes can be largely independent from people's attitudes (Devine & Elliot,

⁶ In contrast to previous studies, in which a central aspect of the scenario was retracted, such as the cause of a fire, the retraction in the present case concerned a relatively peripheral aspect of the scenario, and we hence expected a relatively low number of references to this critical piece of information.

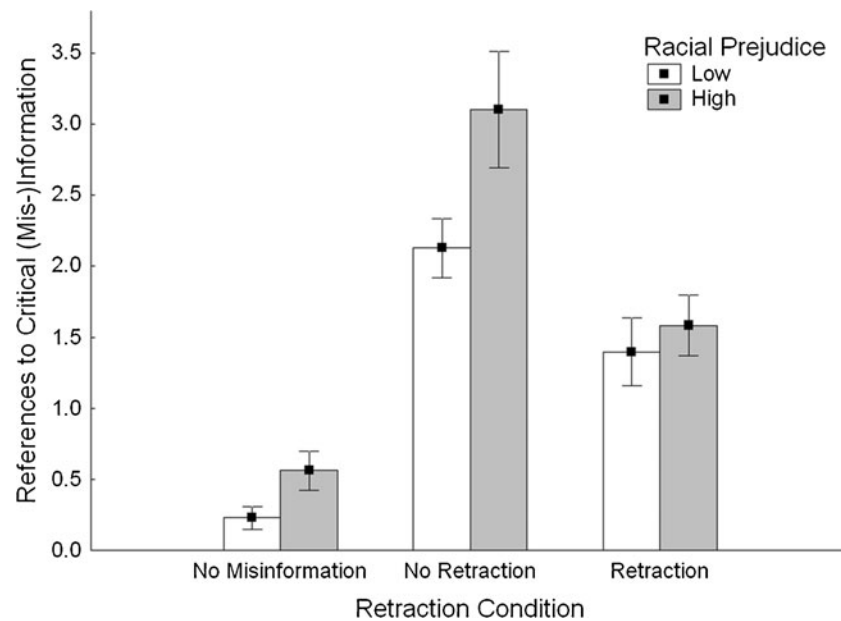


Fig. 1 Mean numbers of references to the critical (mis)information across conditions in Experiment 1. Error bars represent (unpooled) standard errors of the means

1995), participants in the low-prejudice group might have referred to the misinformation despite a retraction purely on the basis of the strong innuendo inherent in the story. This may have artificially inflated the level of postretraction misinformation reliance in the low-prejudice group, potentially masking an interaction involving level of prejudice.

Moreover, the racial information may have been more salient for people relatively high in racial prejudice, and this group's higher scores may simply reflect this difference in salience, rather than being directly related to their racial attitudes. Experiment 2 was designed to address these concerns by using a scenario that was incongruent with stereotypes.

Table 3 Contrasts, Experiment 1

Contrast	$F(1, 138)$	p
Effects of Retraction		
1 Low racial prejudice	4.73	.03*
2 High racial prejudice	20.58	<.001*
Continued-Influence Effects		
3 Low racial prejudice	12.11	.001*
4 High racial prejudice	9.27	.003*
Effects of Racial Prejudice		
5 No misinformation	<1	
6 No retraction	8.53	.004*
7 Retraction	<1	

* Significant, Holm–Bonferroni corrected

Experiment 2

Experiment 2 was similar to Experiment 1, but the piece of misinformation now related to an Aboriginal hero that prevented a robbery, thus running counter to common stereotypes about Indigenous Australians.

Method

Participants scoring high versus low on the ATIA racial prejudice questionnaire were presented with a news report about a bank robbery. We created two versions of the report. Both initially described a citizen preventing the robbery as an Aboriginal; this information was later retracted in one version of the report (the retraction condition) but not in the other (the no-retraction control condition). In contrast to Experiment 1, we omitted a third, no-misinformation control version, as we did not expect above-zero scores in such a condition with the counterstereotypical material used in this study. The no-retraction control condition therefore provided a ceiling level of references to the critical information, whereas the baseline was effectively zero. The experiment employed a 2 (racial prejudice: high vs. low) \times 2 (retraction condition: no retraction, retraction) between-subjects design.

Participants A priori power analysis (G*Power 3; Faul et al., 2007) suggested that in order to detect a medium-size effect of $\eta_p^2 = .1$ at $\alpha = .05$ and $1 - \beta = .80$, the minimum sample size should be 76. We tested a total of $N = 100$ undergraduate students from the University of Western Australia (69 females,

31 males; age range 17–36, mean age 19 years). Participants were sampled from a population of students prescreened with the ATIA ($N = 728$).⁷ The participants were divided into high and low racial prejudice groups on the basis of their ATIA scores and randomly assigned to the different retraction conditions ($n = 25$ per cell).

Stimuli Participants were given a report describing a fictitious attempted bank robbery, which was prevented by an Aboriginal man who disarmed the perpetrator. The report was presented as a series of 14 messages via a Microsoft PowerPoint presentation (see the [Appendix](#)). Each message was shown separately and displayed for a set amount of time (0.4 s per word). The reading time was derived from a pilot study on a different sample of undergraduate students ($N = 10$), and was calculated as the mean reading time plus three standard deviations; this allowed for comfortable reading while not providing excess slack time.

In both conditions, Message 5 stated that “a local Aboriginal resident . . . stepped in front of the main tellers and had convinced the robber to put down his gun.” The two versions of the story only differed at Message 11. In the retraction condition, Message 11 stated that “Police later released a second statement revealing that the man who helped apprehend the intruder was not an Aboriginal man as was first reported.” In the no-retraction condition, Message 11 only repeated information given earlier: “Police later released a second statement . . . confirming the intruder had been carrying a gun.”

Participants’ reliance on misinformation and their understanding of the story were assessed using a questionnaire containing ten inference questions, ten fact-recall questions, and two retraction-awareness questions (see the [Appendix](#)). Again, the inference questions were designed to elicit race-related responses, while also allowing for responses unrelated to the protagonist’s race. For example, the inference question “Why did police say the course of events was ‘unexpected’?” could be answered by referring to the Aboriginal man (e.g., because an Aboriginal hero may have been unexpected), or it could be answered without referring to the Aboriginal man (e.g., because not many people are brave enough to stand up to someone with a gun). The ten fact-recall questions had no relation to the race of the protagonist (e.g., “On which day did the incident occur?”). The retraction-awareness questions were identical to those in Experiment 1.

Procedure The procedure was identical to that of Experiment 1, with the three exceptions that (a) all participants were prescreened with the ATIA, (b) participants read the report

individually on a computer screen, not on paper, and (c) encoding time was fixed.

Results

Racial-prejudice scores ATIA scores ranged from 0.11 to 6 (i.e., nearly the full range of possible scores, 0–6). The mean racial prejudice scores were 1.54 ($SE = 0.10$) and 4.22 ($SE = 0.15$) in the low and high racial-prejudice groups, respectively. This was a significant difference, $F(1, 98) = 218.10$, $MSE = 0.82$, $p < .001$, $\eta_p^2 = .91$. The cell means are given in the lower half of Table 1.

Questionnaire coding Scoring was identical to that in Experiment 1. Again, interrater reliability was found to be very high ($r > .93$ for the inference, fact-recall, and retraction-awareness questions).

Accuracy of recall The mean rates of accuracy for the fact-recall responses (out of a maximum of 1) are given in the lower half of Table 2. A two-way ANOVA with the factors Retraction Condition and Racial Prejudice revealed no significant effects, $F_s < 2.52$, $p_s > .10$.

Awareness of retraction The mean rates of retraction awareness (out of a maximum of 1) were .58 ($SE = .07$) and .68 ($SE = .07$) for the high- and low-prejudice groups, respectively. This was not a significant difference, $F < 1$.

Inferential reasoning The mean inference scores for both the high- and low-prejudice groups across retraction conditions are shown in Fig. 2. As in Experiment 1, a retraction substantially reduced references to misinformation, and inference scores were higher when the critical information was attitude-congruent (i.e., in the low-prejudice group). In other words, a reversal of the stereotype also led to a reversal of the prejudice effect on inference scores.

We ran a two-way between-subjects ANOVA with the factors Retraction Condition and Racial Prejudice, which revealed reliable main effects of retraction condition, $F(1, 96) = 12.13$, $MSE = 1.67$, $p = .001$, $\eta_p^2 = .11$, and racial prejudice, $F(1, 96) = 6.52$, $MSE = 1.67$, $p = .01$, $\eta_p^2 = .06$. The interaction was not significant, $F < 1$. The results indicated that the retraction reduced the number of references to the critical information in both racial-prejudice groups, and that people in the low-prejudice group mentioned the race of the Aboriginal hero more often than did people in the high-prejudice group, both before and after a retraction.

Although the number of postretraction references to misinformation was clearly no different from zero in the

⁷ For pragmatic reasons related to delays in ethics approval and project deadlines, prescreening was done on three separate occasions, and participants were selected from the upper and lower quartiles of the three resulting distributions.

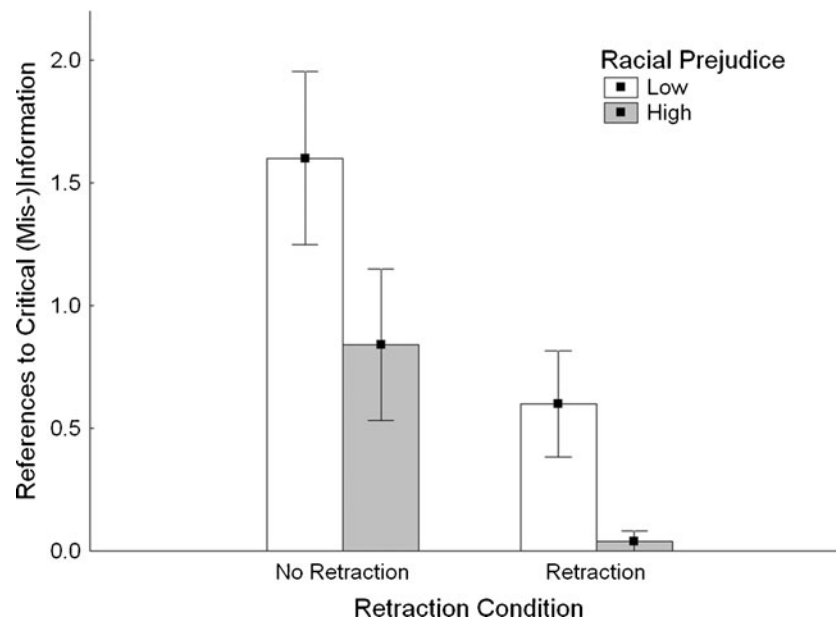


Fig. 2 Mean numbers of references to the critical (mis)information across conditions in Experiment 2. Error bars represent (unpooled) standard errors of the means

high-prejudice group ($t < 1$), the number was substantially above zero in the low-prejudice group, $M = 0.60$, $SE = 0.22$, $t(25) = 2.78$, $p = .01$. The number of references to the critical information was also above zero in the no-retraction condition of the high-prejudice group, $M = 0.84$, $SE = 0.31$, $t(25) = 2.72$, $p = .01$.⁸

Discussion

In Experiment 2, we examined the relationship between racial attitudes and the continued influence of racial misinformation, using a stereotype-incongruent scenario. As in Experiment 1, we again found that a simple retraction significantly reduced reliance on misinformation. Although a retraction did not eliminate reliance on misinformation in the low-prejudice group, the retraction did virtually eliminate misinformation effects in the high-prejudice group, meaning that we found no continued-influence effect in that group. In fact, only one person made a single reference to an Aboriginal “hero” in the high-prejudice group after the retraction (fittingly, this was accompanied by the assumption that the Aboriginal man was cooperating with the robber, an assertion that a number of participants in the high-prejudice group made).

Concerning the effects of racial prejudice, our analyses demonstrated that people scoring low in racial prejudice referred to an Aboriginal hero more often, in particular when no retraction was presented. As in Experiment 1, this shows that

more references to the critical information were made when this information was attitude-congruent. Since the scenario used in Experiment 2 did not conform to any relevant stereotypes, it seems very unlikely that stereotype-based responding had any impact on these results. Moreover, we argue that information regarding an Aboriginal hero would be more salient to a racially prejudiced person. It follows that the present effects, with higher scores in the low-prejudice group, cannot be explained by salience.

We do not believe that the obvious floor effect is reason for concern. First of all, we did not find a significant interaction between retraction and racial prejudice, despite the reduced variance associated with the floor effect. Second, the low range of inference scores was expected, given the counterstereotypical scenario. In particular, we expected the number of references to the critical information to go down to (almost) zero in the high-prejudice group. Our primary interest thus lay in the substantial reduction of inferences in the low-prejudice group, which was unaffected by a floor effect and numerically larger (about 60 %) than the reduction expected on the basis of the results of Experiment 1 (in which a retraction decreased references to the critical information only by about 50 % in the comparable high-prejudice group—i.e., the group for which the retraction was attitude-incongruent).

General discussion

In two experiments, we found that people use race-related information in their inferential reasoning mainly when this information is congruent with their attitudes. People scoring relatively high on a racial prejudice measure mentioned an Aboriginal

⁸ Repeating these analyses excluding participants who scored below 2 on the fact-recall questions ($n=3$) and participants from the retraction condition with a retraction-awareness score of 0 ($n=9$) did not substantially alter the result pattern.

crime suspect more often than people with low prejudice scores, whereas people with low prejudice scores mentioned an Aboriginal hero more often. The fact that the effects reversed when the scenario changed from a negative to a positive depiction of the Aboriginal person(s) is strong evidence that the usage of racial information was determined by people's attitudes and not by other factors, such as salience or reliance on stereotypes. This result provides experimental confirmation of survey-based research that has shown attitudes to be a major determinant of the information that people believe and use in their reasoning, be it the use of misinformation despite retractions (Casiday et al., 2006; Kull et al., 2003; Pedersen et al., 2005; Travis, 2010) or the (non)belief in empirical evidence and (non)support for actions on the basis of empirical evidence (e.g., Aldy, Kotchen, & Leiserowitz, 2012; Fielding, Head, Laffan, Western, & Hoegh-Guldberg, 2012).

In contrast, people's racial attitudes did not determine the effectiveness of retractions. Retractions reduced reliance on the critical information, but they did so equally for people in the high- and low-prejudice groups. In other words, the differences between prejudice groups were roughly equivalent across retraction and no-retraction groups. It is thus possible that any group differences found in surveys measuring belief in retracted misinformation may simply reflect pre-retraction belief differences.

Our results contrast with findings by Nyhan and Reifler (2010) and Nyhan et al. (2013), who reported that retractions were effective mainly when they were attitude-congruent, and could even backfire when they were attitude-incongruent, which is in line with research on motivated reasoning. Motivated reasoning is biased information processing that serves to confirm preexisting beliefs rather than to objectively assess the available evidence (for reviews of motivated cognition, see Kunda, 1990; Redlawsk, 2002; for a review from a misinformation perspective, see Ecker et al., *in press*). For example, Lord, Ross, and Lepper (1979) studied people who had strong opposing views on capital punishment and presented both groups with two fictional articles: one supporting and one refuting the claim that capital punishment reduces crime. Each group was more convinced by the article that supported their own beliefs, and after reading and discussing both articles, the two groups differed even more in their opposing views than before. Redlawsk (2002) and Redlawsk, Civetini, and Emmerson (2010) demonstrated how, at least up to a certain "tipping point," voters can increase their support for their favored political candidates when faced with negative information about them. The most common explanation for such behavior is that motivated reasoners counterargue attitude-incongruent information, in the process activating many arguments supporting their existing attitude ("attitude bolstering"; Jacks & Cameron, 2003; Prasad et al., 2009).

What reasons might underlie this discrepancy between our results and other results that have shown a strong effect of attitudes on the processing of discounting information? One

obvious methodological difference is that our study used fictional materials, whereas most of the studies discussed above used real-world materials. We suggest that this factor is unlikely to explain the observed differences on its own, given that Lord et al. (1979) used fictitious materials but nonetheless observed strong attitude effects. Arguably, as long as the fictional material is realistic and plausible, participants process it in a manner very similar to "real-world" information (cf. Kreitmann, 2006; Young, 2010).

Another difference between the two sets of studies is that our scenario involved a singular episodic event (i.e., a crime), whereas most of the other research has looked at belief in more general assertions (e.g., whether capital punishment deters crime; Lord et al., 1979). Again, this factor by itself is unlikely to explain the differences in outcomes, because attitude effects have also been found with singular episodic materials (e.g., whether President Bush misrepresented the effects of tax cuts; Nyhan & Reifler, 2010; see also Lewandowsky et al., 2005).

We thus propose three reasons for the discrepancy between our results and previous research reporting effects of attitudes on retraction processing: two related to the arguments of fictionality and singularity discussed above, and one related to the measurement of beliefs. First, it seems plausible to assume that real-world information is often encoded repeatedly before it is retracted, and may therefore require stronger retractions than information encoded only once (cf. Ecker et al., 2011). Because people arguably seek out information that is in line with their attitudes (cf. T. J. Johnson, Bichard, & Zhang, 2009; Kunda, 1990), they may encode attitude-congruent real-world myths more often than fictional misinformation presented in the lab.

Second, we suggest that attitude-incongruent retractions will be effective to the degree that they do not require attitude change. To illustrate, imagine a situation in which accepting a retraction will require attitude change: For a Republican supporting President Bush's tax cuts to acknowledge that Bush's claims about the tax cuts were incorrect (Nyhan & Reifler, 2010), it would require the person to accept that (a) a supported politician told an untruth and (b) it was a poor decision to support the associated policies. Those acknowledgments, in turn, would require a change in attitude regarding the tax cuts. Similarly, for a misinformed person, accepting evidence that goes against a misinformed belief—for example, evidence that vaccines do not cause autism—would inevitably require a shift in attitudes. There will thus be strong resistance to an attitude-incongruent retraction if accepting it would inevitably induce attitudinal change. Thus, one could continue to rely on misinformation whose correction would threaten one's worldview (Nyhan & Reifler, 2010, 2011).

In contrast, accepting that a particular crime was not committed by an Aboriginal person, or that a brave act was not performed by an Aboriginal, could be accepted without any change in attitude. In the case of the robbery in Experiment 1,

one could still believe that most liquor store robberies in Australia are perpetrated by Aboriginal people, and that most Aboriginal people are criminals. In fact, a single robbery committed by a non-Aboriginal person does not constitute any evidence against these beliefs. In the case of the attempted bank robbery in Experiment 2, one could still believe that most Aboriginal people are brave and fearless, despite the retraction that the hero was not an Aboriginal person; accepting the retraction does not constitute evidence against this belief. Hence, if accepting a retraction does not require a shift in attitudes, it will seemingly be followed even when it is attitude-incongruent.

Attitude-incongruent retractions may also be effective when people can use strategies to avoid attitude change. For example, people can accommodate exceptions to stereotypes, and thus maintain them by way of a process known as *stereotype subtyping* (Kunda & Oleson, 1995; Richards & Hewstone, 2001). This means, for example, that people with high racial prejudice might be able to accept an Aboriginal hero while maintaining their negative stereotype regarding Aboriginal people if they can identify a seemingly atypical attribute and use it to subtype the “deviant exemplar” (e.g., they might argue that Aboriginal people are usually criminal and coward, but that “true” Aboriginal people from “Outback” Australia may be braver; cf. Pedersen et al., 2004).

In the absence of such an atypical attribute, it seems the only group in the present experiments that was highly motivated to believe one event version over the other was the high-prejudice group of Experiment 2. Participants in this group may have struggled to accept an Aboriginal hero; hence, they were motivated to accept the attitude-congruent retraction. In fact, this was the first group of participants across all of our previous studies that did not show a continued-influence effect (i.e., they showed an elimination of misinformation effects, although one does have to consider the low overall rate of inferences). This group also showed indications of motivated reasoning, with some participants rationalizing that the Aboriginal man might have been an accomplice of the robber. Assuming that misinformation effects arise from a failure of strategic memory processes (cf. the introduction), this interpretation implies that an attitude-based motivation to believe one event version over another can lead to a boost in strategic monitoring—in this case, a high-prejudice group making sure to correct the initial attitude-incongruent event representation.

The third reason for the discrepancy between our results and other results suggesting attitude effects on the processing of retractions may lie in the difficulties that exist in the direct measurement of beliefs (e.g., in surveys). Clearly, what people *say* they believe and what they *actually* believe may be two different “animals,” in particular in nonintimate social interactions (cf. Fazio & Olson, 2003; Lamont, 2007). This means

that when people holding a certain belief are presented with convincing belief-incongruent evidence (such as a detailed refutation), they might change their belief in the direction of the evidence, but they might not overtly acknowledge this change. This could be an attempt to “save face,” or it could be to instill doubt in the person presenting the evidence as to how convincing the evidence is.

Typically, and as we briefly discussed earlier, backfire effects such as those reported by Nyhan and Reifler (2010) and Nyhan et al. (2013) are explained in terms of counterarguing, attitude bolstering, and other motivated reasoning processes. Common to those explanations is the idea that people’s beliefs change in a direction counter to the presented evidence. An alternative view on backfire effects would hold that rather than representing attitude change, they reflect people’s attempts to defend and maintain their attitudes and beliefs. On this view, backfire effects are an occasional and inadvertent consequence of an overzealous attempt to maintain an attitude and protect it against change. Some support for this alternative explanation has come from a study by Gal and Rucker (2010). These authors argued that even though people often express their beliefs more vigorously following disconfirming evidence, this may be an ironic effect of reduced confidence in those beliefs. Specifically, people may have reduced their level of belief in line with the evidence, but the ensuing reduction in confidence in that belief may be threatening to one’s self-concept. In consequence, increased overt advocacy of the belief may be required to fend off this threat. Consonant with this idea, Gal and Rucker found that participants engaged in stronger belief advocacy when they were less confident of their beliefs.

To illustrate, in social interactions both empirical arguments and the expression of one’s belief can be used in an effort to convince others of one’s attitude or to defend one’s attitudes against persuasion; in this sense, attitudes are based on evidence and/or beliefs. This implies that when people (secretly) acknowledge that the real-world evidence landscape has shifted against their belief, they might adjust their belief, and potentially the underlying attitude, in the direction of the evidence. Yet, at the same time, they might move their *expressed* belief in the opposite direction, in order to maintain a perceived balance of evidence and expressed belief in favor of their initial attitude (see also Batson, 1975, for a similar argument). As long as a positive (i.e., attitude-congruent) balance can be maintained—that is, as long as negative (i.e., attitude-incongruent) evidence can be counterbalanced by positive increments in expressed belief—the attitude can be defended and (ostensibly) maintained. If the negative evidence becomes overwhelming, however, a “tipping point” might be reached, at which point the expressed belief cannot sensibly be increased any more. At this point, attitude change may occur and also be acknowledged (cf. Redlawsk et al., 2010).

Conclusion

In two experiments, we showed that preexisting attitudes codetermine people's reliance on (mis)information. That is, people are more likely to use a piece of information in their reasoning when this piece of information is congruent with their attitudes and beliefs. Unlike some previous research, however, we found that the effectiveness of retractions of misinformation was not affected by attitudes. That is, people's attitudes did not affect the extent to which a retraction reduced their reliance on a piece of attitude-relevant misinformation. To reconcile this finding with the existing literature, we suggested that the effectiveness of attitude-incongruent retractions will depend on whether or not accepting the retraction will induce a requirement to change the underlying attitude: When accepting a retraction does not require change in underlying attitudes, it will not be rejected for attitudinal reasons; when a retraction does challenge people's underlying attitudes, they will resist it.

Author note This research was facilitated by a Discovery Grant and an Australian Postdoctoral Fellowship from the Australian Research Council to the first author, and a Discovery Grant and a Discovery Outstanding Researcher Award from the Australian Research Council to the second author. We thank Charles Hanich and Devon Spaapen for research assistance, and Nic Fay for suggesting the stereotype-incongruent scenario used in Experiment 2. The lab Web address is www.cogsciwa.com.

References

- Aldy, J. E., Kotchen, M. J., & Leiserowitz, A. A. (2012). Willingness to pay and political support for a US national clean energy standard. *Nature Climate Change*, *2*, 596–599.
- Ayers, M. S., & Reder, L. M. (1998). A theoretical review of the misinformation effect: Predictions from an activation-based memory model. *Psychonomic Bulletin & Review*, *5*, 1–21.
- Batson, C. D. (1975). Rational processing or rationalization? Effect of disconfirming information on a stated religious belief. *Journal of Personality and Social Psychology*, *32*, 176–184.
- Berinsky, A. J. (2012). *Rumors, truths, and reality: A study of political misinformation*. Unpublished manuscript.
- Bizer, G. Y., Tormala, Z. L., Rucker, D. D., & Petty, R. E. (2006). Memory-based versus on-line processing: Implications for attitude strength. *Journal of Experimental Social Psychology*, *42*, 646–653.
- Bjork, E. L., & Bjork, R. A. (1996). Continuing influences of to-be-forgotten information. *Consciousness and Cognition*, *5*, 176–196. doi:10.1006/ccog.1996.0011
- Casiday, R., Cresswell, T., Wilson, D., & Panter-Brick, C. (2006). A survey of UK parental attitudes to the MMR vaccine and trust in medical authority. *Vaccine*, *24*, 177–184.
- Dechêne, A., Stahl, C., Hansen, J., & Wänke, M. (2010). The truth about the truth: A meta-analytic review of the truth effect. *Personality and Social Psychology Review*, *14*, 238–257.
- Devine, P., & Elliot, A. (1995). Are racial stereotypes really fading? The Princeton Trilogy revisited. *Personality and Social Psychology Bulletin*, *21*, 1139–1150.
- Ecker, U. K. H., Lewandowsky, S., & Apai, J. (2011). Terrorists brought down the plane!—No, actually it was a technical fault: Processing corrections of emotive information. *Quarterly Journal of Experimental Psychology*, *64*, 283–310.
- Ecker, U. K. H., Lewandowsky, S., Oberauer, K., & Chee, A. E. H. (2010). The components of working memory updating: An experimental decomposition and individual differences. *Journal of Experimental Psychology: Learning Memory and Cognition*, *36*, 170–189.
- Ecker, U. K. H., Lewandowsky, S., Swire, B., & Chang, D. (2011). Correcting false information in memory: Manipulating the strength of misinformation encoding and its retraction. *Psychonomic Bulletin & Review*, *18*, 570–578. doi:10.3758/s13423-011-0065-1
- Ecker, U. K. H., Lewandowsky, S., & Tang, D. T. W. (2010). Explicit warnings reduce but do not eliminate the continued influence of misinformation. *Memory & Cognition*, *38*, 1087–1100. doi:10.3758/MC.38.8.1087
- Ecker, U. K. H., Swire, B., & Lewandowsky, S. (in press). Correcting misinformation—A challenge for education and cognitive science. In D. N. Rapp & J. Braasch (Eds.), *Processing inaccurate information: Theoretical and applied perspectives from cognitive science and the educational sciences*. Cambridge, MA: MIT Press.
- Faul, F., Erdfelder, E., Lang, A.-G., & Buchner, A. (2007). G*Power 3: A flexible statistical power analysis program for the social, behavioral, and biomedical sciences. *Behavior Research Methods*, *39*, 175–191. doi:10.3758/BF03193146
- Fazio, R. H., & Olson, M. A. (2003). Implicit measures in social cognition research: Their meaning and use. *Annual Review of Psychology*, *54*, 297–327.
- Fielding, K. S., Head, B. W., Laffan, W., Western, M., & Hoegh-Guldberg, O. (2012). Australian politicians' beliefs about climate change: Political partisanship and political ideology. *Environmental Politics*, *21*, 712–733.
- Gal, D., & Rucker, D. D. (2010). When in doubt, shout! Paradoxical influences of doubt on proselytizing. *Psychological Science*, *21*, 1701–1707.
- Garrett, R. K., Nisbet, E. C., & Lynch, E. K. (2013). Undermining the corrective effects of media-based political fact checking? The role of contextual cues and naïve theory. *Journal of Communication*, *63*, 617–637. doi:10.1111/jcom.12038
- Gilbert, D. T., Krull, D., & Malone, P. (1990). Unbelieving the unbelievable: Some problems in the rejection of false information. *Journal of Personality and Social Psychology*, *59*, 601–613.
- Hart, P. S., & Nisbet, E. C. (2012). Boomerang effects in science communication: How motivated reasoning and identity cues amplify opinion polarization about climate mitigation policies. *Communication Research*, *39*, 701–723.
- Hastie, R., & Park, B. (1986). The relationship between memory and judgment depends on whether the judgment task is memory-based or on-line. *Psychological Review*, *93*, 258–268.
- Jacks, J. Z., & Cameron, K. A. (2003). Strategies for resisting persuasion. *Basic and Applied Social Psychology*, *25*, 145–161.
- Jacobson, G. C. (2010). Perception, memory, and partisan polarization on the Iraq War. *Political Science Quarterly*, *125*, 31–56.
- Johnson, T. J., Bichard, S. L., & Zhang, W. (2009). Communication communities or “cyberghettos?” A path analysis model examining factors that explain selective exposure to blogs. *Journal of Computer-Mediated Communication*, *15*, 60–82.
- Johnson, M. K., Hashtroudi, S., & Lindsay, D. S. (1993). Source monitoring. *Psychological Bulletin*, *114*, 3–28. doi:10.1037/0033-2909.114.1.3
- Johnson, H. M., & Seifert, C. M. (1994). Sources of the continued influence effect: When misinformation in memory affects later inferences. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *20*, 1420–1436. doi:10.1037/0278-7393.20.6.1420
- Kendeou, P., & O'Brien, E. J. (in press). The Knowledge Revision Components (KReC) framework: Processes and mechanisms. In D. N. Rapp & J. Braasch (Eds.), *Processing inaccurate information:*

- Theoretical and applied perspectives from cognitive science and the educational sciences*. Cambridge, MA: MIT Press.
- Kreitmann, N. (2006). Fantasy, fiction, and feelings. *Metaphilosophy*, *37*, 605–622.
- Kull, S., Ramsay, C., & Lewis, E. (2003). Misperceptions, the media, and the Iraq war. *Political Science Quarterly*, *118*, 569–598.
- Kunda, Z. (1990). The case for motivated reasoning. *Psychological Bulletin*, *108*, 480–498. doi:10.1037/0033-2909.108.3.480
- Kunda, Z., & Oleson, K. C. (1995). Maintaining stereotypes in the face of disconfirmation: Constructing grounds for subtyping deviants. *Journal of Personality and Social Psychology*, *68*, 565–579. doi:10.1037/0022-3514.68.4.565
- Lamont, P. (2007). Paranormal belief and the avowal of prior scepticism. *Theory and Psychology*, *17*, 681–696.
- Lewandowsky, S., Ecker, U. K. H., Seifert, C. M., Schwarz, N., & Cook, J. (2012). Misinformation and its correction: Continued influence and successful debiasing. *Psychological Science in the Public Interest*, *13*, 106–131.
- Lewandowsky, S., Stritzke, W. G. K., Oberauer, K., & Morales, M. (2005). Memory for fact, fiction, and misinformation: The Iraq War 2003. *Psychological Science*, *16*, 190–195. doi:10.1111/j.0956-7976.2005.00802.x
- Loftus, E. F. (2005). Planting misinformation in the human mind: A 30-year investigation of the malleability of memory. *Learning and Memory*, *12*, 361–366. doi:10.1101/lm.94705
- Loken, B., & Hoverstad, R. (1985). Relationships between information recall and subsequent attitudes: Some exploratory findings. *Journal of Consumer Research*, *12*, 155–168.
- Lord, C., Ross, L., & Lepper, M. (1979). Biased assimilation and attitude polarization: The effects of prior theories on subsequently considered evidence. *Journal of Personality and Social Psychology*, *37*, 2098–2109.
- Nyhan, B., & Reifler, J. (2010). When corrections fail: The persistence of political misperceptions. *Political Behavior*, *32*, 303–330.
- Nyhan, B., & Reifler, J. (2011). *Opening the political mind? The effects of self-affirmation and graphical information on factual misperceptions*. Unpublished manuscript.
- Nyhan, B., Reifler, J., & Ubel, P. A. (2013). The hazards of correcting myths about health care reform. *Medical Care*, *51*, 127–132. doi:10.1097/MLR.0b013e318279486b
- Oberauer, K., & Vockenberg, K. (2009). Updating of working memory: Lingering bindings. *Quarterly Journal of Experimental Psychology*, *62*, 967–987. doi:10.1080/17470210802372912
- Pedersen, A., Attwell, J., & Heveli, D. (2005). Prediction of negative attitudes toward Australian asylum seekers: False beliefs, nationalism, and self-esteem. *Australian Journal of Psychology*, *57*, 148–160.
- Pedersen, A., Beven, J. P., Walker, I., & Griffiths, B. (2004). Attitudes toward Indigenous Australians: The role of empathy and guilt. *Journal of Community and Applied Social Psychology*, *14*, 233–249.
- Prasad, M., Perrin, A. J., Bezila, K., Hoffman, S. G., Kindleberger, K., Manturuk, K., & Powers, A. S. (2009). “There must be a reason”: Osama, Saddam, and inferred justification. *Sociological Inquiry*, *79*, 142–162.
- Ratzan, S. C. (2010). Editorial: Setting the record straight: Vaccines, autism, and The Lancet. *Journal of Health Communication*, *15*, 237–239.
- Redlawsk, D. P. (2002). Hot cognition or cool consideration? Testing the effects of motivated reasoning on political decision making. *Journal of Politics*, *64*, 1021–1044.
- Redlawsk, D. P., Civettini, A. J. W., & Emmerson, K. M. (2010). The affective tipping point: Do motivated reasoners ever “get it”? *Political Psychology*, *31*, 563–593.
- Richards, Z., & Hewstone, M. (2001). Subtyping and subgrouping: Processes for the prevention and promotion of stereotype change. *Personality and Social Psychology Review*, *5*, 52–73.
- Schwarz, N., Sanna, L. J., Skurnik, I., & Yoon, C. (2007). Metacognitive experiences and the intricacies of setting people straight: Implications for debiasing and public information campaigns. In M. P. Zanna (Ed.), *Advances in experimental social psychology* (Vol. 39, pp. 127–161). San Diego, CA: Academic Press. doi:10.1016/S0065-2601(06)39003-X
- Travis, S. (2010). *CNN poll: Quarter doubt Obama was born in U.S.* Retrieved from <http://politicalticker.blogs.cnn.com/2010/08/04/cnn-poll-quarter-doubt-president-was-born-in-u-s/>
- Wilkes, A. L., & Leatherbarrow, M. (1988). Editing episodic memory following the identification of error. *Quarterly Journal of Experimental Psychology: Human Experimental Psychology*, *40*, 361–387. doi:10.1080/02724988843000168
- Wimshurst, K., Marchetti, E., & Allard, T. (2004). Attitudes of criminal justice students to Australian indigenous people: Does higher education influence student perceptions? *Journal of Criminal Justice Education*, *15*, 327–350.
- Young, G. (2010). Virtually real emotions and the paradox of fiction: Implications for the use of virtual environments in psychological research. *Philosophical Psychology*, *23*, 1–21.