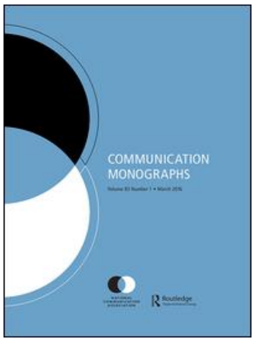

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How to unring the bell: A meta-analytic approach to correction of misinformation

Nathan Walter and Sheila T. Murphy

Annenberg School for Communication and Journalism, University of Southern California, Los Angeles, CA, USA

ABSTRACT
The study reports on a meta-analysis of attempts to correct misinformation ($k = 65$). Results indicate that corrective messages have a moderate influence on belief in misinformation ($r = .35$); however, it is more difficult to correct for misinformation in the context of politics ($r = .15$) and marketing ($r = .18$) than health ($r = .27$). Correction of real-world misinformation is more challenging ($r = .14$), as opposed to constructed misinformation ($r = .48$). Rebuttals ($r = .38$) are more effective than forewarnings ($r = .16$), and appeals to coherence ($r = .55$) outperform fact-checking ($r = .25$), and appeals to credibility ($r = .14$).

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Misinformation; correction; debiasing; meta-analysis; rebuttals

The notion that people are misinformed about health, politics, science, and the environment has almost reached the point of truism (Nyhan & Reifler, 2010). After all, it is far from being a coincidence that the Oxford Dictionary announced that "post-truth" is its 2016 word of the year (Oxford Dictionary, 2016). Their choice seems apt, given that a substantial portion of the population strongly believe that climate change is an elaborate Chinese hoax, the MMR vaccine causes autism, and that the 44th President of the United States is a Kenyan-born Muslim.

The pervasiveness of misinformation is nothing new. Indeed, during WWII, Allport and Lepkin's (1945) seminal study of wartime rumors found that one fourth of respondents adopted misinformation. According to more recent research, Oliver and Wood (2014) suggest that half of the American public consistently endorses at least one conspiracy theory. The prevalence of misinformation is particularly problematic if, as some believe, its debiasing tends to be ineffective or may even backfire, and strengthen the falsehood (Lewandowsky, Ecker, Seifert, Schwarz, & Cook, 2012). Though questions regarding the efficacy of correction attempts have been an integral part of misinformation research from its beginning, the empirical results are mixed. While some studies record substantial reductions in reported misinformation (e.g., Ecker, Lewandowsky, & Apai, 2011), other studies reveal nonsignificant results (e.g., Jolley & Douglas, 2014). Ideally, such contradictions could be disentangled by an overarching theoretical framework that makes specific predictions regarding the moderators that can either enhance or attenuate the impact of corrective messages. Nonetheless, while the continued influence of misinformation has

CONTACT Nathan Walter nathanw@usc.edu
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received considerable attention in recent years (e.g., Lewandowsky et al., 2012), studies that have pursued theoretical questions have often reached contradictory conclusions. For instance, in concurrence with predictions pertaining to message-sidedness research (Hovland, Lumsdaine, & Sheffield, 1949) and inoculation theory (McGuire, 1964), studies have found that the presentation of facts and myths in the same message can engage the receiver and lead to knowledge gain (Cameron et al., 2013). Conversely, supporting a metacognitive approach to correction of misinformation (Schwarz, 1998), results from other studies indicate that the combination of facts and myths can be detrimental to debiasing, as people misremember the message and find it difficult to differentiate between facts and myths (Skurnik, Yoon, & Schwarz, 2007).

Given the growth of misinformation and the conflicting theoretical views about the efficacy of correction attempts, questions surrounding the debiasing of misinformation are as pertinent as ever. To begin answering these questions, we conducted a meta-analysis that focuses on the overall effect of various attempts to correct misinformation and relevant moderators that were theoretically and empirically identified as important factors that may disentangle previous inconsistencies.

The current meta-analysis

Building on previous research, the current study defines misinformation as "cases in which people's beliefs about factual matters are not supported by clear evidence and expert opinion" (Nyhan & Reifler, 2010, p. 305). This broad definition serves the purpose of the current inquiry as it does not distinguish between uncertainty and a deliberate intent to mislead, nor between the various origins of misinformation, including vested interest groups, politicians, media, rumors, and conspiracies (Lewandowsky et al., 2012).

To the best of our knowledge, this report provides the broadest and most extensive formal meta-analysis that systematically compares attempts to correct misinformation across the major contexts in which this phenomenon has been studied, including science, health, politics, marketing, and crime. For instance, while Blank and Launay's (2014) meta-analysis has provided insights into the misinformation effect, it was largely restricted to studies of post-warning and ways to protect eyewitness memory against misinformation. More recently, Chan, Jones, Hall Jamieson, and Albarracín (2017) used a meta-analytic approach to investigate the factors underlying debiasing of misinformation. The results revealed substantial effects on all relevant outcomes; however, the analysis focused only on reports published from 1994 to 2015, including three relevant moderators (i.e., the generation of explanations in line with the misinformation, the generation of counterarguments to the misinformation, and the level of detail of the debunking message). Thus, pertinent questions surrounding the moderating role of audience characteristics (e.g., age and culture), message characteristics (e.g., topic), and design characteristics (e.g., immediate effects vs. delayed effects) remain unaddressed.

The current meta-analysis casts a much broader net of the misinformation literature to analyze theoretical and empirical moderators that may promote a clearer understanding of successful debiasing. Specifically, the current meta-analysis focuses on various strategies that were used in the literature to debias misinformation, including appeals to consensus, coherence, source credibility, fact-checking, and providing general warnings. The analysis focuses on experimental comparisons between a misinformation condition and a correction condition. This comparison isolates the unique contribution of the corrective message and directly estimates the efficacy of correction. With regard to research outcomes, we assess the influence of corrections on beliefs in misinformation because it is the most common measurement used to assess the efficacy of correction treatments. With this information in mind, the first research question is:

RQ1: What is the average effect of corrective messages on beliefs in misinformation?

Following Lewandowsky et al.'s (2012) call to examine the variables that enhance or attenuate the success of debiasing, our second goal was to investigate potential moderators. These factors are primarily based on theoretical propositions and empirical evidence of previous analyses, and they are broadly categorized as sample characteristics, message characteristics, and research design characteristics.

Sample characteristics

For convenience purposes, studies often use college students. However, the effect sizes derived from college students and nonstudents are often different in terms of directionality and magnitude (Peterson, 2001). Further, considering the fact that amounts of topical knowledge (e.g., science and politics) vary as a function of education, college students are perhaps less susceptible to misinformation. For instance, Guy, Kashima, Walker, and O'Neill (2014) identified education as an important factor that can counteract the impact of ideology on beliefs in climate change. Thus, college student samples are expected to yield stronger effects for correction than nonstudent samples. Conversely, Hamilton (2011) showed that educated republicans were less likely to view global warming as a threat, compared to their less educated counterparts. According to this view, preexisting beliefs and ideology seem to override correction attempts (Lewandowsky et al., 2012). Thus, it is hard to predict the interplay between sample type and adoption of misinformation.

As one might expect, the spread and nature of misinformation vary by country and culture (Smith, Appleton, & MacDonald, 2013). For example, evolution acceptance rates and climate change acceptance rates are found to be dramatically different across countries and regions (Sinatra, Kienhues, & Hofer, 2014). While there is little evidence concerning the role played by the region of study in moderating correction of misinformation, the few studies that directly compared samples from different countries reached interesting conclusions. For instance, a study that used representative samples of Australian and U.S. participants found that, among Australians, consensus information regarding global warming tended to neutralize the effects of worldview, whereas, among U.S. participants, worldview-related effects persisted (Cook & Lewandowsky, 2016).

Message characteristics

Although misinformation is pervasive in many social contexts including science, health, politics, and marketing, how debiasing is coded, delivered, and interpreted may be dissimilar across disciplines and topics. In other words, different topics can produce distinct results for correction attempts. Generally speaking, misconceptions regarding climate change (Cook & Lewandowsky, 2016), evolution, and healthcare reform (Berinsky,

2017) could be harder to correct, as people's religious and political identities are deeply implicated. In contrast, individuals should be less invested in issues such as misleading content in a social psychology course (Kowalski & Taylor, 2009). Surprisingly, there are very few studies that offer direct comparisons between different classes of messages (but see Nyhan & Reifler, 2010; Weeks, 2015). Further, studies that examine corrections can either target real-world misinformation (e.g., denial of climate change) or constructed misinformation (e.g., fictional plane crash). By design, exposure to real-world misinformation tends to pose more challenging tests for the power of debiasing, compared to constructed misinformation (Berinsky, 2017). As opposed to corrections of constructed misinformation, attempting to correct real-world misinformation introduces practical challenges, such as previous exposure, defensive processing, and potential floor/ceiling effects (Thorson, 2016).

According to Lewandowsky et al. (2012), recipients assess the truth of statements by attending to several key message features: (a) is the information compatible with what I believe? (b) Is the information internally coherent? (c) Does it come from a credible source? and (d) Do other people believe it? To some extent, these truth evaluations correspond with common strategies that have been used to correct for misinformation, including appeals to consensus (e.g., emphasizing the overall agreement among scientists regarding global warming), coherence (e.g., providing alternative explanations to misleading information about vaccine safety), source credibility (e.g., highlighting the fact that official agencies disagree with the assertion that vaccines can cause autism), fact-checking[1] (e.g., determining the veracity of statements regarding political policies), and general warnings (e.g., providing a cautionary statement regarding news consumed on social media).

Highlighting consensus is assumed to be effective because it encourages people to think of important social norms or use perceived agreement as a heuristic to guide their beliefs (van der Linden, Clarke, & Maibach, 2015). Alternatively, appeals to coherence rely on the notion that information presented without internal contradictions is easily processed and less likely to encourage message derogation (Johnson-Laird, 2012). Further, the literature on source credibility suggests that messages delivered by a credible source are deemed more trustworthy compared to messages linked to low-credibility sources (Pornpitakpan, 2004); hence, corrective messages that leverage on source credibility are more likely to be effective. In addition, fact-checking has come to play an important role in media coverage. According to this strategy, misinformation is corrected by systematically evaluating the accuracy of specific statements often relying on rating scales that label information as true or false (Amazeen, Thorson, Muddiman, & Graves, 2015). While the fact-checkers focus on specific details within a message, another approach has used general warnings to alert people that information could be misleading. These types of messages can either precede a false statement or immediately follow it; potentially increasing suspicion and encouraging more active processing of information (Ecker, Lewandowsky, & Tang, 2010).

Clearly, these strategies are far from being exclusive and they can often overlap (e.g., fact-check may contain a general warning or attack source credibility); yet, in some cases, corrective information highlights a particular message, enabling us to assess the relative efficacy of different approaches to debiasing (e.g., Ecker et al., 2010; Kowalski & Taylor, 2009; Smith et al., 2011).

Research design characteristics

Though variations in effects based on research design are to be expected (Hovland, 1959), the literature on correction of misinformation does not make direct predictions regarding the superiority of a particular research design. It stands to reason, however, that lab experiments will be associated with stronger correction effects, compared to online/field experiments. Given the higher control achieved in lab experiments, participants are, perhaps, paying more attention to corrective information. Further, the efficacy of corrective attempts can be examined immediately after exposure or with a delayed measurement (usually requiring participants to engage in a filler-task). Although it is likely that methods used to alleviate misinformation will be more effective when assessed immediately, there is little evidence to indicate that delayed measurements produce weaker effects.

The placement of the corrective message with respect to the statement of misinformation can, presumably, also affect debiasing. While some studies position the corrective message after the misinformation (i.e., rebuttal), other studies place corrections before exposing individuals to erroneous information (i.e., forewarning). It is likely that the placement of the correction relative to the misinformation, before or after, may have ramifications for its failure or success. Thus, this meta-analysis also seeks to understand:

> *RQ2:* Do sample characteristics (student vs. nonstudent), region of study, message topic, nature of information (constructed vs. real-world), study design (lab vs. online-field), debiasing technique, effect type (immediate vs. delayed), and correction placement (forewarning vs. rebuttal) moderate the effect of correction on belief in misinformation?

Method

Selection of studies

Literature search

Studies used in the meta-analysis were obtained in three ways. First, relevant electronic databases were systematically searched for empirical reports that focused on misinformation and correction (i.e., *Google Scholar, All Academic, JSTOR, Medline, ProQuest, PubMed, Communication and Mass Media Complete, Educational Resources Information Center*). The search located journal publications, conference papers, book chapters, doctoral dissertations, from a wide range of adjacent disciplines. The specific terms (and their derivations) that were used to perform the search included: misinformation, correction, debiasing, false belief, retraction, debunk. These were combined with: effect, persuasion, comprehension, climate, vaccine, health, GMO, and tobacco. Second, we examined the reference lists for each publication to find potential studies that were not located by the search terms. Finally, we contacted 12 leading scholars in the field of misinformation and asked them to identify omissions in our study corpus, as well as share their unpublished results.

Inclusion criteria

In order to be included in the meta-analysis, studies had to meet the following criteria. First, each study had to include correction of misinformation. Second, studies had to report on quantitative outcomes of exposure to correction attempts. Third, studies had to measure the effect of corrective information on beliefs. Fourth, studies had to employ an experimental design that included two conditions that differ only with

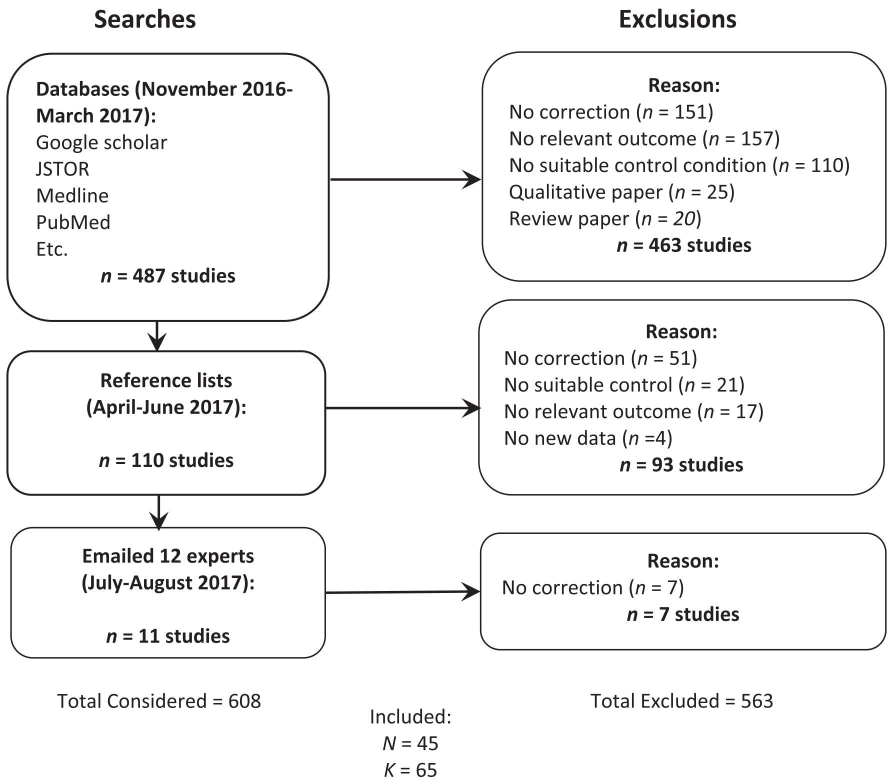

Figure 1. Search strategy flow chart.

respect to the inclusion of a corrective message. Finally, studies had to report on appropriate statistics (e.g., t values, means, standard deviations, counts, frequencies, zero-order correlations, or exact p values) for calculating an effect size. In cases where sufficient information was not available in the report ($n = 5$), corresponding authors were asked to provide additional data. After the screening process, 45 research reports that documented the results of 65 separate studies were included in the meta-analysis (~10% unpublished), with a total sample size of 23,604 (see Figure 1 for a flow chart that outlines the search strategy).

Coding of outcomes

A single effect size was calculated per sample. When studies reported on several relevant outcomes (e.g., belief in climate science and belief in scientific consensus regarding climate science), measures were averaged. Though most samples ($k = 59$) included only one relevant outcome, six samples included two outcomes. For the sake of consistency, effect sizes were transformed into a correlation estimate (r)

Coding of moderators

Studies were coded based on the type of population they focused upon, including college student samples ($k = 35$), studies of nonstudent samples ($k = 29$), and one study that included both students and nonstudent participants. With respect to the region of study, 45 samples were associated with North America, 11 samples were from Oceania, 7 samples were from Western Europe, and East Asia was represented by two samples.

The general context of the study was coded into six categories: (a) politics ($k = 16$); (b) crime ($k = 14$); (c) health ($k = 9$); (d) science ($k = 6$); (e) marketing ($k = 9$); and (f) other[2] ($k = 11$). The final sample included 27 studies that used real-world misinformation and 38 studies that used constructed misinformation. Attempts to correct misinformation were categorized either as: (a) fact-checking ($k = 21$); (b) appeals to credibility ($k = 17$); (c) coherence ($k = 19$); (d) general warnings ($k = 3$); (e) appeals to consensus ($k = 4$); or (f) a combination of several techniques ($k = 1$). Reports were coded for the design type that was utilized to gather the data: lab experiments ($k = 35$) and online/field experiments ($k = 30$). Studies were trichotomized into three groups, including cases where measures of beliefs immediately followed the corrective treatment ($k = 44$), cases where there was a brief filler-task between the corrective treatment and the measure of beliefs ($k = 9$), and cases where the time between the corrective treatment and the measure of beliefs was one day or greater ($k = 11$). The placement of the corrective attempt was coded as a rebuttal ($k = 56$) or forewarning ($k = 6$). Table 1 provides a complete outline of the coding of moderators by study.

Inter-coder reliability

Approximately 46% of our final sample ($k = 30$) was coded by two independent coders and assessed for inter-coder reliability. All moderating variables as well as effect estimates were used to evaluate the level of agreement with *Krippendorff's alpha*, resulting in agreement of .84 or above for all variables,[3] indicating a satisfactory reliability. Inclusion/exclusion decisions were made by the authors after the initial corpus of studies was created.

Analysis

The results of random-effects models are reported. Whereas fixed-effects assume that there is a common main effect that is "true" for all reports, random-effects make no such assumption, suggesting that effects are relevant beyond the specific populations from which they were drawn (Hedges & Vevea, 1998). Based on Borenstein, Hedges, Higgins, and Rothstein's (2005) approach to random-effects, the combined random-effect was computed by assigning more weight to the studies that carry more information, using the inverse of the variance rather than the sample size (the inverse variance is proportional to sample size but it provides a more nuanced measurement). Correlation coefficients (r) were calculated using the statistical package, Comprehensive Meta-Analysis (v.3; Borenstein et al., 2005).

To examine the possibility that variance in effect sizes could be accounted for by measurement error in the dependent variable (i.e., beliefs), we also tested the random-effects model with the procedure proposed by Hunter and Schmidt (2004). First, for studies that reported on the internal consistency of the belief measure, corrected effect sizes were computed with the formula offered by Hedges and Olkin (1985). Overall, only 32.30% of all studies reported on reliability with a mean reliability of .84 ($SD = .11$). Second, given the sporadic availability of reliability coefficients, instead of estimating

Table 1. Overview of studies included in meta-analysis.

Study	*N*	Topic	Message	Location	Strategy	Delay
Aikin et al. (2017)[a,b,c]	6454	Health	Misinformation concerning a fictitious drug	N. America	Fact-check	≥day
Aikin et al. (2015)[a,b,c]	517	Health	Misinformation concerning a fictitious drug	N. America	Fact-check	Immediate
Amazeen et al. (2016)[a,b]	677	Politics	Description of a Congressman	N. America	Fact-check	Filler-task
Armstrong et al. (1979)	100	Marketing	Misinformation concerning Listerine	N. America	Credibility	≥day
Berinsky (2012) – Study 1[a,b,e]	400	Politics	Rumors concerning the Affordable Care Act	N. America	Credibility	≥day
Berinsky (2012) – Study 3[a,b,e]	300	Politics	Rumors concerning the Affordable Care Act	N. America	Credibility	≥day
Berinsky (2015) – Study 1[a,b]	704	Politics	Rumors concerning the Affordable Care Act	N. America	Credibility	Immediate
Berinsky (2015) – Study 2[a,b]	556	Politics	Rumors concerning the Affordable Care Act	N. America	Credibility	≥day
Bernhardt et al. (1986)[a,b]	1668	Marketing	STP corrective advertising	N. America	Credibility	≥day
Biener et al. (2007)[a]	177	Health	Misinformation concerning tobacco	N. America	Fact-check	Immediate
Christiaansen and Ochalek (1983) – Study 1[c]	45	Crime	Shoplifting	N. America	Warning	≥day
Christiaansen and Ochalek (1983) – Study 2[c]	60	Crime	Wallet-snatching	N. America	Warning	≥day
Clark et al. (2013) – Study 4[a,e]	63	Science	Misinformation concerning climate change	N. America	Fact-check	≥day
Cook et al. (2017) – Study 1	600	Science	Misinformation concerning climate change	N. America	Consensus	Immediate
Cook et al. (2017) – Study 2[a,b,d]	196	Science	Misinformation concerning climate change	N. America	Consensus	Immediate
Darke et al. (2008) – Study 1[c]	219	Marketing	Misleading claim concerning a product	N. America	Fact-check	Filler-task
Darke et al. (2008) – Study 4[a,b,c]	221	Marketing	Misleading claim concerning a product	N. America	Fact-check	≥day
Darke et al. (2008) – Study 5[a,b,c]	90	Marketing	Misleading claim concerning a product	N. America	Fact-check	Filler-task
Davies (1997) – Study 1[c]	144	Other	Retraction of a study	W. Europe	Fact-check	Immediate
Dixon et al. (2015)[a,b]	124	Health	Misinformation concerning vaccines	N. America	Consensus	Immediate
Dyer and Kuehl (1978)[a]	72	Marketing	Misinformation concerning Listerine	N. America	Credibility	≥day
Ecker et al. (2011) – Study 1[c]	40	Crime	Misinformation concerning a plane crash	Oceania	Coherence	Immediate
Ecker et al. (2011) – Study 2[c]	64	Crime	Misinformation concerning a plane crash	Oceania	Coherence	Immediate
Ecker et al. (2011) – Study 3[c]	120	Crime	Misinformation concerning a plane crash	Oceania	Coherence	Filler-task
Ecker et al. (2010) – Study 1[c,d]	75	Crime	Misinformation concerning a minibus accident	Oceania	Coherence	Filler-task
Ecker et al. (2010) – Study 2[c,d]	67	Crime	Misinformation concerning a minibus accident	Oceania	Coherence	Filler-task
Ecker et al. (2015) – Study 1[c]	126	Crime	Misinformation concerning a car accident	Oceania	Coherence	Immediate
Ecker et al. (2015) – Study 2[c]	120	Crime	Misinformation concerning a car accident	Oceania	Coherence	Immediate
Ecker et al. (2014) – Study 1[c]	96	Crime	Misinformation concerning a robbery	Oceania	Credibility	Immediate
Ecker et al. (2014) – Study 2[c]	100	Crime	Misinformation concerning a robbery	Oceania	Credibility	Immediate
Ecker et al. (2011) – Study 1[c]	92	Crime	Misinformation concerning a warehouse fire	Oceania	Coherence	Immediate
Ecker et al. (2011) – Study 2[c]	138	Crime	Misinformation concerning a warehouse fire	Oceania	Coherence	Immediate
Garrett and Weeks (2013)[a,b]	574	Politics	Misinformation concerning health policy	N. America	Fact-check	Immediate
Garrett et al. (2013)[a,b]	520	Politics	Misinformation concerning 9/11	N. America	Fact-check	Immediate
Greitemeyer (2014)	103	Other	Retraction of a study	W. Europe	Fact-check	Immediate
Huang (2017) – Study 1[a,b]	504	Politics	Rumors on the internet	E. Asia	Credibility	Immediate

(*Continued*)

Table 1. Continued.

Study	*N*	Topic	Message	Location	Strategy	Delay
Huang (2017) – Study 2[a,b]	640	Politics	Rumors on the internet	E. Asia	Credibility	Immediate
Johnson and Seifert (1994) – Study 3a[c]	40	Crime	Misinformation concerning a warehouse fire	N. America	Coherence	Immediate
Kortenkamp and Basten (2015) – Study 1[b,c]	166	Science	Misinformation concerning air pollution	N. America	Combination	Immediate
Kortenkamp and Basten (2015) – Study 2[b,c]	166	Science	Misinformation concerning water pollution	N. America	Consensus	Immediate
Kortenkamp and Basten (2015) – Study 3[b,c]	166	Health	Misinformation concerning mosquito-borne illness	N. America	Fact-check	Immediate
Mazis and Adkinson (1976)	83	Marketing	Misinformation concerning Listerine	N. America	Credibility	Filler-task
Misra (1992) – Study 2[c]	50	Other	Debriefing	N. America	Fact-check	Immediate
Misra (1992) – Study 3[c]	46	Other	Debriefing	N. America	Fact-check	Immediate
Morgan and Stolman (2002)[d]	230	Marketing	Disclaimers in advertising	N. America	Warning	Filler-task
Nyhan and Reifler (2010) – Study 1[b]	150	Politics	Misinformation concerning Iraqi WMDs	N. America	Coherence	Immediate
Nyhan and Reifler (2010) – Study 2[b]	195	Politics	Misinformation concerning Iraqi WMDs, tax cuts, and policy about stem cell research	N. America	Coherence	Immediate
Nyhan and Reifler (2015a)[a,b,c]	500	Politics	Political misconduct	N. America	Coherence	Immediate
Nyhan et al. (2016) – Study 1[a,b]	1682	Crime	Misinformation about the crash of TWA Flight 800	N. America	Credibility	Immediate
Peter and Koch (2016)[a,b,c,d]	335	Health	Detection of bowl cancer	W. Europe	Fact-check	≥day
Pingree et al. (2014)[b,c]	436	Politics	Misinformation concerning a political event	N. America	Credibility	Immediate
Rapp and Kendeou (2007) – Study 1[c]	64	Other	Misleading information concerning stories	N. America	Coherence	Immediate
Rich and Zaragoza (2016) – Study 1[c]	215	Crime	Misleading information concerning a jewelry theft	N. America	Credibility	Immediate
Rich and Zaragoza (2016) – Study 2[c]	228	Crime	Misleading information concerning a jewelry theft	N. America	Coherence	Immediate
Rich (2013) – Study 1[c,e]	327	Crime	Misleading information concerning a jewelry theft	N. America	Coherence	Immediate
Sawyer and Semenik (1978)[a,e]	142	Marketing	Misinformation concerning Listerine	N. America	Credibility	Filler-task
Tangari et al. (2010)[a]	390	Health	Misinformation concerning tobacco	N. America	Fact-check	Immediate
Thorson (2016) – Study 1[a,b,c]	101	Politics	Misleading information concerning political misconduct	N. America	Fact-check	Immediate
Thorson (2016) – Study 2[a,b,c]	345	Politics	Misleading information concerning political misconduct	N. America	Fact-check	Immediate
Weeks (2015)[a,b]	512	Politics	Misinformation concerning health policy and illegal immigration	N. America	Fact-check	Immediate
Wilkes (1999) – Study 1[c]	36	Crime	Misinformation concerning a warehouse fire	W. Europe	Coherence	Immediate
Wilkes (1999) – Study 2[c]	48	Crime	Misinformation concerning a warehouse fire	W. Europe	Coherence	Immediate
Wilkes and Leatherbarrow (1988) – Study 1[c]	90	Crime	Misinformation concerning a warehouse fire	W. Europe	Coherence	Immediate
Wood (2014) – Study 1[e]	227	Science	Misinformation concerning climate change	N. America	Credibility	Immediate
Wright (1993)[c]	68	Other	Misleading information about breakfast	W. Europe	Fact-check	Immediate

[a]Nonstudent sample.
[b]Online/field experiment.
[c]Constructed misinformation.
[d]Forewarning.
[e]Unpublished.

reliability based on the number of items included in the measure (Spearman–Brown formula), we followed the Hunter–Schmidt method and estimated the distribution of reliabilities in our sample (Hunter & Schmidt, 2004, p. 308). Third, corrected effect sizes were used to analyze the data with the Hunter–Schmidt meta-analysis program (Schmidt & Le, 2014).

In order to examine the role played by moderating variables, we performed the Hedges' Q test. Analogous to the omnibus test that estimates the difference between groups in ANOVAs, the Q test provides a general assessment of variance and specific contrasts need to be used to explore the differences among group means (Hedges & Pigott, 2001). Hence, the confidence intervals associated with a group means merit special attention and significant moderation does not indicate that there are significant differences between all group means.

Results

RQ1: Main effect of correction on belief in misinformation

Across 65 individual studies (with a mean sample size of 336.14 and a median of 166) the mean effect size for reduction in post-correction misinformation was moderate, positive, and significant ($r = .35$, 95% CI [.26, .44], $p = .0005$), with significant heterogeneity in effect sizes, $Q(64) = 3614.78$, $I^2 = 98.23\%$, $p = .0005$ (see Table 2). To address the concern that the significant heterogeneity in effect sizes could be attributed to measurement error in the dependent variable (i.e., beliefs), we repeated the meta-analysis using the Hunter–Schmidt approach to artifact correction. According to the barebones meta-analysis (before correcting for measurement error), the estimated mean effect was $r = .22$ (95% CI [.02, .40], $p = .03$). After correcting for measurement error, the effect size increased to $r = .24$ (95% CI [.05, .40], $p = .01$) and the results indicated that 59.84% of the variance in observed effect sizes could be explained by the corrected artifact, which suggests that more than 40% of the variance could be potentially linked to various moderators. Notably, the average effect recorded with the Hunter–Schmidt (HS) estimator was weaker than the effect recorded with the random-effects model in CMA (Borenstein et al., 2005). These discrepancies are to be expected as the HS estimator has been shown to be negatively biased (Viechtbauer, 2005). With this in mind, we proceeded to examine the potential role played by moderators.

RQ2: The effects of moderators on correction of misinformation

In line with the finding that five or more studies are needed to reasonably achieve power from random-effects models that are greater than the statistical power of the individual studies (Jackson & Turner, 2017), moderation analyses were conducted for categories that included at least five cases. The results revealed that college student samples ($r = .50$, 95% CI [.30, .66], $p = .0005$, $k = 35$) and nonstudents samples ($r = .14$, 95% CI [.10, .18], $p = .005$, $k = 29$) did differ significantly in effect sizes ($Q(1) = 10.65$, $p = .001$). In other words, corrective messages appear to be more effective in reducing beliefs in misinformation for student samples compared with nonstudent samples. Yet, further probing the data revealed that studies that used student samples were also more likely to be

Table 2. List of studies included in meta-analysis.

Study	Year	Study number	*N*	*r* [CI]	α	Corrected *r* [CI]
Aikin et al.	2017	1	6454	.05 [.02, .07]	.65	.07 [.04, .10]
Aikin et al.	2015	1	517	.15 [.07, .24]	.50	.24 [.16, .31]
Amazeen et al.	2016	1	677	.16 [.09, .23]	.94	.21 [.12, .30]
Armstrong et al.	1979	1	100	.32 [.14, .48]	–	–
Berinsky	2012	1	400	−.04 [−.18, .10]	–	–
Berinsky	2012	3	300	.11 [−.03, .26]	–	–
Berinsky	2015	1	704	.11 [.04, .17]	–	–
Berinsky	2015	2	556	.09 [−.01, .18]	–	–
Bernhardt et al.	1986	1	1668	−.03 [.08, .03]	–	–
Biener et al.	2007	1	177	.15 [.01, .29]	–	–
Christiaansen and Ochalek	1983	1	45	.47 [.23, .65]	–	–
Christiaansen and Ochalek	1983	2	60	.41 [.19, .59]	–	–
Clark et al.	2013	4	63	.22 [−.02, .43]	–	–
Cook et al.	2017	1	600	.19 [.11, .26]	–	–
Cook et al.	2017	2	196	.06 [−.08, .20]	–	–
Darke et al.	2008	1	219	.22 [.09, .34]	.76	.27 [.13, .39]
Darke et al.	2008	4	221	.13 [.01, .26]	.93	.14 [.01, .27]
Darke et al.	2008	5	90	.27 [.07, .44]	.95	.32 [.10, .51]
Davies	1997	1	144	.27 [.12, .41]	–	–
Dixon et al.	2015	1	124	.25 [.14, .36]	.80	.28 [.18, .40]
Dyer and Kuehl	1978	1	72	.30 [.08, .49]	–	–
Ecker et al.	2011	1	40	.97 [.94, .99]	–	–
Ecker et al.	2011	2	64	.95 [.93, .97]	–	–
Ecker et al.	2011	3	120	.99 [.98, .99]	–	–
Ecker et al.	2010	1	75	.29 [.08, .48]	–	–
Ecker et al.	2010	2	67	.39 [.18, .57]	–	–
Ecker et al.	2015	1	126	.61 [.50, .70]	–	–
Ecker et al.	2015	2	120	.82 [.76, .86]	–	–
Ecker et al.	2014	1	96	.26 [.07, .43]	.91	.27 [.08, .44]
Ecker et al.	2014	2	100	.25 [.07, .42]	.91	.27 [.08, .43]
Ecker et al.	2011	1	92	.27 [.07, .44]	–	–
Ecker et al.	2011	2	138	.22 [.06, .37]	–	–
Garrett and Weeks	2013	1	574	.16 [.08, .24]	.88	.18 [.10, .26]
Garrett et al.	2013	1	520	.39 [.29, .48]	–	–
Greitemeyer	2014	1	103	.23 [.04, .40]	.81	.25 [.07, .42]
Huang	2017	1	504	.09 [.01, .17]	–	–
Huang	2017	2	640	.08 [.01, .15]	–	–
Johnson and Seifert	1994	3a	40	.06 [−.25, .35]	–	–
Kortenkamp and Basten	2015	1	166	.59 [.49, .67]	.80	.63 [.54, .70]
Kortenkamp and Basten	2015	2	166	.92 [.90, .94]	.80	.93 [.92, .95]
Kortenkamp and Basten	2015	3	166	.37 [.24, .49]	.80	.41 [.28, .52]
Mazis and Adkinson	1976	1	83	.37 [.18, .54]	–	–
Misra	1992	2	50	.82 [.73, .88]	.95	.84 [.76, .89]
Misra	1992	3	46	.35 [.08, .57]	.90	.38 [.11, .60]
Morgan and Stolman	2002	1	230	.06 [−.07, .18]	–	–
Nyhan and Reifler	2010	1	150	−.11 [−.26, .06]	–	–
Nyhan and Reifler	2010	2	195	−.02 [−16, .12]	–	–
Nyhan and Reifler	2015	1	500	.12 [.03, .20]	–	–
Nyhan et al.	2016	1	1682	.11 [.06, .16]	–	–
Peter and Koch	2016	1	335	.12 [.01, .22]	.93	.12 [.01, .22]
Pingree et al.	2014	1	436	.29 [.20, .37]	–	–
Rapp and Kendeou	2007	1	64	.02 [−.22, .26]	–	–
Rich and Zaragoza	2016	1	215	.26 [.13, .38]	–	–
Rich and Zaragoza	2016	2	228	.43 [.33, .53]	–	–
Rich	2013	1	327	.14 [.03, .24]	–	–
Sawyer and Semenik	1978	1	142	.14 [−.03, .29]	–	–
Tangari et al.	2010	1	390	.10 [.01, .20]	.88	.12 [.01, .23]
Thorson	2016	1	101	.22 [.03, .40]	.92	.23 [.04, .40]
Thorson	2016	2	345	.18 [.07, .28]	.89	.19 [.09, .30]
Weeks	2015	1	512	.38 [.31, .45]	–	–
Wilkes	1999	1	36	.12 [−.04, .28]	–	–

(*Continued*)

Table 2. Continued.

Study	Year	Study number	N	r [CI]	α	Corrected r [CI]
Wilkes	1999	2	48	.17 [.02, .30]	–	–
Wilkes and Leatherbarrow	1988	1	90	.21 [.01, .39]	–	–
Wood	2014	1	227	.07 [−.06, .20]	–	–
Wright	1993	1	68	.35 [.09, .57]	–	–

Notes: Values in the r column are the standard difference in means between the correction condition and a no correction condition transformed to Pearson's r. Values in the α column are Cronbach's alpha coefficients for the measure of beliefs. Values in the corrected r column are the effects of correction on belief in misinformation corrected for measurement error with the formula offered by Hedges and Olkin (1985).

conducted in a lab setting; χ^2 (1, $N = 64$) = 24.74, $p = .0005$, and focus on constructed misinformation χ^2 (1, $N = 64$) = 19.86, $p = .0005$. To examine whether the recorded difference between student and nonstudent samples will persist after controlling for type of design (lab/field-online) and nature of misinformation (real-world/constructed), we conducted a meta-regression. Interestingly, after controlling for type of design and nature of misinformation, sample type was not a significant predictor ($b = .23$, SE = .19, $p = .23$) and the only significant moderator was nature of misinformation ($b = .27$, SE = .15, $p = .04$).[4]

With regard to region of study, the analysis recorded a significant moderation ($Q(2)$ = 8.47, $p = .014$); effects appeared stronger in samples from Oceania ($r = .77$, 95% CI [.37, .93], $p = .001$, $k = 11$), followed by samples from North America ($r = .24$, 95% CI [.17, .31], $p = .0005$, $k = 45$), and Western Europe ($r = .18$, 95% CI [.12, .24], $p = .0005$, $k = 7$). The general topic associated with the correction had a significant impact on beliefs (Q (4) = 10.15, $p = .047$), such that topics related to crime tended to yield stronger effects ($r = .64$,[5] 95% CI [.23, .86], $p = .005$, $k = 14$), followed by health ($r = .27$, 95% CI [.15, .39], $p = .0005$, $k = 9$), marketing ($r = .18$, 95% CI [.08, .29], $p = .001$, $k = 9$), and politics ($r = .15$, 95% CI [.08, .21], $p = .0005$, $k = 16$). Interestingly, the analysis resulted in a non-significant effect of correction on science-related beliefs ($r = .44$, 95% CI [−.04, .75], $p = .07$, $k = 6$). As expected, beliefs in constructed misinformation were easier to debunk ($r = .48$, 95% CI [.33, .61], $p = .0005$, $k = 38$), whereas beliefs in real-world misinformation tended to be more resilient to change ($r = .14$, 95% CI [.10, .19], $p = .0005$, $k = 27$). These differences were significance at the 95% level ($Q(1) = 15.24$, $p = .0005$).

The choice of a debiasing technique played a significant role in determining effects on beliefs in misinformation ($Q(2) = 10.40$, $p = .006$), as appeals to coherence ($r = .55$, 95% CI [.23, .77], $p = .002$, $k = 19$) produced stronger effects compared with fact-checking ($r = .25$, 95% CI [.18, .32], $p = .0005$, $k = 21$), and appeals to credibility ($r = .14$, 95% CI [.09, .20], $p = .0005$, $k = 17$). The analysis did document a significant difference ($Q(2) = 15.21$, $p = .0005$), between research designs that measured immediate effects on beliefs ($r = .37$, 95% CI [.27, .47], $p = .0005$, $k = 44$), research designs that included a brief filler-task between the correction and the measure of beliefs ($r = .48$, 95% CI [−.09, .82], $p = .095$, $k = 9$), and studies that delayed the outcome measure for at least one day ($r = .13$, 95% CI [.06, .19], $p = .0005$, $k = 11$). Further, as predicted, there was a significant difference ($Q(1) = 4.67$, $p = .031$), between studies that utilized lab experiments to measure effects on beliefs ($r = .46$, 95% CI [.26, .63], $p = .0005$, $k = 35$) and studies that employed online/field experimental designs ($r = .22$, 95% CI [.14, .30], $p = .0005$, $k = 30$). As previously mentioned, however, this effect disappears when controlling for sample type and nature of misinformation. Finally, the analysis indicated that rebuttals ($r = .38$, 95%

Table 3. The effects of correction on beliefs in misinformation by moderator.

Variable	*r*	*K*	*n*	*Q*	*p*	95% CI
Main effect	.35	65	23604			[.26, .44]
Sample				10.65	.001	
Students	.50	35	4285			[.30, .66]
Nonstudents	.14	29	19091			[.10, .18]
Topic				10.15	.047	
Crime	.64	14	792			[.23, .86]
Health	.27	9	2082			[.15, .39]
Marketing	.18	9	1217			[.08, .29]
Politics	.15	16	2016			[.08, .21]
Science	.44	6	502			[−.04, .75]
Region of study				8.47	.014	
North America	.24	45	6697			[.17, .31]
Oceania	.77	11	454			[.37, .93]
Western Europe	.18	7	360			[.12, .24]
Nature of information				15.24	.0005	
Constructed	.48	38	3912			[.33, .61]
Real-world	.14	27	3885			[.10, .19]
Design				4.67	.031	
Lab	.46	35	4077			[.26, .63]
Online/field	.22	30	19527			[.14, .30]
Debiasing technique				10.40	.006	
Coherence	.55	19	2520			[.23, .77]
Credibility	.14	17	7725			[.09, .20]
Fact-checking	.25	21	11772			[.18, .32]
Delay				15.21	.0005	
≥day	.13	11	10011			[.06, .19]
Filler-task	.48	9	1536			[−.09, .82]
Immediate	.37	44	11827			[.27, .47]
Placement				10.93	.001	
Forewarning	.16	6	1503			[.08, .24]
Rebuttal	.38	56	21471			[.28, .47]
Published				22.46	.0005	
Yes	.38	59	22345			[.28, .47]
No	.10	6	1259			[.03, .16]

CI [.28, .47], $p = .0005$, $k = 56$) were significantly more effective than forewarnings ($r = .16$, 95% CI [.08, .24], $p = .0005$, $k = 6$; [$Q(1) = 10.93$, $p = .001$]). Table 3 summarizes the effects of correction on beliefs in misinformation by sample, message, and design characteristics.

Publication bias

One of the most common statistical tests to detect and correct for a publication bias was introduced by Duval and Tweedie (2000a, 2000b). According to the Trim and Fill procedure; (a) smaller studies causing asymmetry are removed; (b) "true" averaged effect is estimated; and (c) the center of the plot is filled with the omitted studies. Based on the trim and fill assessment, there was a clear indication that a publication bias exists in the sample of observed studies. In particular, the test suggested that the adjusted main effect for correction of belief in misinformation is stronger than the one observed in the data ($r = .47$, 95% CI [.39, .55]). These results, however, should be interpreted with caution, as other evidence point to an opposite conclusion. Namely, when directly comparing published studies ($k = 59$) with unpublished studies ($k = 6$) within our sample, the analysis records a significant difference ($Q(1) = 22.46$, $p = .0005$), indicating that unpublished studies tended to retrieve weaker effects ($r = .10$, 95% CI [.03, .16]), compared to published studies ($r = .38$, 95% CI [.28, .47]).

Discussion

Research has widely explored the interplay between misinformation and debiasing, proposing a wide variety of contextual variables that can either facilitate or attenuate correction attempts. While substantially advancing our understanding, individual studies are also limited to particular topics, samples, outcomes, and designs. Employing a meta-analytic approach, the current study attempted to provide more general principles and conclusions regarding the body of knowledge associated with correction of misinformation.

The results show that corrective attempts can reduce misinformation across diverse domains, audiences, and designs. Most notably, corrections have a moderate-level effect on misinformation-related beliefs that persist even after controlling for measurement error. The analysis also revealed that debiasing attempts appear to be more successful in informing the audience about health compared to politics.[6] It seems that people are more resistant to change when it comes to their political identity. A potential explanation for the resistance associated with political misinformation pertains to education. While higher levels of education are usually a positive predictor for acceptance of health and scientific authority, when it comes to politics, correction attempts seem to be less effective, particularly among more educated political partisans (Nyhan, Reifler, & Ubel, 2013). As expected, the results indicated that constructed misinformation is easier to debunk compared to real-world misinformation. Either as a consequence of low-involvement or lack of previous exposure, people seem to be more open-minded when considering corrections of constructed misinformation. Alternatively, it can be argued that individuals are motivated to reject correction of real-world misinformation, as it can pose a threat to important aspects of their social identity. When considering the limited ecological validity of constructed misinformation, the potency of corrective messages in real-world contexts appears to be weak.

With regard to the specific debiasing techniques, appeals to coherence are more successful in reducing the influence of misinformation, compared to fact-checking and source credibility.[7] Indeed, corrective messages that integrate retractions with alternative explanations (i.e., coherence) emerge as an effective strategy to debunk falsehoods. When debiasing strategies rely solely on retractions (e.g., fact-checking), they run the risk of painting an incoherent image of the events. According to this logic, if someone believes that President Obama was born in Kenya, it might not be enough to simply present them with the facts. In addition, a successful correction would also include a coherent explanation for how and why the false rumor started. Once people are exposed to a coherent message that can explain the chain of events, they will be more likely to substitute the false information with the retraction. Additionally, the relative weak effects associated with corrective messages that appeal to source credibility are alarming but not surprising. In fact, this result fits well with the increase in political polarization and the growing erosion in public trust toward official sources. As argued by Lewandowsky et al. (2012), source's credibility is a function of belief: "If you believe a statement, you judge its source to be more credible" (p. 119). Hence, there is a need to further explore the interplay between beliefs and credibility judgments, focusing on potential methods that will encourage people to reconsider this epistemic circularity.

Timing appears to influence the relative success of corrective messages. In particular, when beliefs are assessed immediately after exposure to the corrective message, average effects are slightly weaker compared with studies that allow some time to pass before measuring the relevant outcomes. Yet, the weakest effects are recorded when the time between a correction treatment and the measurement of the outcome exceeds a day. While it is somewhat tempting to posit a curvilinear effect of time delay, it is important to emphasize that the average effect for studies with a short delay (i.e., brief filler-task) was strong but nonsignificant. Hence, at this point, we can only suggest that immediate measurement of the outcome tends to produce stronger effects than a relatively long delay (≥day).

Finally, there are some weak evidence that audience characteristics play a role in changing beliefs. Specifically, corrections seem to work better for student samples compared to nonstudent samples. A potential explanation for this finding is concerned with chronological age. Namely, differences pertaining to chronological age are often used to explain learning ability, preference for sources of information, as well as susceptibility to social influence (Phillips & Sternthal, 1977). In part, these differences are attributed to an age-related decline in cognitive ability and the dynamic nature of need for cognition (Spotts, 1994). However, as the meta-regression illustrated, these effects are likely to stem from design-related decisions (studies that expose participates to constructed misinformation are more likely to use student samples) rather than some inherent differences between student and nonstudent samples.

The current study is associated with several important limitations. With respect to the selection criteria, the analysis did not differentiate between studies from the continued influence paradigm (for a summary see Lewandowsky et al., 2012) and studies associated with post-event misinformation (for a summary see Loftus, 2005). While this decision was in line with the comprehensive analysis, the differences between the two types of research procedures should not be overlooked. These research paradigms address the issue of misinformation from different angles, with the post-event misinformation literature speaking to the ability of erroneous information to distort the accounts of events and the continued influence literature focusing on the power of retractions to eliminate the influence of misinformation. Given these differences, it stands to reason that the approaches do not rely on comparable theoretical mechanisms. Nonetheless, acknowledging the fact that the central purpose of the current study was to explore whether the adverse influence of misinformation can be undone, both strands of research provide equally relevant insights on correction of misinformation.

Moreover, the current study is Western-centric, focusing on correction of misinformation mainly in the U.S. context, while neglecting the fact that misinformation is a truly global and culturally bound phenomenon. In fact, the few empirical inquiries that chose to highlight the intercultural facets of misinformation support the possibility that corrective messages may produce different outcomes in different societies (e.g., Cook & Lewandowsky, 2016). Thus, future studies comparing correction attempts across cultures would further extend our understanding of the interplay between misinformation and its correction. Finally, while the decision to focus on experimental designs helped us to assess the causal influence of corrective messages on misinformation, it also limited the scope of studies in our corpus. Given the centrality of various social media platforms to the spread of misinformation, it would be interesting to examine whether the corrective message can reduce the misleading information in naturalistic settings.

Practical implications

This meta-analysis furthers our understanding of correction attempts and how they fare across various situations. Overall, the results offer an optimistic perspective on debiasing of misinformation. While it is true that corrections can prove to be ineffective, or even counterproductive, most often, they work. On a practical level, while fact-checking can be an effective tool for addressing falsehoods, ideally, corrective messages should include a retraction along with an alternative explanation for the misleading information (i.e., coherence). If the information is retracted without providing an alternative explanation, people's understanding of a topic may not feel coherent, leading them to deny the new information and reinstate the beliefs that existed before the retraction (Seifert, 2002). To effectively debunk misleading claims, messages should provide a coherent explanation that describes what really happened and why did it happen.

While the adverse influence of exposure to misinformation cannot be completely undone, it is much more effective to rebuttal the erroneous claims than to inform people that they are about to be exposed to misleading information. Though, in theory, forewarnings are supposed to inoculate against misleading arguments, they have only a limited capacity to minimize adoption of misinformation. To this end, post-warnings, or rebuttals, are generally more successful in reducing people's belief in a specific content of misinformation.

In conclusion, the goal of this project was to summarize the main findings from nearly a century of research that attempted to correct misinformation. It is our hope that this study will spark additional research to match the prevalence and complexity of real-world misinformation.

Notes

1. While appeals to coherence typically also include fact-checking, the coherence strategy requires an alternative explanation for the outcome (Rich & Zaragoza, 2016).
2. A category that included a depiction of general events that were not directly associated with the other topics.
3. Sample $\alpha = .99$; region of study $\alpha = .93$; topic $\alpha = .84$; nature of misinformation $\alpha = .88$; debiasing technique $\alpha = .85$; study design $\alpha = .94$; delay $\alpha = .88$; placement of correction $\alpha = .87$.
4. $Q(3) = 12.03$, $p = .007$, $R^2 = .16$.
5. This result should be interpreted with caution because all the studies within the crime category were associated with constructed misinformation (as opposed to real-world misinformation). In fact, based on a post-hoc meta-regression that controlled for "nature of misinformation" (constructed/real-world), studies that focused on crime did not significantly predict effects sizes for correction of misinformation ($b = .26$, $SE = .16$, $p = .15$).
6. Direct comparison between health and politics ($Q(1) = 3.75$, $p = .04$).
7. Direct comparison between coherence and fact-checking ($Q(1) = 3.84$, $p = .04$) and direct comparison between coherence and appeals to source credibility ($Q(1) = 5.56$, $p = .02$).
8. All references for the studies included in the meta-analysis can be found in appendix A, which is provided as an online supplemental file.

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Notes on contributors

Nathan Walter received his Ph.D. from the Annenberg School for Communication and Journalism. He will be an Assistant Professor in the fall of 2018 at Northwestern University.

Sheila T. Murphy is a Professor in Annenberg School for Communication and Journalism.

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