

RUNNING HEAD: MISINFORMATION AND RESOLUTION

Trace-Strength and Source-Monitoring Accounts of Accuracy
and Metacognitive Resolution in the Misinformation Paradigm

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Abstract

Two experiments are reported that investigate the impact of misinformation on memory accuracy and metacognitive resolution. In Experiment 1, participants viewed a series of photographs depicting a crime scene, were exposed to misinformation that contradicted details in the slides, and later took a recognition memory test. For each answer, participants were required to indicate whether they were willing to testify (report) their answer to the Court and to rate confidence. Misinformation impaired memory accuracy but it had no effect on resolution, regardless of whether resolution was indexed with confidence-rating measures (gamma correlation and mean confidence) or a report-option measure (type-2 discrimination: d'). In Experiment 2, a similar accuracy-confidence dissociation was found, and the misinformation effect occurred mostly with fine-grained responses, suggesting that responding was based on recollected details. We argue that the results support source-monitoring accounts of accuracy and resolution rather than accounts based on trace strength.

Keywords: misinformation, suggestibility, metacognitive monitoring, resolution, source monitoring, strength, confidence

Trace-Strength and Source-Monitoring Accounts of Accuracy and Metacognitive Resolution
in the Misinformation Paradigm

For over 30 years, psychologists have investigated the effect of post-event misinformation on memory accuracy (see Loftus, 2005 for a recent review). This research has shown that when people are exposed to misleading details that contradict or supplement actual information contained in a witnessed event, they often falsely recognize or recall those details on a later memory test. The fact that people make these types of errors has not been contested. However, the mechanisms that are at the root of the effect have been the subject of considerable debate (e.g., see Bekerian & Bowers, 1983; Belli, 1989; Christiansen & Ochalek, 1983; Lindsay, 1990; Lindsay & Johnson, 1989; Loftus, Miller, & Burns, 1978; McCloskey & Zaragoza, 1985).

A related topic is the effect that misinformation has on confidence. When people wrongly accept misinformation, are they sceptical about its veracity or is the new “memory” accepted with assurance? Several studies have reported the latter, with participants being more confident in incorrect responses involving the acceptance of misinformation than in other types of incorrect responses, and sometimes having more confidence in erroneous misinformation than in correct responses (e.g., see Cole & Loftus, 1979; Greene, Flynn, & Loftus, 1982; Loftus, Donders, Hoffman, & Schooler, 1989; Loftus et al., 1978; Luna & Migueles, 2009; Ryan & Geiselman, 1991; Vornik, Sharman, & Garry, 2003).

These findings with confidence suggest that misinformation impairs *resolution*, that is, the extent to which subjective confidence ratings (assigned to different items by an individual participant) discriminate correct answers from incorrect ones. If confidence assigned to suggested details is as high (or higher) than confidence for correct details, then participants will have difficulty using confidence to discriminate the accuracy of their answers. Surprisingly, very few studies have directly investigated the effect of misinformation on resolution, but the few that have been published unanimously confirm this prediction. For example, Weingardt, Leonesio, and Loftus (1994) presented participants with a video of a liquor store robbery and later misled them in a written narrative about two of four critical details. Finally, a 2AFC recognition test was administered which required a confidence rating (ranging from 50% to 100%) that their decision was correct. Weingardt et al. found that misinformation impaired recognition accuracy, but more importantly, analyses of the confidence data indicated that resolution was also significantly impaired. In fact, in their Experiment 1,

participants demonstrated something akin to negative resolution: Misleading items rated as having chance-level accuracy (50%) had actual accuracy over 60%, whereas other misleading items rated with a higher level of confidence (70%, 90%, and 100%) had actual accuracy at about chance level (50%). Tomes and Katz (2000) presented similar findings: Whereas the Goodman-Kruskal gamma correlation (G) between confidence and accuracy in their study was .61 for control items, it plummeted to a mere .05 for misinformation items.¹ These results led Tomes and Katz to the strong conclusion that, “When misinformation is presented, confidence becomes useless as an indicator of veracity” (p. 279). Finally, Bonham and González-Vallejo (2009) replicated Tomes and Katz pattern of results using the same materials. In their Experiment 1, G was much greater for control items (.47) than misinformation items (-.03), demonstrating pronounced resolution impairment.²

That resolution is severely impaired by misinformation is unfortunate news for the legal system. If it is the case confidence truly is “a useless indicator of veracity” (Tomes & Katz, 2000, p. 279), then various mechanisms that could have regulated misinformation’s effect on memory retrieval are no longer useful. For example, by using a report option, an eyewitness in a courtroom whose memory has been affected by misinformation could potentially achieve the same accuracy rate as an eyewitness who has not been misled (Goldsmith & Koriat, 2008; Higham, 2002, 2007; Koriat & Goldsmith, 1996). By saying “I don’t know” to questions associated with low-confidence candidate answers, and only reporting to the Court candidate answers that have high confidence, high accuracy of reported information can be achieved even if retrieval is poor. However, this high accuracy can only be achieved if confidence is valid predictor of accuracy. If it is not, such regulatory mechanisms are ineffectual, and possibly even harmful.

At a theoretical level, why might confidence in suggested details match or exceed confidence in correct details resulting in poor resolution? One class of models might account for this effect by examining the memory strength of the alternatives under consideration (e.g., Dougherty, Gettys, & Ogden, 1999; Dougherty, 2001). For control test items in a typical misinformation study using a two-alternative recognition task, the person must discriminate between a new-incorrect alternative not presented in the experiment and an old-correct alternative presented in the event (see top panel of Figure 1). Hence, there will be a large mean strength difference between these two alternatives, with the old-correct alternative having higher mean strength than the new-incorrect alternative by virtue of the fact that it was presented earlier. However, for misinformation test items, both the correct and

incorrect alternatives are old: the correct one was presented in the event whereas the incorrect one was presented in the post-event. Consequently, both alternatives should have comparable strengths (see bottom panel of Figure 1). To make a recognition decision, it might simply be assumed that the chosen alternative on a given trial is the one with the greater strength. One might also assume that confidence corresponds to the strength of the chosen item (i.e., higher strength of the chosen alternative corresponds to greater confidence). Under these assumptions, a simple trace-strength model would predict that misinformation will impair accuracy, and if the incorrect (suggested) alternative is chosen for a misinformation item, it will be assigned high confidence. The impairment of accuracy occurs because, compared to the control items, there is a higher likelihood for misinformation items that the incorrect (suggested) alternative will exceed the strength of the correct alternative and be chosen erroneously. High confidence is expected because, if the incorrect (suggested) alternative is chosen, it will have high strength, necessarily higher than the strength of the competing correct alternative (otherwise the correct alternative would have been chosen). The trace-strength model also makes a clear prediction for resolution. For misinformation items, because the strength of incorrect (suggested) alternatives is as high or even higher than the strength of the correct alternative, and because confidence is based on trace strength, confidence will not be useful as an accuracy discriminator, resulting in poor resolution (Figure 1).

Although a simple trace-strength model accounts nicely for the extant literature demonstrating the negative effect of misinformation on resolution, there is ample evidence from other misinformation studies that people do not always base memory decisions on the mere strength of the recognition alternatives. For example, accounts based on *source monitoring* (e.g., Higham, 1998; Johnson, Hashtroudi, & Lindsay 1993; Lindsay, 1990, 2008; Lindsay & Johnson, 1989; Zaragoza & Lane, 1994) posit that recollection of memories' *details* (e.g., perceptual information) is used to make source discriminations. Thus, although the incorrect (suggested) alternative may have strength that is comparable to that of the correct alternative for misinformation test items, it may be offered with low confidence, or rejected altogether, because some recollected memory information designates it to the post-event rather than the event. For example, if there is retrieval of an alternative's typeface as it was presented in written form in the post-event narrative, the person might infer that the source of the alternative is the narrative and not the photographic slide sequence.

People do not automatically engage in source monitoring, and their willingness to do so is at

least partly under strategic control and influenced by task demands. For example, Lindsay & Johnson (1989) found that a source-monitoring test that demanded participants to indicate the source of their memories yielded better performance than a simple recognition test. Presumably, had participants spontaneously monitored source during recognition to the same extent as during the source-monitoring test, this performance difference would not have been observed. More generally, this result suggests that if participants are to monitor source, they must be encouraged to do so. Otherwise, they may respond merely to the strength of the alternatives and show serious deficits in both accuracy and resolution if misinformation is presented, as the trace-strength model predicts.

Critically, close scrutiny of the research that has demonstrated misinformation's impairment to resolution indicates quite clearly that participants were not encouraged to monitor source. First, recognition tests were used, which, as just mentioned, are not as effective as source-monitoring tests for promoting source discrimination. Second, and perhaps more crucially, none of the participants in any of the studies were warned directly that they had been misled either before or after the event. Weingardt et al. (1994) at least specified to participants that they should base their memory-test responses on what they saw, but neither Tomes and Katz (2000) nor Bonham and González-Vallejo (2009) even included this instruction. Together, recognition testing and the lack of an experimenter warning may have encouraged lax source monitoring and overreliance on strength to make judgments, resulting in the poor resolution for misinformation items that was observed.

We report two experiments on the misinformation effect in this paper. To test the hypothesis presented above, both experiments incorporate some design features intended to encourage rather than discourage source monitoring, but which may be considered unusual for research on the misinformation effect. First, all participants received a clear warning that they had been exposed to misleading information prior to witnessing the event (Experiment 1) and/or prior to writing the memory test (Experiments 1 and 2). Although others have used warnings in misinformation studies (e.g., Chambers & Zaragoza, 2001; Lindsay, 1990) the nature and extent of our warnings were somewhat atypical as explained below. Second, we used numerous critical items per participant: 30 (15 control; 15 misinformation) in Experiment 1 and 24 (12 control; 12 misinformation) in Experiment 2. A large number of critical items allowed us to obtain reasonable estimates of resolution and it promoted the use of a wide range on the confidence and accuracy scales per participant. Third, we provided participants with an option to "withhold" their answer, but unlike previous research incorporating such

an option (e.g., Blank, 1998; Roebbers & Schneider, 2005), an answer was still required even if it was chosen (i.e., a best guess). Previous research has shown that such an option with forced output can be used to compute the hit rate (HR) and the false alarm rate (FAR) using type-2 signal detection theory (SDT; e.g., see Higham, 2002, 2007; Higham & Arnold, 2007a, 2007b; Higham & Tam, 2005; see also Koriat & Goldsmith, 1996). These rates were then used to derive an estimate of resolution (d') that could be used along with the more common resolution measures: G and mean confidence. Fourth, in an attempt to distinguish between the trace-strength and source-monitoring accounts, we presented the correct-control details twice to participants in Experiment 2, once in the slides and once in the post-event narrative. As will become clear, the two accounts offer opposite predictions for the effect this will have on resolution. Finally, in an attempt to ascertain the nature of the memory errors we observed in Experiment 1, we designed a unique test that contained both coarse- and fine-grained answers in Experiment 2.

Experiment 1

Method

Participants. Participants were 6 male and 12 female ($n = 18$) psychology students at the University of Southampton who completed the study for course credits. Ages ranged from 19 to 26 years ($M = 20.78$ years, $SD = 1.59$ years). Participants were placed into two counterbalance groups as outlined below.

Design and materials. The experiment was a 2 (testify: yes/no) x 2 (item type: misinformation/control) within-subjects design. The crime scene consisted of 15 slides and 15 corresponding narratives. The narratives contained 30 critical details, two pertaining to each slide in the crime-scene sequence. One version of each critical detail (misinformation) misrepresented the detail in the slide, whereas the other version (control) either omitted the misinformation, or described the detail in neutral form. For any given participant, half the critical details occurred in their misinformation form and half in their control form, with the assignment counterbalanced across participants. Misinformation appeared evenly throughout the slide sequence for each crime scene; that is, each narrative contained one piece of misinformation and one control version of another critical detail.

A 30-item two-alternative recognition memory test was constructed and made into booklets. Each booklet contained a page of instructions followed by five pages of questions. For each question,

there were spaces to write an answer (A or B), a decision about testifying (Y/N), and a confidence rating about the accuracy of each response (50-100%). Each question on the test queried a critical detail, with two questions (and critical details) per slide. The questions appeared in chronological order, starting with questions about slide 1 and ending with questions about slide 15. Across questions, option A versus B represented the correct answer 14 versus 16 times, respectively.

Procedure. Participants entered the lab and were seated in front of an Apple 24-inch iMac computer, which was used to present the crime-scene slides and narratives using Apple Keynote software. The first two slides contained instructions that informed participants that they would be shown a series of slides and written descriptions depicting a murder scene. They should study the slides (8 seconds each) and descriptions (roughly 15 seconds each) closely, but that the narratives would contain some misinformation. They were also told that it was vital that any information they provide be accurate and informative and to ask any questions before proceeding. Participants then viewed the slides one at a time, with each slide filling the computer monitor. Eight seconds was allotted to study each slide, after which the screen went blank, then a written narrative appeared which described the details of the slide. The narratives corresponding to each slide ranged in length depending on the amount of detail depicted. Longer display times were allowed for longer narratives so that all participants could finish reading them (range: 15-25 seconds).

The 15 slides were digital photographs of a staged murder. The photographs showed the perpetrator's car leaving the crime scene, the victim's home, a knife, and the victim's body. Two versions of each narrative were created so that the control and misinformation versions of the critical details could be presented and counterbalanced. The photographs remained in the same order for both versions.

After the slide show and narratives for a given crime scene had been presented, participants were given a Sudoku puzzle to complete for 10 minutes as a distractor task. Then they were administered a booklet containing the questions for the recognition memory test. While writing the test, participants were asked to imagine that they were in a courtroom. They were instructed to answer all questions, but to indicate for each question whether they were confident enough to offer their answer as testimony in court by circling either "Y" or "N" next to a "Testify" prompt. They were reminded that the narratives read earlier contained some inaccurate details and therefore not to assume that if a detail from one of the narratives was remembered that it was guaranteed to be

correct; to perform well on the test, they must accurately remember what happened on the slides, which may or may not correspond to the account given in the narratives.

Additionally, they were provided with the following instructions:

There are 30 questions in total, each with two alternative answers. Fifteen of these questions relate to details about which you have been misinformed. In other words, a narrative that you read contained misleading information about that detail, so you have to be very careful when answering these questions. For these questions, one alternative is correct (i.e., it appeared only in the slides) whereas the other is incorrect (i.e., it was read about only in the narrative). The other 15 questions relate to details about which you have received no misinformation. In other words, the narrative did not contain misleading information about that detail. For these questions, one alternative is correct (i.e., it appeared only in the slides) whereas the other is incorrect (i.e., it is a new detail). Use this information to help you to answer the questions correctly and make decisions about which answers to use in your testimony.

Results

Alpha level of .05 was adopted for all statistical tests reported in this paper.

Response likelihoods. Participants testified just over half of their answers and there was no difference in the testify rates for the different item types, $F < 1$, (control: $M = .53$, $SE = .03$; misinformation: $M = .54$, $SE = .04$).

Accuracy. Mean accuracy as a function of item type (misinformation/control) and report option (testify/withhold) is shown in Table 1. A 2 X 2 repeated-measures Analysis of Variance (ANOVA) on these accuracy data revealed a main effect of item type, $F(1,17) = 6.94$, $MSE = 0.024$, $\eta^2 = .29$ and a main effect of report option, $F(1,17) = 117.77$, $MSE = 0.018$, $\eta^2 = .87$. Accuracy was higher for control items ($M = .77$, $SE = .03$) than misinformation items ($M = .67$, $SE = .03$) and it was higher for testified responses ($M = .89$, $SE = .03$) than withheld responses ($M = .55$, $SE = .03$). The interaction was not significant, $F < 1$.

Resolution. To investigate the effect of misinformation on resolution, we first computed G (Table 2), and then we compared mean confidence assigned to correct versus incorrect responses (Table 3). For both of these analyses, item type (control/misinformation) was included as a variable in the analysis but the data were collapsed across the report-option variable.³

If resolution was impaired by misinformation, then G should be less for misinformation items

than control items. However, as shown in Table 2, G was remarkably similar between the item types, $F < 1$, although it was well above chance (0) for both, lower -95% confidence interval = .37.

For the analysis on mean confidence, an impairment of resolution by misinformation would be indicated by an interaction between accuracy and item type (i.e., a smaller difference in confidence ratings assigned to correct versus incorrect responses for misinformation items compared to control items). This interaction from a 2 (item type: control/misinformation) X 2 (accuracy: correct/incorrect) within subjects ANOVA was not significant, $F < 1$. Instead, this analysis revealed only a significant main effect of accuracy, $F(1,16) = 33.67$, $MSE = 219.44$, $\eta^2 = .68$: mean confidence was higher for correct answers ($M = 79.61$, $SE = 1.97$) than incorrect ones ($M = 58.77$, $SE = 4.62$), indicating that resolution was reasonably good. The main effect of item type was not significant, $F(1,16) = 1.93$, $MSE = 61.67$, $\eta^2 = .11$.

Finally, the report-option variable was used to compute the type-2 HR and FAR and d' (Table 4).⁴ The type-2 HR and FAR are defined as the proportion of correct and incorrect responses that are testified, respectively, and the standardized difference between them (d') can be used to index resolution (e.g., see Higham, 2007). In particular if the HR is high whereas the FAR is low, there is good discrimination between correct and incorrect answers, and d' will be high. Conversely, if these rates are equivalent, then there is failure to discriminate accurate from inaccurate responses, and d' will equal zero. Because the testify decision was made separately from the confidence rating, investigating resolution in this way has the advantage of providing a converging test of the effect of misinformation on resolution, one that is not based on confidence data. If misinformation adversely affected resolution, there would be a reduction in d' for misinformation items compared to control items (i.e., a reduction in accuracy discrimination). However, a one-way within-subjects ANOVA indicated that this difference was not significant, $F < 1$ (Table 4). As with the previous measures of resolution based on confidence ratings, d' was well above chance for both item types, lower -95% confidence interval = 0.65.

Discussion

Despite warnings given both pre- and post-event and instructions given prior to the test indicating the proportion of control versus misinformation test items, a significant misinformation effect on accuracy was obtained. However, this effect was not accompanied by a reduction in resolution. Regardless of whether resolution was indexed with confidence ratings (G , mean confidence) or by

type-2 discrimination based on testify decisions (d'), no evidence of a misinformation impairment on resolution was obtained. This null effect was not attributable to noisy, low levels of resolution overall. Indeed, resolution was surprisingly high regardless of the particular measure chosen; G exceeded .50 for both misinformation and control items, there was a sizeable difference in confidence ratings assigned to correct versus incorrect answers, and d' was well above chance (see Tables 2, 3, and 4). Thus, the results of Experiment 1 support the contention that if participants are clearly informed that they have been misled, resolution remains quite high and intact despite the misinformation and its effect on accuracy.

Accuracy was higher for testified answers than withheld ones (Table 1), indicating that participants were able to regulate accuracy. This difference is consistent with the analyses on resolution reported above because reasonable levels of resolution are needed for accuracy to be regulated with a report option (e.g., see Higham, in press; Koriat & Goldsmith, 1996). However, there was no tendency for the testify rates to vary according to item type.

Experiment 2

The primary aim of Experiment 2 was to replicate and extend the central results of Experiment 1 and to further investigate the trace-strength and source-monitoring accounts of misinformation effects on confidence and accuracy. To achieve this end, there were a few critical differences between this experiment and the last one. First, instead of the correct-control alternatives being shown only once in the slides, they were repeated in Experiment 2 by also mentioning them in the post-event narratives. Thus, for control items, the correct control alternative was seen in the slides and mentioned in the narratives (two presentations), whereas the incorrect alternative was new (no presentations). For misinformation items, nothing changed between Experiments 1 and 2; the correct alternative was seen once in the slides whereas the incorrect alternative was mentioned once in the narrative.

The purpose of this change was to pit the predictions of the trace-strength model and the source-monitoring framework against each other. If responding is based on strength, this change should boost the impairment to resolution caused by misinformation (see Figure 2). For control items, the repetition of the correct-control alternatives would enhance the difference in mean strength between them and the new, incorrect-control alternatives compared to a situation in which the correct control detail is presented only once (e.g., Experiment 1 depicted in Figure 1). Thus, if responding is

based on trace strength, we should observe excellent accuracy and resolution for control items that should far exceed that for misinformation items, where strength levels are comparable between the correct and incorrect alternatives (each presented once).

On the other hand, the source-monitoring framework makes a different prediction. In Experiment 2, the correct-control alternative is presented in *different* and *competing* sources (once in the slides and once in the narrative; Figure 2). As a result, there may be conflicting source details recollected for correct-control alternatives, making source discrimination difficult. For example, the colour of the alternative might be recollected from its photographic presentation in the slides and its typeface recollected from its written presentation in the narrative. From the source-monitoring perspective, there is no reason for this source-ambiguous alternative to be correctly chosen with any regularity over the new, incorrect alternative, which provides no experimental-source information at all. Nor is there any reason to suppose it will be assigned high confidence if it is chosen. Therefore, in contrast to good accuracy and resolution for control items in this experiment as predicted by the trace-strength model, the source-monitoring framework predicts poorer accuracy and lower confidence for these items. Compared to the prediction of the trace-strength model, the situation is slightly better for the misinformation items for the source-monitoring model: each alternative is at least unique to one source and retrieved source details can be used to discriminate between the two alternatives that have comparable strength. Consequently, the net effect according to the source-monitoring framework is that by repeating correct control details in different sources in Experiment 2, misinformation may produce little impairment, and perhaps even facilitation, to resolution (see Figure 2).⁵

Some changes were implemented in Experiment 2 to lessen the possibility that participants would deliberately avoid reading the narrative so as not to be misled. First, we did not warn them about misinformation until immediately before the memory test, after presentation of the slide show was complete. This change also increased the ecological validity of Experiment 2. In real eyewitness scenarios, it is unlikely that information is encoded as correct or incorrect, which is likely to occur if warnings are given prior to the witnessed event, as in Experiment 1. Second, a single narrative containing all the misleading details was constructed and presented in a scrambled chronological order after all the slides had been viewed. Participants were required to indicate the correct order, which would have necessitated that they encoded all the (mis)information in the narrative.

Finally, as a further test of the trace-strength and source-monitoring accounts, we implemented a change to the structure of the test questions. Specifically, we added one correct and one incorrect coarse-grained alternative to the two fine-grained alternatives for each test question. This change meant that each test question had four possible answers: one fine-correct, one fine-incorrect, one coarse-correct, and one coarse-incorrect. The trace-strength and source-monitoring accounts differ in terms of the amount of conscious recollection that supports responding. On the one hand, a simple trace-strength account suggests that misinformation's effect on accuracy involves no recollection at all; the errors are caused by the comparable strengths of the misinformation and correct alternatives. Conversely, source-monitoring errors are recollection-based, involving retrieval of fine-grained, episodic details that can possibly become bound to the wrong item (e.g., Holmes, Waters, & Rajaram, 1998). Hence, if responding entails source monitoring, then misinformation's effect on accuracy should be manifested more in fine- than coarse-grained responses. On the other hand, if responding is based on strength, then no such prediction holds. If anything, given that strength by its very nature is vague and non-specific, participants may choose to respond predominantly with coarse-grained answers because fine-grained choices are not supported by recollection. If so, then according to the trace-strength account, the misinformation effect should be manifested more in coarse-grained than in fine-grained answers. To ensure that participants were able to tell apart experimenter-defined fine- versus coarse-grained answers, we manipulated the incentives to choose each answer type. As in Experiment 1, participants in Experiment 2 were also provided with a report option.

Method

Participants. A total of 23 female and 17 male students ($n = 40$) from the University of Southampton participated individually in the experiment in exchange for £5. Ages ranged from 18 to 46 years ($M = 22.27$ years, $SD = 5.15$ years). Twenty participants were randomly assigned to the low-incentive group and 20 to the high-incentive group.

Design and materials. The design was a 2 (item: control/misinformation) X 2 (incentive: high/low) mixed-factorial design with incentive manipulated between subjects. A murder crime scene was used again in this experiment, with 15 slides the same as those used in Experiment 1 plus three additional slides. However, unlike Experiment 1 in which there was alternating slide-narrative presentation, misinformation details were mentioned in a single narrative (approximately 500 words)

that was presented at the end of the slideshow. Also, unlike Experiment 1, critical items were chosen so that both fine- and coarse-grained versions could be constructed.

In total, 24 critical items were embedded in the narrative, of which 12 appeared in a fine-grained control form, and 12 in a fine-grained misinformation form. Assignment of the critical details to their control and misinformation forms was counterbalanced across participants. Approximately 1-3 critical details in the narrative pertained to each slide so that all aspects of the slideshow were represented. Throughout the narrative, no more than two critical details of the same form occurred in a row.

A four-alternative free-choice recognition memory test was constructed that consisted of 32 questions, 24 pertaining to critical items, and 8 to filler items. Twelve of the 24 critical-item questions pertained to control items and 12 to misinformation items. The four alternatives for all critical item questions consisted of: 1) correct-fine-grained alternative, 2) incorrect-fine-grained alternative, 3) coarse-grained version of "1", and d) coarse-grained version of "2". For the 12 questions pertaining to control items, the fine-correct alternative corresponded to a detail that appeared in the slide and was mentioned in the narrative, whereas the fine-incorrect alternative corresponded to new information. On the other hand, for the 12 questions pertaining to misinformation items, the fine-correct alternative corresponded to a detail that appeared only in the slide whereas the fine-incorrect alternative corresponded to a detail that had been read only in the narrative. For example, the DVD "Basic" appeared in one of the slides. For half the participants, the narrative mentioned that the DVD was "Basic," whereas for the other half, the narrative mentioned that the DVD was "Grease." For the test question about this slide, participants were asked to choose between 1) "Basic" (fine-correct), 2) "Grease" (fine-incorrect), 3) "an action film" (coarse-correct), and 4) "a musical film" (coarse-incorrect). Thus, there were two correct and two incorrect answers that varied in specificity. Assignment of these answers to the a, b, c, and d alternatives was counterbalanced across test questions. The grain size of the critical-item alternatives was manipulated in a number of different ways including the use of colours (e.g., coarse: *dark colour*, fine: *black*), numbers (coarse: *an odd number*, fine: *35*), and category membership (coarse: *a single storey building*, fine: *bungalow*). However, in all cases, the fine-grained alternative could be considered more specific than its coarse-grained counterpart.

Eight filler items were included in the test. For these four-alternative items, there were two incorrect fine-grained alternatives, an incorrect coarse-grained alternative, and a correct coarse-

grained one. The intention was that the inclusion of these items would vary the question structure enough so that strategic decision making would have little influence on performance. The data from these filler items were not analysed. A single, fixed order of the questions was used for all participants that queried details in the slides in the order in which they appeared. The filler items were randomly interspersed amongst the critical-item questions.

Participants had the opportunity for each question of choosing a “don’t know” option. If this option was chosen, participants were presented again with the same question with the same four alternatives and were then required to provide a response. It was not possible to advance to the next question until a response was provided. By gathering best guesses to “don’t know” responses, it was possible to calculate resolution in the same way as Experiment 1. Without these responses, it would not have been possible to know whether withheld answers were correct or not, which would have made it impossible to calculate the HR, FAR, and d' .

Procedure. After completing a consent form, participants were seated in front of a 24-inch Apple iMac computer and viewed the 18 slides of the murder crime scene on the computer screen. The slides were presented for 5 seconds each separated by a 1-second black screen. After the slideshow was complete, participants engaged in a filler task of solving anagrams (unscrambling sets of six or seven letter common words) for 5 minutes, and then they were presented with the narrative containing the misinformation on the computer screen. The narrative was a summary of the entire slideshow of approximately 500 words. To encourage encoding of the critical items, the narrative was broken into 9 segments that were presented in a scrambled order. Participants’ task was to put these segments into chronological order by placing numbers from 1 to 9 next to each segment. There was no time limit for this task. This method of introducing misinformation has been successfully used in previous research (e.g., Zaragoza & Lane, 1994).

After chronologically sorting the narrative, participants completed a second filler task (word recall) for approximately 15 minutes. This task was not related to the current experiment. After finishing the filler task, participants read the instructions for the memory test, which also appeared on the computer screen. The instructions emphasised that some of the details provided in the narrative were inaccurate so there should be no assumption that if a detail from the narrative was remembered, that it must also have been present on one of the slides. The instructions indicated, using an example, that most questions had two alternatives that were specific and two that were not, that most questions

had more than one correct answer, and that if none of the answers seemed correct, the “don’t know” option could be used.

For participants in the high-incentive group, the following instructions were given:

You are the only eyewitness of this crime and the police need information that is very likely to be correct. If you report errors, they could be very costly causing the investigation to fail, so it is critical that you be as accurate as you can. Providing very specific details is not so critical in case they are wrong. Thus, if you have low confidence in your answer, it is better to choose a vague answer or to click the “don’t know” button than to choose an incorrect specific answer. This will avoid costly mistakes. Only choose specific answers if you are highly confident about your answer.

Participants in the low-incentive group received these instructions instead:

There were several eyewitnesses to this crime and the police are in the initial stages of the investigation looking for leads. Consequently, it is not critical that you be accurate on every single question because the police can compare your evidence with other eyewitness reports. It is more important that your answer be specific and informative, so that the police have information to go on that they haven't already obtained from other eyewitnesses. Thus, if you have any confidence at all in your answer, it is better to choose a specific answer than to choose a vague one or to click the “don’t know” button. Only choose vague answers or the “don’t know” option if you really have no idea about which answer(s) is correct.

It was indicated to participants that the four alternatives would be shown to them again even if the “don’t know” option was chosen. Participants were told that any incentives that applied to the free-report stage did not apply, but that answers in the forced-report stage were to be used by the experimenters to find out what they would have said had they answered the question. Finally, regardless of whether the “don’t know” option had been used, participants were instructed to rate their confidence in the accuracy in their answer using a 0% to 100% scale. Participants then completed the 32-item (24 critical items, 8 filler items) recognition memory test on the computer using the mouse to select answers. Each question was presented individually on the computer monitor and the test was self-paced.

Results and Discussion

For all mixed ANOVAs reported for Experiment 2, only the incentive variable was manipulated

between subjects.

Response likelihoods. Overall, more than half of participants' responses were fine-grained ($M = .59$, $SE = .02$), -95% confidence limit = .55. As expected, more fine-grained answers were chosen in the low-incentive group ($M = .64$, $SE = .03$) than in the high-incentive group ($M = .53$, $SE = .03$), $F(1,38) = 6.80$, $MSE = 0.04$, $\eta^2 = .15$. This result indicates that participants were able to distinguish between the experimenter-defined fine- versus coarse-grained alternatives and responded to our incentive instructions. There was also no difference in the rate of fine-grain responses according to item type, $F < 1$ (control: $M = .58$, $SE = .02$; misinformation: $M = .60$, $SE = .03$).

About three-quarters of responses were reported, a rate which did not differ according to either item type, $F < 1$ (control: $M = .77$, $SE = .03$; misinformation: $M = .75$, $SE = .03$) or incentives, $F(1,38) = 1.26$, $MSE = .02$, $\eta^2 = .015$ (low incentive: $M = .79$, $SE = .03$; high incentive: $M = .74$, $SE = .03$).

Accuracy. Table 1 shows mean accuracy (collapsed across grain size) as a function of item type, incentive and report option. A 2 (incentive: high/low) X 2 (report option: reported/withheld) X 2 (item: control/misinformation) mixed ANOVA was conducted on accuracy.⁶ It revealed a main effect of item type, $F(1,31) = 8.05$, $MSE = 0.08$, $\eta^2 = .21$, and a main effect of report option, $F(1,31) = 44.79$, $MSE = 0.06$, $\eta^2 = .59$. Accuracy was higher for control ($M = .72$, $SE = .03$) than misinformation items ($M = .57$, $SE = .03$) and it was higher for reported ($M = .79$, $SE = .02$) than withheld items ($M = .50$, $SE = .03$). No other main effect or interaction was significant, largest $F(1,31) = 1.64$, $MSE = 0.04$, $\eta^2 = .05$.

Accuracy by grain size. To investigate whether the misinformation effect was manifested predominantly in coarse- versus fine-grained answers, we conducted a 2 (incentive: high/low) X 2 (grain size: coarse/fine) X 2 (item: control/misinformation) mixed ANOVA on accuracy. The data, shown in Table 5, were collapsed across the report option variable to avoid a predominance of empty cells. The ANOVA yielded a main effect of item, $F(1,37) = 21.23$, $MSE = 0.04$, $\eta^2 = .37$, and a main effect of grain size, $F(1,37) = 36.94$, $MSE = 0.03$, $\eta^2 = .50$.⁷ Accuracy was higher for control items ($M = .76$, $SE = .02$) than misinformation items ($M = .61$, $SE = .02$) and it was higher for fine-grained answers ($M = .77$, $SE = .02$) than coarse-grained answers ($M = .60$, $SE = .02$). The main effect of misinformation was qualified by a marginal interaction with grain size, $F(1,37) = 4.03$, $MSE = 0.05$,

$\eta^2 = .10$, $p = .05$. The marginal interaction occurred because the misinformation effect was larger for fine-grained responses than coarse-grained responses. Indeed, only the misinformation effect for fine-grained responses was significant, $F(1,37) = 26.96$, $MSE = 0.04$, $\eta^2 = .42$ and $F(1,37) = 2.19$, $MSE = 0.05$, $\eta^2 = .06$, for fine-grained and coarse-grained responses, respectively. The main effect of grain size was qualified by an interaction with incentive, $F(1,37) = 4.67$, $MSE = 0.03$, $\eta^2 = .11$. This interaction reflected the fact that the accuracy advantage of fine-grained responses over coarse-grained responses was larger in the high-incentive group than the low-incentive group (Table 5), although both effects were significant, $F(1,19) = 53.80$, $MSE = 0.02$, $\eta^2 = .74$ and $F(1,18) = 5.45$, $MSE = 0.04$, $\eta^2 = .23$, respectively. No other main effect or interaction was significant, largest $F < 1$.

Resolution. As in Experiment 1, the effect of misinformation on resolution was investigated with G (Table 2), mean confidence (Table 3), and type-2 d' (Table 4). For the analyses involving confidence ratings (G and mean confidence), item type (control/misinformation) and incentive (high/low) were included as variables in the analysis, but the data were collapsed across the report-option variable. On the other hand, the report-option variable was used to define the HR and FAR for the analysis on d' .

The 2 (incentive: high/low) X 2 (item: control/misinformation) mixed ANOVA on G revealed no significant effects, largest $F(1,38) = 2.37$, $MSE = .11$, $\eta^2 = .06$. However, G was well above chance (0) in all conditions, lowest -95% confidence limit = .38. An analogous ANOVA on d' also revealed no significant effects, largest $F(1,38) = 1.12$, $MSE = 0.75$, $\eta^2 = .03$, but as with G , all d' 's were well above chance, lowest -95% confidence limit = .45. Finally, a 2 (incentive: high/low) X 2 (item: control/misinformation) X 2 (accuracy: correct/incorrect) ANOVA on mean confidence revealed only a main effect of accuracy, $F(1,38) = 81.50$, $MSE = 253.2$, $\eta^2 = .68$. Participants were more confident in accurate responses ($M = 68.06$, $SE = 2.43$) than inaccurate ones ($M = 45.35$, $SE = 3.15$), indicating reasonably good resolution. No other main effect or interaction was significant, although the three-way interaction was marginal, $F(1,38) = 3.17$, $MSE = 115.9$, $\eta^2 = .08$, $p = .08$. In summary, there was no evidence that resolution was moderated by misinformation in any of these analyses, all F s < 1 .

Discussion

The results of Experiment 2 replicated and extended the central results of Experiment 1. A robust misinformation effect on accuracy was obtained, but misinformation had no discernible effect

on resolution. Again, this null effect was not attributable to low levels of resolution. Rather, both analyses involving confidence ratings (G and mean confidence) and the analysis involving the report option (d') indicated that resolution was reasonably good in all conditions.

Comparable resolution for control and misinformation items severely limits the generalisability of the conclusion based on previous research that misinformation has a devastating effect on people's ability to monitor the accuracy of their responses (e.g., Bonham & González-Vallejo, 2009; Tomes & Katz, 2000; Weingardt et al., 1994). If multiple critical items are used and participants are clearly informed that they have been misled, resolution remains quite high and intact despite the misinformation and its effect on accuracy. In contrast to previous research that has directly examined resolution in the misinformation paradigm, the current results suggest that participants were not solely relying on strength when making their recognition decisions and confidence ratings. If they had been, resolution for control items would have far exceeded that for misinformation items. Instead, the results suggest that participants were monitoring the source of the alternatives, which may have been difficult for repeated correct-control alternatives because they were presented in different, competing sources. Source confusion for correct-control alternatives would have limited the advantage that control items had over misinformation items, possibly rendering comparable resolution levels between the item types, which is exactly the pattern of results that we observed.

Added support that participants' responses were based to some degree on consciously recollected details that allowed them to engage in source discrimination is obtained by examining the graininess of their decisions. More than half of all responses were fine-grained, and the difference between control and misinformation accuracy was only significant with fine-grained answers.

As in Experiment 1, accuracy was higher for reported answers than for withheld answers, indicating that participants were successful at regulating accuracy. Such regulation would not have been possible without reasonably high levels of resolution (Koriat & Goldsmith, 1996). However, as in Experiment 1, there was no evidence that participants were *selective* in their use of the report option.

General Discussion

Robust misinformation effects on accuracy were found throughout both experiments. However, contrary to previous reports (Bonham & González-Vallejo, 2009; Tomes & Katz, 2000; Weingardt et al., 1994), these effects occurred without a concomitant effect on resolution. This dissociation between accuracy and resolution was observed with three different indices of resolution:

the Goodman-Kruskal gamma correlation (G), mean confidence, and type-2 discrimination (d'). The first two measures, commonly used in the metacognitive literature, are based on confidence ratings, whereas the third is derived from the rates of reporting correct (HR) versus incorrect (FAR) answers. All indices revealed reasonably good levels of resolution throughout all conditions in both experiments. Thus, the current results attest to the robustness of the misinformation effect, but indicate that the effect is not always accompanied by poor resolution performance.

If there are few misleading details and no indication that misleading information has been presented in the experiment, which is typical of previous studies that have directly examined resolution in the misinformation paradigm, participants have no reason to be vigilant about source and may have relied mostly on the strength of the alternatives. However, we suspect that the atypical design features of our experiments – particularly multiple critical items and greatly emphasised warnings about the presence of misleading information – may have encouraged more source monitoring than has been observed in previous research. These features likely made it evident that simply responding to the strength of the alternative(s) would not be enough for successful completion of the task. By attending to the recollected details of memories in our experiments, any effect of the high strength of misinformation alternatives could be appropriately discounted. Naturally, this would lead to better accuracy, but even if the misinformation alternative was mistakenly chosen, there may have been enough contradictory evidence regarding its source to lower confidence and improve resolution overall.

Further evidence of source-monitoring processes was found in Experiment 2 in which participants chose predominantly fine-grained rather than coarse-grained responses, and the difference between control- and misinformation-item accuracy was only significant for fine-grained answers. These results show that participants' memories were not vague and gist-based, which is what would be expected if strength was the primary basis of responding and source details were not being incorporated into their judgments.

Can the simple strength model that we have tested in these experiments be modified to account for our results? One possible change would be to base confidence on the *relative* strength of the two competing recognition alternatives instead of the *absolute* strength of the chosen alternative. Such a mechanism is implemented in Dougherty et al. 's (1999; see also Dougherty, 2001) Minerva-Decision Making (MDM) model of confidence. As the name implies, this model is based on

Hintzman's (1988) MINERVA model of memory. It is assumed that alternative answers access a distributed memory system and strength (in this case, *echo intensity*) is computed based on the similarity of each alternative to traces in memory. In a misinformation paradigm, this means that misinformation alternatives, by virtue of having been presented earlier in the experiment, will have higher strength than new-incorrect alternatives and comparable strength to correct alternatives. However, rather than confidence being based on the strength of the chosen alternative per se, it is computed by comparing the relative strengths of the two alternatives under consideration; large and small differential strength translates into high and low confidence, respectively. Hence, for misinformation items, confidence in the incorrect misinformation alternative will be low because of the comparable strength of the competing correct alternative, possibly leading to good resolution.

The problem with this amendment to the trace-strength model is that low differential strength between the alternatives means that confidence in *both* correct and incorrect responses to misinformation items should be low. Lowering confidence to correct responses would impair resolution of misinformation items compared to control items, just as if confidence was based on absolute strength. As noted, we found no evidence for impaired resolution due to misinformation or low confidence in correct responses to misinformation items. For example, as shown in Table 3, in both of our experiments, confidence in correct responses to misinformation items was high and considerably greater than confidence in incorrect responses. In our view, it is necessary to consider more than the strength of the alternatives to explain our results by taking into account participants' use of source information.

Because resolution was reasonably good and unaffected by misinformation, participants were in a position to strategically regulate their accuracy. Indeed, in both experiments, accuracy of reported answers was considerably higher than the accuracy of withheld answers (Table 1). However, as noted above, participants exhibited no tendency whatsoever to be *selective* in their use of the report option by, for example, reporting fewer answers to misinformation items than to control items. Equivalent reporting rates between the item types even occurred in Experiment 1 in which participants were informed that there was an equal number of each item type on the test, information which could presumably be used to assist in the selection process.

Conclusions

Three decades of memory research has examined the effects of misinformation on memory

accuracy. Some of this research has suggested that misinformation has a profound effect on confidence and that resolution is impaired. However, in our studies, which had clear warnings about misinformation and multiple critical items, misinformation impaired memory accuracy but had no detectable effect on resolution. These results contribute to a growing body of research on metacognitive processes underpinning the regulation of memory accuracy in forensic contexts.

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Footnotes

¹ Gamma is an ordinal measure of association (in this case between accuracy and confidence levels) advocated by Nelson (1984).

² Research demonstrating poor resolution for witnessed events seemingly accords nicely with analogous findings in other eyewitness memory domains. For example, many researchers investigating line-up identifications have found that post-identification confidence is a poor indicator of accuracy. In an oft-cited meta-analysis of confidence and eyewitness identifications, Bothwell, Deffenbacher, and Brigham (1987) reported an accuracy-confidence correlation of only $r = .25$. However, restriction of range may have been a problem underpinning this conclusion. For example, Lindsay, Read and Sharma (1998) found that the correlation was much higher if there is greater variation in the ability to identify the target.

³ One participant was dropped from these analyses of resolution because no incorrect responses were made to control items.

⁴ To compute d' , HR and/or FAR values of 1 were first replaced by $1 - 1/2n$, whereas values of 0 were replaced by $1/2n$, where n equals the number of observations on which the rate is based (i.e., the denominator of the rate). This correction, recommended by Macmillan and Creelman (2005), eliminates undefined values of d' .

⁵ We understand that repeating control details is an unusual control condition for studying the misinformation effect. However, our focus is on resolution and the effect that misinformation has on it. Experiment 1 already established that a misinformation effect on accuracy can be obtained with multiple items, emphasised warnings, and the more standard control condition. Consequently, there was no need to establish the effect on accuracy in Experiment 2 as well. Instead, our aim for Experiment 2 was to discriminate between the trace-strength and source-monitoring accounts of accuracy and confidence. Repeating the correct-control alternative provided this opportunity.

⁶ Seven participants were dropped from this analysis because they withheld no answers.

7 One participant was dropped from this analysis because no coarse-grained answers were chosen.

Table 1

Mean Accuracy (Standard Errors) in Experiments 1 and 2 as a Function of Item Type and Report Option

Report Option	Item Type	
	Control	Misinformation
Experiment 1		
Report (Testify)	.92 (.02)	.86 (.04)
Withhold	.61 (.05)	.49 (.05)
Experiment 2 – Low Incentive		
Report	.84 (.03)	.69 (.04)
Withhold	.54 (.10)	.50 (.08)
Experiment 2 – High Incentive		
Report	.82 (.03)	.72 (.04)
Withhold	.61 (.09)	.34 (.07)

Table 2

Mean Gamma Correlations (Standard Errors) in Experiments 1 and 2 as a Function of Item Type

Experiment and Group	Item Type	
	Control	Misinformation
Experiment 1	.55 (.07)	.55 (.08)
Experiment 2		
Low Incentive	.61 (.08)	.44 (.09)
High Incentive	.52 (.08)	.58 (.09)
Overall	.56 (.06)	.51 (.06)

Table 3

Mean Confidence (Standard Errors) in Experiments 1 and 2 as a Function of Item Type and Accuracy

Accuracy	Item Type	
	Control	Misinformation
Experiment 1		
Correct	77.83 (2.81)	81.39 (2.01)
Incorrect	57.90 (4.51)	59.63 (5.23)
Experiment 2 – Low Incentive		
Correct	68.05 (4.00)	66.36 (4.03)
Incorrect	39.92 (5.66)	46.62 (5.31)
Experiment 2 – High Incentive		
Correct	66.65 (2.83)	71.18 (2.88)
Incorrect	47.03 (2.87)	47.83 (3.45)

Table 4

Mean d' (Standard Errors) in Experiments 1 and 2 as a Function of Item Type

Experiment and Group	Item Type	
	Control	Misinformation
Experiment 1	1.00 (.16)	1.04 (.19)
Experiment 2		
Low Incentive	.77 (.15)	.87 (.22)
High Incentive	.90 (.18)	.59 (.18)
Overall	.83 (.12)	.73 (.14)

Note: d' was based on hit rate = $P(\text{report}|\text{correct})$ and false alarm rate = $P(\text{report}|\text{incorrect})$.

Table 5

Mean Accuracy (Standard Errors) in Experiment 2 for Fine- and Coarse-Grained Answers as a Function of Item Type and Incentives

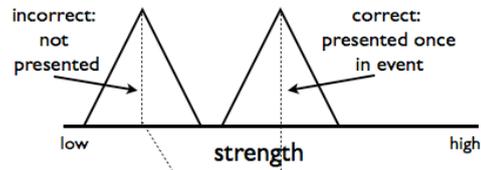
Response Type	Item Type	
	Control	Misinformation
	Low Incentive	
Coarse-Grained	.66 (.06)	.60 (.05)
Fine-Grained	.85 (.03)	.64 (.05)
	High Incentive	
Coarse-Grained	.61 (.05)	.52 (.05)
Fine-Grained	.91 (.03)	.69 (.05)

Figure Captions

Figure 1. Resolution predictions of the trace-strength model for the typical experimental design on the misinformation effect (e.g., Experiment 1). For control test items, the incorrect alternatives are new and hence the distribution has lower mean strength than the distribution of correct alternatives, which appeared in the event (slides). For misinformation test items, however, both alternatives were presented, the correct one in the event (slides) and the incorrect one in the post-event (narrative). The result is that, compared to the distribution of incorrect control alternatives, the mean strength of the distribution of incorrect alternatives for misinformation items is increased. The result is overlapping of the distributions of correct and incorrect alternatives for misinformation test items, impairing resolution relative to control test items.

Figure 2. Hypothetical distributions of participants' resolution scores (e.g., G , d') for control and misinformation test items in Experiment 2 as predicted by the trace-strength (TS) and source-monitoring (SM) models. According to the TS model, high differential trace strength between correct and incorrect alternatives results in high resolution (cf. Figure 1). This is the case for control items because the correct alternative has been presented twice whereas the incorrect one is new. However, resolution will be low for misinformation items because both the correct and incorrect alternatives were each presented once, resulting in similar trace strength. The SM model, on the other hand, maintains that resolution is more a function of source discrimination. The two competing sources (event and post-event) for the twice presented, correct-control alternative could lead to source confusion, thus impairing resolution for control items relative to the prediction of the TS model. For misinformation items, the SM model maintains that source details can be used to discriminate between alternatives of comparable strength, thus enhancing resolution relative to the prediction of the TS model. The net result is that the TS model predicts a large impairment to resolution caused by misinformation whereas the SM model predicts that misinformation will have little to no effect.

**Control - high resolution
(little distribution overlap)**



**Misinformation - low resolution
(substantial distribution overlap)**

