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Meta-analytic evidence for the persuasive effect of narratives on beliefs, attitudes, intentions, and behaviors

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ABSTRACT

Although narratives are often credited with the capacity to change opinions, empirical tests of this prediction have produced mixed results. To provide a more precise test of narrative's effect on beliefs, attitudes, intentions, and behaviors, we performed meta-analyses on studies that evaluated narrative's persuasive influence on these outcomes. Results suggested positive relationships between exposure to a narrative and narrative-consistent beliefs ($k = 37$; $N = 7,376$; $r = .17$), attitudes ($k = 40$; $N = 7,132$; $r = .19$), intentions ($k = 28$; $N = 5,211$; $r = .17$), and behaviors ($k = 5$; $N = 978$; $r = .23$). Moderator analyses on the effect of fictionality yielded mixed results. Neither medium of presentation nor research design influenced the magnitude of the narrative-persuasion relationship. However, results suggested the presence of unidentified moderators.

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Narratives; stories; beliefs; attitudes; intentions; behavior; persuasion

Loosely speaking, a message may be called a *narrative* if it is a story that contains information about setting, characters, and their motivations. In the last two decades, research related to the effects of narratives on opinion change has grown at a dramatic rate. A search of the Communication and Mass Media Complete database for articles with the term “narrative” in the title returned 15 citations for the year 1993. The same search for the year 2003 showed a 380% increase in published articles, and for 2013, a 700% increase. This single database indicates that 1,346 peer-reviewed papers were published on the topic in the last 20 years.

There are many possible reasons for the burgeoning interest in narrative, one of which is the belief that stories can be an especially potent means of creating opinion change. Narratives have been implicated in support for controversial political policies (Iguarta, 2010), the end of slavery (Strange, 2002), improved health (Hinyard & Kreuter, 2007), and recruitment into terrorist groups (Casebeer & Russell, 2005). Other studies, however, report that narrative has little or no effect on these (or other) outcomes (e.g., Cheney, Kohler, & Muilenburg, 2006; Prati, Pietrantoni, & Zani, 2012), or that it produces effects contrary to those advocated (e.g., Gesser-Edelsburg & Endevelt, 2011). Given such inconsistency, whether narratives are as powerful as some believe becomes an empirical issue. As such, this project is oriented toward a single overarching question: Do narratives persuade? As our references

to politics, slavery, health, and terrorism suggest, the answer has considerable applied value. In addition, an estimate of the association between narrative and persuasion (and knowledge of possible moderators) would provide an empirical foundation to the theories of narrative that are currently under construction (e.g., Green & Brock's (2002) Transportation-Imagery Model; Slater & Rouner's (2002) Extended Elaboration Likelihood Model). Given both its applied and theoretical significance, an empirically derived answer to our research question is of substantial importance. Before turning to the central features of the project, we elaborate on the concept of narrative itself.

Narrative, defined

Researchers have defined narratives in terms of events they convey (Abbott, 2008; Genette, 1982), the temporality and sequential nature of those events (Brooks, 1984; Ricoeur, 1980), the causal linkages between the events (Bal, 1997; Onega & Landa, 1996), or the meaning that readers derive from them (Fisher, 1987). Although these definition types provide some information about the nature of narrative, Ryan (2007) argued that they rely too heavily on implicit elements and have therefore each failed to constitute a satisfactory definition. Instead, any definition should support that narratives are about (a) problem solving, (b) conflict, (c) interpersonal relations, (d) human experience, and (e) the temporality of existence.

Using these basic features as a guide, Ryan (2007) developed a "toolkit" for defining narrative. Specifically, she argued that the degree to which a message can be called a narrative is based on eight propositions (p. 29):

- Narratives must be about worlds populated with individual entities
- Narrative worlds must be situated in time and experience significant changes
- Changes to narrative worlds must be caused by non-habitual physical events
- Some narrative event participants must be intelligent beings with agency
- Some narrative events must consist of purposeful action by the agents
- The sequence of narrative events must be causally linked and lead to closure
- At least one of the narrative events must be asserted as fact for the story world
- The story must communicate something meaningful to the audience.

Together, these eight conditions allow scholars to compose their own definitions for narrative based on their research objectives. From this toolkit, we propose that a narrative is a *cohesive, causally linked sequence of events that takes place in a dynamic world subject to conflict, transformation, and resolution through non-habitual, purposeful actions performed by characters*. This definition is useful for the purposes of our analysis, as it distinguishes narrative from several other types of communication, including (but not limited to) static descriptions, lists of causally unconnected events, recipes, pure rhetorical arguments, advice, and instructions.

Focus and scope of this project

To provide an estimate of the relationship between narrative and persuasion, we conducted a series of meta-analyses. Because there are other meta-analyses on related issues, it is important to explain how our research differs from previous efforts. First,

consider Allen and Preiss (1997), who summarized projects that compared narrative evidence to statistical evidence. Their approach is useful for assessing which form of evidence is superior to the other. This is most meaningful if one assumes that narrative and statistical forms are naturally opposed to each other, such that message designers must choose between them. However, we do not believe that to be the case. Narratives in the form of anecdotes constitute evidence and, sometimes, characters in narratives speak in statistical arguments. Accordingly, our efforts focused on experiments that contrasted narrative with control conditions in which no message or no topically relevant message appeared. This approach enabled us to isolate the unbiased persuasive effect of narrative by comparing it to a zero-effect baseline, thereby illustrating whether narratives have the potential to persuade independent of the other types of evidence with which they might be combined.

Our work is also distinct from that of Van Laer, De Ruyter, Visconti, and Wetzels (2014), who focused on narrative transportation, one of several possible mediators of narrative effects (Bilandzic & Busselle, 2013; Moyer-Gusé, 2008). Because of evidence that casts doubt on the construct validity of the transportation scale (e.g., Oliver, Dillard, Bae, & Tamul, 2012),¹ we thought it wise to assess the durability of the narrative-persuasion relationship before attempting to isolate its mediators.

Finally, in contrast to Shen and Han (2014) and Tukachinsky and Tokunaga (2013), we excluded studies that examined the persuasive potency of an entire series of narratives. Although we believe that there is great value in understanding the effects of protracted entertainment-education campaigns, we see equal utility in assessing the effects of individual stories. In addition, the existence of two informative meta-analyses on entertainment-education (Shen & Han, 2014; Tukachinsky & Tokunaga, 2013) made it sensible to focus our efforts on a question that had not yet been studied.

Meta-analysis and knowledge claims

The quantitative summary of primary studies (i.e., meta-analysis) can be expected to yield two types of knowledge claims (Dillard, 1998): relationship-present and relationship-absent claims. Relationship-present claims assert the existence of some association between X and Y. Researchers can understand the magnitude of the relationship in terms of effect-size statistics such as the correlation coefficient. It is also important to assess the degree of certainty that the data provide about the effect size. Certainty can be expressed in terms of the confidence interval or its meta-analytic analog, the credibility interval.

A second sort of knowledge claim relates to the lack of association between two variables. These have been termed relationship-absent claims because they assert the absence of an association between X and Y. Contrary to the dogma embodied in the assumption that “one can never prove the null,” it is quite possible to advance relationship-absent hypotheses and evaluate their validity using information contained in the effect size and the credibility interval. Precise estimates of the likelihood of relationship-absent claims are available via power analyses (Cohen, 1992). That said, given a large number of subjects and studies, such as one might encounter in a meta-analysis, drawing a conclusion may be as simple as looking at a difference and deciding that it is not sufficiently large to be of any consequence.

Although data can be evaluated to provide support for relationship-present and relationship-absent claims, it is possible that some may be uncomfortable with the latter, given the pervasiveness of the former. However, this position is couched in the pre-Popperian philosophy of positivism, which claims that knowledge is based on the recognition of differences. Although this view still holds sway in some circles, scientific investigation has been moving toward a more comprehensive understanding of what constitutes knowledge for the last three decades. More recent conceptions have been influenced by an improved understanding of the deficiencies inherent to statistical significance testing, as well as advances in the utility of power analysis. Currently, social science generally understands that the estimation of effects and non-effects are simply two sides of the same coin. Because specific relationship-present and relationship-absent knowledge claims are of theoretical importance for understanding narrative persuasion, we consider both in our assessment of the data.

The main prediction

Reviews of the narrative literature describe several theoretically grounded reasons to expect narrative to exert an effect on persuasion (Bilandzic & Busselle, 2013). The theoretical paper by Moyer-Gusé (2008) may be the most comprehensive in this regard. She suggests that narrative may persuade by reducing reactance, counter arguing, or selective avoidance; increasing perceived vulnerability or self-efficacy; and/or changing perceived norms or outcome expectancies. Despite the differences associated with these mechanisms of narrative persuasion, each mechanism suggests that narratives induce change in several cognitive and behavioral outcomes.

Given the many theoretical arguments for the persuasive effectiveness of narrative, the primary goal of this project was to synthesize the literature related to narrative's effect on four key indices of persuasion: changes in beliefs, attitudes, intentions, and behavior. Although we address each outcome category separately in our analyses, we make the same prediction for each. Therefore, we offer a single, encompassing hypothesis:

H1: Exposure to a narrative produces story-consistent change in beliefs, attitudes, intentions, and behaviors.

Potential moderators

Fictionality

At first blush, it seems obvious that when individuals are told that a narrative is fictional, they should discount whatever factual claims the story might make. This follows from the notion that tagging a narrative as fictional is equivalent to identifying it as factually inaccurate. However, narrative theorists contend that evaluation of a story's objective truth status is not entirely relevant. Because narrative processing involves the active creation of a story world, narratives are constructed such that they are simply accepted (Busselle & Bilandzic, 2008). On this view, decoupling fact and fiction is an effortful process that occurs subsequent to message acceptance, and may range from difficult to impossible. In other words, the default value for narrative processing is acceptance that requires active, conscious effort to undo (Gilbert, 1991). Obviously, this issue is of profound

theoretical importance to the theory and application of narrative. Consequently, we offer the following relationship-absent hypothesis:

H2: The effect of a narrative on persuasion is not moderated by the perceived fictionality of the narrative.

Medium of presentation

Some researchers have contended that the narrative form exists independent of the medium through which it is communicated (Busselle & Bilandzic, 2008). Stories may be told around campfires, shown in movie theaters, or conveyed in books that may or may not include illustrations. Of course, this does not mean that there are not variations in the quality of stories themselves. Rather, it expresses the interesting theoretical claim that “a story is a story,” regardless of the medium through which it is expressed. By implication, some have argued that medium alone is unimportant to narrative processing or impact. As such, we hypothesize:

H3a: The effect of narrative on persuasion is not moderated by medium of presentation.

It is also apparent that different media possess different attributes that can affect how a narrative is experienced (Busselle & Bilandzic, 2008). Whereas video stories provide images, written narratives require the reader to envision characters, contexts, and actions. Visual media control the rate at which a narrative is presented, but text allows message consumers to read at their own pace. From studies of non-narrative persuasion, there is evidence that these channel variations can influence beliefs and attitudes (Chaiken & Eagly, 1983) via differences in the salience of the source. Green and Brock’s (2002) theory of narrative persuasion suggests a more precise expectation. The emphasis that those authors place on visual imagery suggests that people are persuaded by narrative to the extent that they are transported into that narrative. Thus, precluding the ability of message consumers to generate mental images should inhibit the persuasive potency of narrative. To explore this possibility, we tested the following hypothesis in contrast to H3a:

H3b: The persuasive effect of narratives presented via visual media is weaker than the persuasive effect of narratives presented via nonvisual media.

Conceptual variations within categories of persuasive outcomes

Additional questions revolve around the uniformity of the belief and intention constructs. Although we have thus far described beliefs as if they constituted a single, homogeneous category, research suggests distinctions that are potentially consequential. For example, one type of belief focuses on features of the world (e.g., birds can fly), whereas others emphasize what people believe about how others would have them behave (e.g., I believe that my spouse thinks that I should quit smoking). This longstanding distinction (Morris, 1956) remains part of contemporary theories of norms (Lapinski & Rimal, 2005). Another potentially important conceptual demarcation exists with regard to self-efficacy beliefs, which are couched in the notion that the message consumer can execute the behavior in question. Self-efficacy is an important part of many contemporary theories of persuasion (e.g., Fishbein & Ajzen, 1975), and existing data strongly underscore the value of understanding self-efficacy as a precursor to behavior. Expectations represent yet another

form of beliefs that figure prominently in the study of human communication. Burgoon's expectation violations theory (Burgoon & Jones, 1976), for example, still garners interest from a number of communication researchers. Given the distinctions between them, we test for differences in narrative's capacity to affect change in each of these belief types.

Behavioral intentions represent decisions that individuals make about whether or not to pursue a particular course of action. Similar to the conceptual demarcations between belief types, one can distinguish between intentions to engage in a particular activity as opposed to the intention to urge others to engage in that same activity (e.g., conducting a breast cancer self-examination versus urging one's mother to perform a breast self-exam). Although narrative theory does not parse these opinion variables quite so finely, it remains an open question as to whether or not stories have the power to induce change across both intention types.

RQ1: To what extent, if any, are narrative effects moderated by conceptual variations within categories of persuasive outcomes?

Method

Literature search

We identified studies for the meta-analyses by searching ProQuest's PsycInfo and PsycARTICLES databases, the Communication and Mass Media Complete database, the Thomson Reuters Social Sciences Citation Index, and Google Scholar. To locate theses or dissertations that may not have appeared in these databases, we also searched ProQuest's Dissertations and Theses index, which catalogs students' work among institutions that belong to the Committee for Institutional Cooperation (CIC). When using these databases and indices, we performed title, subject, and keyword searches of the following two sets of search terms in all possible cross-set combinations: (a) *narrative(s)* and *story(ies)* and (b) *persuasion*, *persuade*, *influence*, *beliefs*, *attitudes*, *intentions*, and *behaviors*. These search guidelines produced an initial sample of 6,427 studies. Upon obtaining search results, we read each study's title and abstract to gauge its applicability to the current research. By deleting studies that were (a) irrelevant on the basis of their titles and/or abstracts or (b) duplicates of studies obtained from databases we had already searched, we eliminated 6,181 papers, leaving us with a sample of 246 studies. We then reviewed the reference pages of these 246 studies to identify any relevant research that we may have missed during our electronic searches. This review provided us with an additional 52 studies. As a result of these steps, we identified 298 studies that ostensibly appeared to be viable candidates for inclusion in our analyses.

We then used three criteria concerning the studies' respective methods and designs to determine if they could be included in our analyses. First, the study had to include at least one outcome measure related to beliefs, attitudes, intentions, or behaviors. Second, the study had to have at least one condition in which an effect associated with a single-narrative, outcome-related stimulus was compared to an effect derived from the participant having not been exposed to the narrative stimulus (or any message like it). We allowed effects from both true experiments and quasi-experiments to be included in the meta-analysis, making for six distinct methodological designs among the studies we tested: pretest-posttest, a no-message control condition, an unrelated-message control condition,

pretest-posttest quasi-experiment, pretest-posttest non-equivalent group quasi-experiment, and posttest-only non-equivalent group quasi-experiment (see Trochim, 2006, for descriptions of these design types). Finally, we included only those studies for which a statistical metric of the association between narrative and persuasion that was convertible to Pearson's r was available (either in the study itself or via contact with the study's author[s]). These procedures yielded a final sample of 74 studies performed between 1983 and 2013. Sixty of these studies were published in peer-reviewed journals, eight were Ph.D. dissertations or Master's theses, and six were papers presented at academic conferences. A flowchart depicting our study selection process appears in Figure 1. Effect size estimates and study features are summarized in Tables S1–S4, which are not included in this publication but are available from the first author or online at the Open Science Framework (OSF) data hosting website (<https://osf.io/xwscy/>). References for all studies from which at least one effect size was gleaned are also available from the first author or the OSF (<https://osf.io/9vcqg/>).

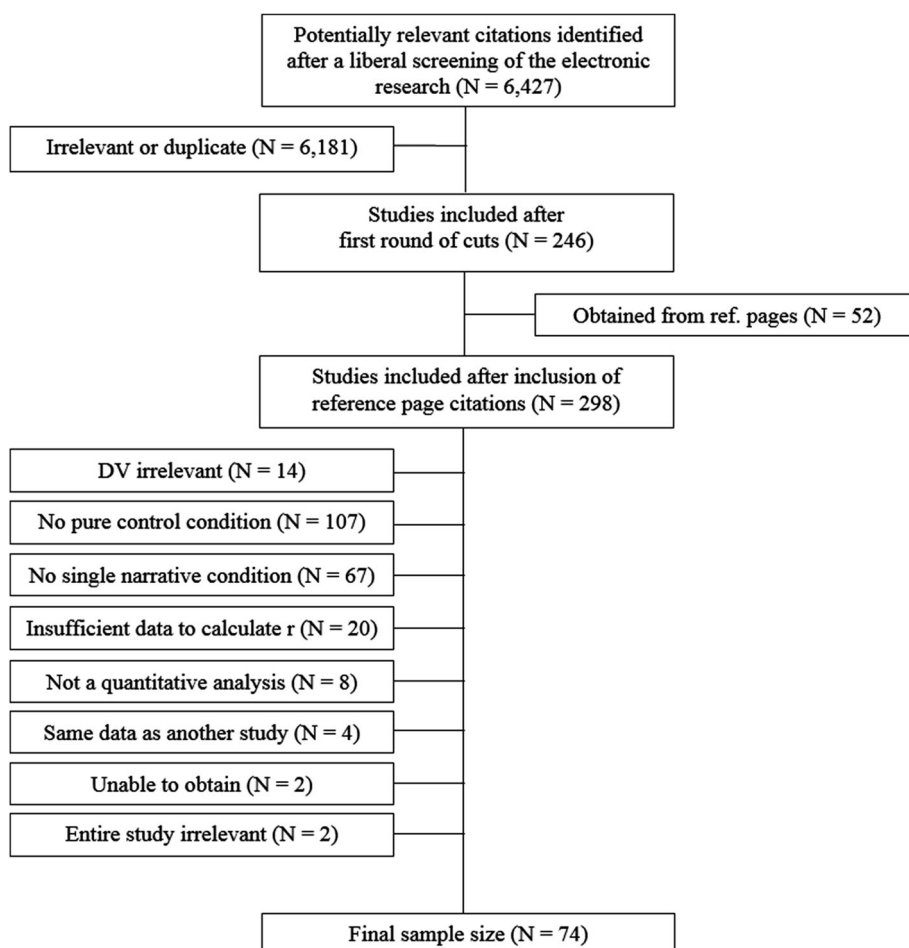


Figure 1. Flowchart of literature search. This figure illustrates the number of studies we obtained and discarded to arrive at our final sample of 74 studies.

Statistical analyses

We used Schmidt and Le's (2005) Meta-Analysis Program (Version 1.1) to execute the Hunter–Schmidt random-effects analyses (Hunter & Schmidt, 2000). Pearson's r was used as the index of effect size. We entered the weighted sample sizes associated with the statistical tests (N_w), individual effect sizes (r), reliability estimates of the independent variables (r_{xx}), and reliability estimates of the dependent variables (r_{yy}) into the program. Inclusion of the reliability estimates allowed for the correction for measurement error for each effect size.

We entered effects as positive if narratives were demonstrated to affect beliefs, attitudes, intentions, or behaviors in a manner consistent with those advocated in the message. Effects were negative if beliefs, attitudes, intentions, or behaviors changed in a manner that was opposite to those advocated in the narrative.

Effect sizes

For each meta-analysis, the parameter used to describe the relationship between the independent variable and dependent variable was the reported correlation weighted by sample size, r_w . We adjusted r_w to correct for measurement error, thus providing us with r'_w . We also calculated an 80% credibility interval ($CrI_{80\%}$) around r'_w for each meta-analysis (Hunter & Schmidt, 2004). Credibility intervals are confidence intervals that have been corrected for statistical artifacts (measurement error only in this paper). As with confidence intervals, they are centered around a point estimate. However, credibility intervals are centered around the corrected point estimate, whereas confidence intervals are centered around the uncorrected point estimate. Corrected estimates are preferred for our analyses, as they more closely reflect the true effect in the population.

Following Hunter and Schmidt (2004), we examined the ratio of expected variance (EV) to observed variance (OV) in r'_w against the 75% rule, which assumes that if 75% of the OV can be attributed to sampling error, it is likely that the remaining 25% is due to artifacts for which there is no possible correction (p. 401). Whereas EV/OV ratios of 75% or higher are considered homogenous, lesser values suggest the presence of moderator variables in the data.

Units of analysis

The unit of analysis (k) for each meta-analysis was the individual statistical test of narrative's persuasive effect on a given dependent measure. In the majority of studies, statistical tests were performed on an array of dependent variables. Because we wished to maximize the number of observations in each meta-analysis, individual statistical tests within studies were extracted and included in the appropriate meta-analysis. In situations when an individual study conducted statistical tests on dependent variables of the same type (i.e., beliefs, attitudes, intentions, or behaviors), we took steps to avoid double-counting participants by either (a) choosing only one effect size for inclusion in the appropriate meta-analysis, or (b) averaging the effect sizes and reliability

estimates to construct a composite-dependent measure for that outcome type. The former option was exercised in cases when there was one distinct statistical test that most closely represented the narrative-outcome relationship. The latter option was exercised in cases when a number of dependent measures were of the same type and were closely related to one another. We also removed effect sizes from moderator analyses in an attempt to bring the number of effects for each moderator type closer to equality.²

Procedures

After retrieving our sample of 74 studies, we converted the reported effect sizes to Pearson's r in all cases for which transformation was necessary. For studies in which these measures of effect size were unavailable in the paper as published, we contacted the corresponding author to request data that would allow us to calculate the relevant effect(s) in that study. Not all authors responded, and not all who did were able to recover the data that we needed to calculate r . Once we obtained all available r coefficients, we classified the effects into one of four categories (beliefs, attitudes, intentions, and behaviors) and performed meta-analyses on the four subsets of data. After estimating the overall effect for each of the four dependent variables, we generated scatterplots of the corrected effect sizes (r') by their corresponding sample sizes (N_w) to facilitate the identification of outliers (Figures 2–5). In addition to using this scatterplot as a general guide, we also identified outlying effect sizes quantitatively by calculating the degree to which all effect sizes deviated from the mean effect size. Effects that were more than two standard deviations from the mean were treated as outliers. Once these effects were identified, we eliminated them from the dataset.

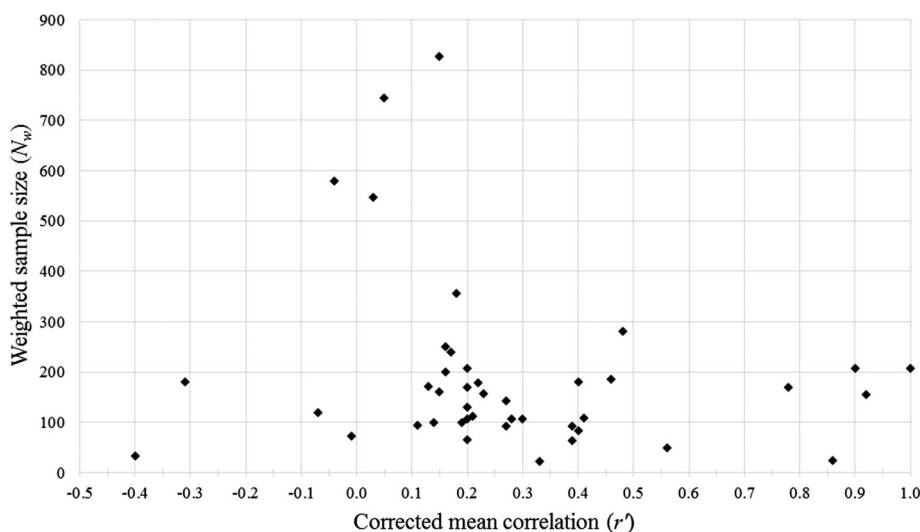


Figure 2. Scatterplot of r' by N_w for belief effects.

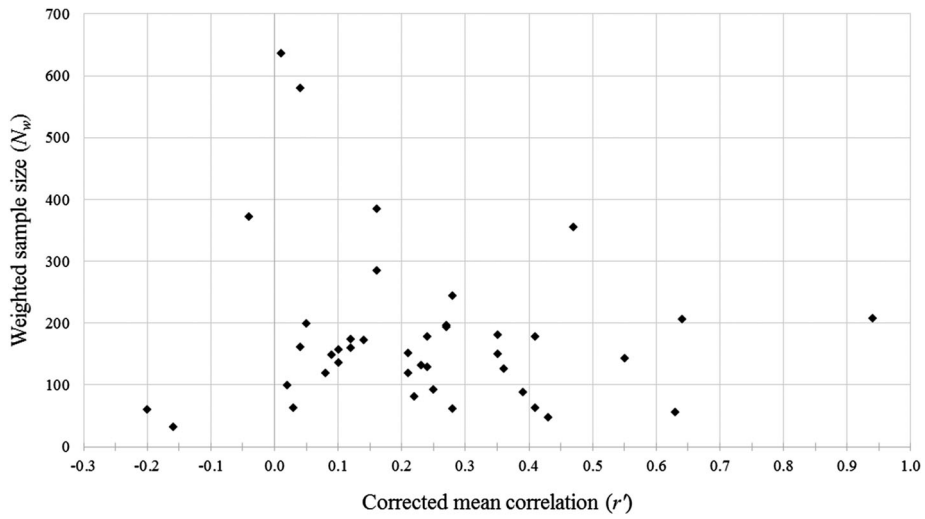


Figure 3. Scatterplot of r' by N_w for attitude effects.

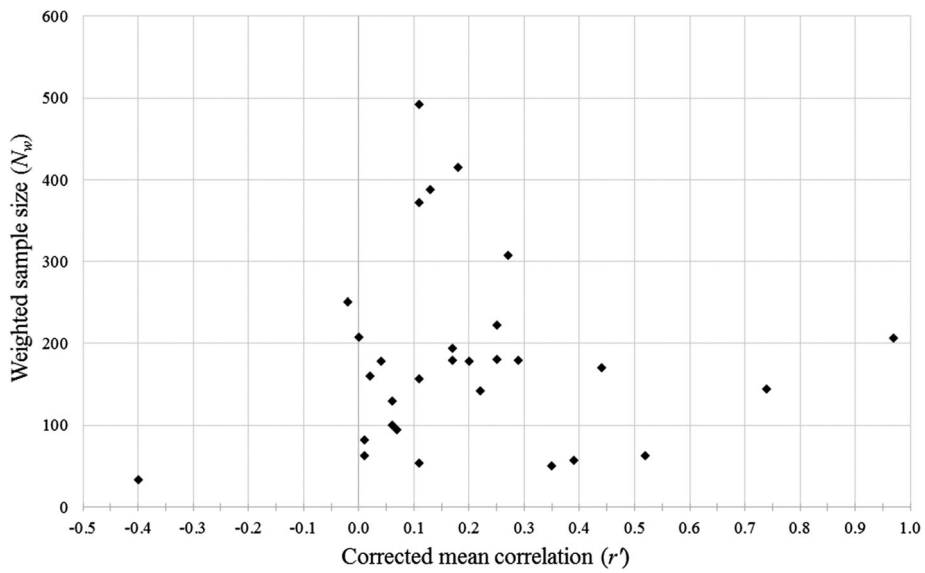


Figure 4. Scatterplot of r' by N_w for intention effects.

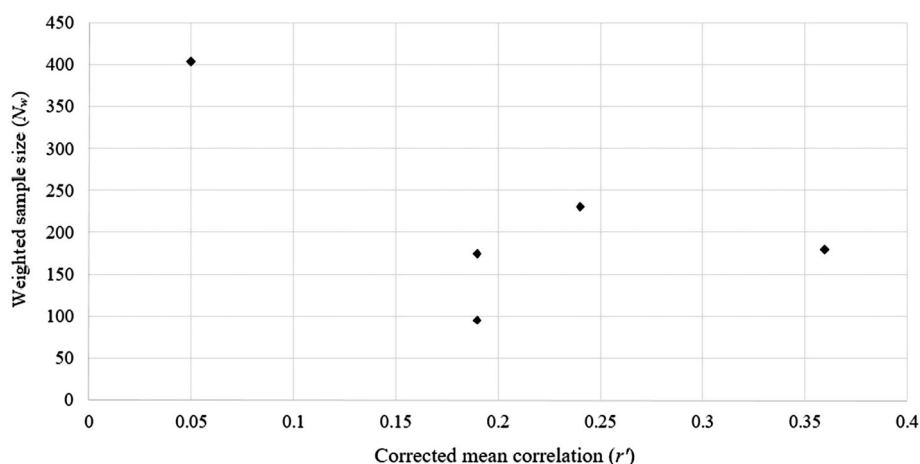


Figure 5. Scatterplot of r' by N_w for behavior effects.

Using the same scatterplots, we also evaluated the data for evidence of publication bias. Publication bias would manifest in the scatterplots as a cluster of large effect sizes paired with relatively small samples. In contrast, a scatterplot that took the shape of an inverted funnel would indicate that data in the meta-analyses are not biased on the basis of publication (Egger, Davey-Smith, Schneider, & Minder, 1997; Levine, Asada, & Carpenter, 2009). The scatterplots indicate that there is a high degree of variance among effects calculated with low sample sizes and a low degree of variance among studies with high sample sizes, producing an inverted-funnel shape of scatterplot data-points.³ Given this result, we saw no evidence of publication bias.

Next, for each of the four outcome variables, we tested for the possibility that variation of effect sizes within each outcome type may be attributable to study design. Because (a) the credibility intervals overlapped in the belief, attitude, and intention data (Tables 1–3), and (b) the behavior data featured only studies with no-message control conditions (with the exception of a single pretest-posttest non-equivalent groups quasi-experimental effect), there was no evidence to suggest that there were differences attributable to study design. Therefore, we collapsed the data related to each outcome across design type and tested for the influence of theoretical moderators.

Results

Beliefs

After removing six outliers, meta-analysis of the belief data ($k = 37$, $N = 7,376$) produced a r'_w of .17 with an 80% credibility interval that did not include zero ($\text{CrI}_{80\%}$: .02–.32). The ratio of expected to OV was only 29%, a value that was well below Hunter and Schmidt's (1990) recommended 75% cut-off. Thus, we performed moderator analyses. See Table 1 for a summary of the results.

Table 1 Summary of results for beliefs

						80% credibility interval									
Variables	<i>r</i>	(<i>SD</i>)	<i>k</i>	<i>N</i>	<i>EV/OV</i>	-1	0	.1	.2	.3	.4	.5			
Main Effect	.17	(.11)	37	7376	29%	.02							.32		
<i>Moderators</i>															
<i>Fictionality</i>															
Fiction	.18	(.19)	25	4055	14%	-.07							.42		
Nonfiction	.25	(.12)	23	2354	44%	.09							.40		
<i>Medium</i>															
Text	.36	(.26)	24	2728	11%	.03							> .70		
Video	.13	(.10)	19	4118	34%	.00							.25		
Audio	.35	(.09)	6	403	69%	.23							.46		
Theater	.15	(.15)	5	506	30%	-.04							.34		
<i>Belief type</i>															
World	.21	(.11)	39	5026	41%	.07							.35		
Normative	.07	(.07)	3	1056	39%	-.02							.16		
Efficacy	.23	(.09)	4	592	46%	.11							.34		
Expectations	.08	(.00)	5	596	100%	.08							.08		
<i>Control type</i>															
PrePost	.26	(.00)	10	906	100%	.26							.26		
Control msg.	.13	(.17)	12	1801	22%	-.09							.35		
No message	.20	(.10)	22	3217	43%	.08							.33		
PrePostQE	.12	(.13)	4	443	32%	-.06							.29		
PrePostNEGQE	.07	(.15)	5	1253	15%	-.12							.26		

Notes: PrePostQE = Pretest-posttest quasi-experiment. PrePostNEGQE = Pretest-posttest, non-equivalent groups quasi-experiment.

Fictionality

Partitioning the data on perceived fictionality ($k_{\text{fiction}} = 25$, $N_{\text{fiction}} = 4,055$; $k_{\text{NF}} = 23$, $N_{\text{NF}} = 2,354$) revealed a slightly larger mean effect for nonfiction narrative stimuli. However, the credibility intervals overlapped. Therefore, there was no evidence to suggest that belief change differs as a function of a narrative's perceived fictionality. However, the $\text{CrI}_{80\%}$ for the fictional data included zero, whereas interval for the nonfictional stimuli did not. This suggests that although the two mean effects are not significantly different, nonfictional narrative stimuli significantly affect belief change, but fictional narrative stimuli do not. This inferential problem turned out to be common in subsequent analyses.

Medium

Because the credibility intervals associated with text ($k_{\text{text}} = 24$, $N_{\text{text}} = 2,728$), video ($k_{\text{video}} = 19$, $N_{\text{video}} = 4,118$), audio ($k_{\text{audio}} = 6$, $N_{\text{audio}} = 403$), and theatrical ($k_{\text{theater}} = 5$, $N_{\text{theater}} = 506$) stimuli overlapped, we saw no evidence that narrative effects varied as a function of medium.⁴ However, the $\text{CrI}_{80\%}$ associated with theatrical stimuli included zero, whereas the credibility intervals associated with text-, video-, and audio-based stimuli did not.

Belief type

We classified beliefs into four categories: world beliefs ($k_{\text{world}} = 39$, $N_{\text{world}} = 5,026$), normative beliefs ($k_{\text{norm}} = 1056$, $N_{\text{norm}} = 1,056$), efficacy beliefs ($k_{\text{effic}} = 4$, $N_{\text{effic}} = 592$), and expectations ($k_{\text{expect}} = 596$, $N_{\text{expect}} = 596$). World beliefs relate to participants'

perceptions of the world around them (e.g., I believe that ghosts exist; Ramsey, Venette, & Rabalais, 2011). Normative beliefs concern participants' judgments about how others would have them behave (e.g., others believe that I should be an organ donor; Morgan, King, Smith, & Ivic, 2010). Efficacy beliefs are those that pertain to an individual's faith that he or she can perform a particular task or embody a particular characteristic (e.g., I can engage in safer sexual practices; Kiene & Barta, 2003). Finally, expectations are beliefs about what may happen in the future (e.g., I may develop skin cancer at some point in my life; Dunlop, Wakefield, & Kashima, 2009). Similar to the fictionality and medium moderator analyses, the credibility intervals associated with each respective belief type overlapped with each other, suggesting they were not different from one another.

Attitudes

Meta-analysis of the attitude data ($k = 40$, $N = 7,132$) produced an r'_w of .19 ($\text{CrI}_{80\%} = .001-.39$). Similar to the belief data, the ratio of EV to OV was well below 75% ($\text{EV/OV} = 20\%$), so we again performed moderator analyses (see Table 2).

Table 2 Summary of results for attitudes

						80% credibility interval						
Variables	<i>r</i>	(SD)	<i>k</i>	<i>N</i>	EV/OV	-.1	0	.1	.2	.3	.4	.5
Main Effect	.19	(.16)	40	7132	20%	.00 ----- .39						
<i>Moderators</i>												
<i>Fictionality</i>												
Fiction	.20	(.15)	40	4983	27%	.01 ----- .40						
Nonfiction	.14	(.18)	12	1879	19%	-.08 ----- .37						
<i>Medium</i>												
Text	.20	(.21)	22	2637	18%	-.07 ----- .47						
Video	.20	(.15)	23	3263	26%	.01 ----- .39						
Audio	.14	(.14)	2	225	31%	-.03 ----- .32						
Theater	.11	(.02)	5	702	96%	.09 --- .13						
<i>Control type</i>												
PrePost	.28	(.08)	8	1236	28%	.12 ----- .43						
Control msg.	.12	(.20)	19	2877	15%	-.13 <----- .38						
No message	.21	(.14)	19	2175	36%	.03 ----- .38						
PrePostQE	.15	(.09)	5	702	50%	.03 ----- .26						
PrePostNEGQE	.19	(.00)	2	359	100%	.19 .19						

Notes: PrePostQE = Pretest-posttest quasi-experiment. PrePostNEGQE = Pretest-posttest, non-equivalent groups quasi-experiment.

Fictionality

The overlap in the respective credibility intervals suggested that there was no significant difference between fictional ($k_{\text{fiction}} = 40$, $N_{\text{fiction}} = 4,983$) and nonfictional ($k_{\text{NF}} = 12$, $N_{\text{NF}} = 1,879$) stimuli in their capacity to change attitudes. However, these results are complicated by the fact that the credibility interval associated with nonfictional stimuli included zero, whereas the credibility interval associated with fictional stimuli did not. This suggests that fictional narratives are an effective means of inducing attitude change, but nonfictional narratives are not.

Medium

Once again, overlapping 80% credibility intervals for all four types of media ($k_{\text{text}} = 22$, $N_{\text{text}} = 2,637$; $k_{\text{video}} = 23$, $N_{\text{video}} = 3,263$; $k_{\text{audio}} = 2$, $N_{\text{audio}} = 225$, $k_{\text{theater}} = 5$, $N_{\text{theater}} = 702$) indicated that no one stimulus was more effective than another in changing participants' attitudes. Nevertheless, two of the four types of media (video and theater) produced credibility intervals that did not include zero, suggesting that video-based and theatrical narrative stimuli are significant drivers of attitude change.

Intentions

Analysis of the intention data ($k = 28$, $N = 5,211$) showed a positive effect with a credibility interval that did not include zero ($r'_w = .17$; $\text{CrI}_{80\%} = .01-.32$). The EV/OV ratio did not clear the 75% criterion, so we conducted moderator analyses on these data (see Table 3).

Table 3 Summary of results for intentions

Variables	r	(SD)	k	N	EV/OV	80% credibility interval	
Main Effect	.17	(.14)	28	5211	26%	.01	.32
<i>Moderators</i>							
<i>Fictionality</i>							
Fiction	.14	(.06)	20	2547	59%	.06	.22
Nonfiction	.21	(.19)	10	1652	14%	-.03	.46
<i>Medium</i>							
Text	.17	(.18)	11	1819	16%	-.05	.40
Video	.14	(.03)	14	2797	85%	.10	.18
Theater	-.02	(.11)	3	226	54%	-.17 <	.12
<i>Orientation</i>							
Self	.14	(.09)	25	4303	43%	.02	.25
Other	.23	(.00)	4	752	100%	.23	.23
<i>Control type</i>							
PrePost	.17	(.00)	2	268	100%	.17	.17
Control msg.	.19	(.07)	8	1203	57%	.09	.28
No message	.12	(.12)	10	1570	30%	-.03	.28
PrePostQE	.12	(.27)	3	226	15%	-.22 <	.47
PrePostNEGQE	.14	(.00)	5	914	100%	.14	.14
PostNEGQE	.14	(.00)	2	907	100%	.14	.14

Notes: PrePostQE = Pretest-posttest quasi-experiment. PrePostNEGQE = Pretest-posttest, non-equivalent groups quasi-experiment. PostNEGQE = Posttest-only, non-equivalent groups quasi-experiment. Results of the moderator analyses for which $k = 2$ (i.e., the PrePost and PostNEGQE analyses) should be interpreted with caution, as results of meta-analytic techniques are most valid when $k \geq 3$.

Fictionality

The overlap in the respective credibility intervals suggests that there is no significant difference between fictional ($k_{\text{fiction}} = 20$, $N_{\text{fiction}} = 2,547$) and nonfictional ($k_{\text{NF}} = 10$, $N_{\text{NF}} = 1,652$) narrative stimuli with respect to intention change. That said, the effects related to nonfictional narratives yielded a $\text{CrI}_{80\%}$ that included zero. In contrast, the $\text{CrI}_{80\%}$ derived from fictional stimuli effects did not.

Medium

Our meta-analyses of effect sizes related to text-based ($k_{\text{text}} = 11$, $N_{\text{text}} = 1,819$), video-based ($k_{\text{video}} = 14$, $N_{\text{video}} = 2,797$), and theatrical narratives ($k_{\text{theater}} = 3$, $N_{\text{theater}} = 226$) yielded a series of positive values. But, as with other effect types, the credibility intervals overlapped substantially, indicating that no one medium was superior to others at inciting intention change.

Intention orientation

Finally, we distinguished the intention effect sizes on the basis of whether the narrative from which they were derived advocated a behavioral intention that was oriented toward the individual exposed to the narrative ($k_{\text{self}} = 25$, $N_{\text{self}} = 4,303$) or a valued other ($k_{\text{other}} = 4$, $N_{\text{other}} = 752$). For example, one study (Greene & Brinn, 2003) included a measure for the extent to which the participant intended to use sunblock in the future. We dubbed this a “self-oriented” outcome. In another study (Wilkin et al., 2007), there was a measure for the extent to which participants intended to suggest that their female relatives get mammograms. We called this an “other-oriented” outcome. Effect sizes were positive in both analyses and neither credibility interval included zero.

Behaviors

Analysis of the behavior data ($k = 5$, $N = 978$) revealed a r'_w of .23 ($\text{CrI}_{80\%} = .22-.23$). An EV/OV ratio of 100% indicated that sampling error could account for the OV in the data set. Accordingly, we did not pursue analyses of moderator variables.

Summary of results

Taken together, the results of the main and moderator analyses reveal several notable findings. First, none of the credibility intervals associated with narrative’s main effect on beliefs, attitudes, intentions, or behaviors included zero. This suggests that although the r'_w associated with each of these analyses was relatively small, exposure to narrative stimuli does have a significant and positive effect on these outcomes. These results provide empirical support for Hypothesis 1. Second, our tests of theoretically relevant moderators showed that narrative’s effect on beliefs, attitudes, intentions, and behaviors was not influenced by the narrative’s fictionality (supporting Hypothesis 2), the medium through which the narratives are presented (supporting Hypothesis 3a and refuting Hypothesis 3b), or other outcome-specific factors (in response to Research Question 1).

Discussion

The effect of narrative on persuasion

The primary contribution of this research was the estimation of the causal impact of narrative on persuasion. The results were consistent across the four outcome variables—beliefs, attitudes, intentions, and behaviors—with effect size estimates ranging from .17 to .23. In none of the four cases did the 80% credibility intervals encompass zero. For beliefs, attitudes, and intentions, the findings were based on a relatively large number of

studies (28–40) and a large number of research participants (5,211–7,376). The lack of publication bias in the data (see Note 3 and Figures 2–5) indicates that across the literature, journals have apparently not given preference to studies that produce large effect sizes over those that produce small effect sizes. Thus, there is no evidence to suggest that the relationship between narrative and persuasion that we describe in this study is falsely inflated as a function of publication bias. Moreover, the effects were not an artifact of any particular research design. Thus, we can categorically conclude that *narrative does exert a causal influence on four of the most common indices of persuasion*.

What should we make of the claim that narrative is an especially potent means of persuasion? Here, a more nuanced answer is required. In conjunction with past research on persuasion, our findings suggest that the effects for narrative are about on par with other types of persuasive messages. A review of nine meta-analyses of persuasion variables showed an average effect size (r) of .18 (Dillard, 1998). Thus, if effect sizes are viewed competitively, narrative is holding its own against some stalwart challengers. Second, it is important to note that even modest effects can yield important changes. This is obviously true in domains wherein the winner takes all regardless of the size of the majority vote (e.g., elections). Less obviously, but equally true, small effects of narratives may cumulate with repetition, thereby collectively causing substantial change. Third, however, it is important to note that our analysis treated narrative as if it has a direct effect on persuasion. To our knowledge, no one believes that the process is that simple. Rather, narrative researchers have proposed multiple mechanisms through which narrative may exert its effects (Bilandzic & Busselle, 2013; Moyer-Gusé, 2008). Measurement of these mediating processes is needed for a more complete understanding of how narrative persuades. As this work proceeds, it will be useful to bear in mind that although narrative may function through multiple processes, there is no requirement that they are all operative in every circumstance (Niederdeppe, Shapiro, & Porticella, 2011). Efforts to construct a comprehensive theory of narrative will need to incorporate both moderators *and* mediators. To the extent that entertainment-education can be viewed as a type of narrative, the meta-analyses of Shen and Han (2014) and Tukachinsky and Tokunaga (2013) have already made valuable contributions along these lines.

Fictionality

Samuel Coleridge is credited with advancing the idea that message recipients engage in the willing suspension of disbelief if a writer is able to imbue a tale with human interest and a semblance of truth. Precisely the opposite idea has become a mainstay of narrative research. Following the philosophy of Baruch Spinoza (see Curley, 1988), Gilbert (1991) proposed that people initially accept information as true, then evaluate its veracity only if they are motivated to do so. Thus, individuals are naturally inclined toward belief. In addition, rather than suspend disbelief willingly, audiences must actively and effortfully construct disbelief (Busselle & Bilandzic, 2008). This logic leads to the expectation that narrative content will be accepted as true in many circumstances. Even when message consumers are told that a story is fictional, they may not be inclined to engage in the mental effort to undo the work that went into cognitively creating the story world.

To a certain extent, the data were consistent with the notion that perceived fictionality does not cancel out the effects of narrative. For the belief, attitude, and intention data, the

credibility intervals for fictional and non-fictional narratives overlapped, allowing us to conclude that they are not significantly different. However, for the belief data, the credibility interval associated with fictional narratives included zero, but the credibility interval associated with nonfictional narratives did not. For the attitude and intention data, however, the credibility intervals for fictional narratives did not include zero, but the credibility interval associated with the nonfictional narratives did. Although the results are not simple, they do not necessarily discredit the notion that narratives are processed in a way that involves simultaneous comprehension and acceptance. However, *the results do require that we look more closely into the possibility that narrative processing may not be as categorically Spinozan as has been proposed*. We see this as one of the most important unresolved issues facing the study of narrative persuasion.

Medium of presentation

The data suggested that narratives can be effective means of inducing opinion changes irrespective of the medium of presentation. Given the large number of subjects and studies, coupled with the observation that there was no evidence of medium differences across any of the four outcome variables, it seems that we can place considerable confidence in this finding. Further confidence may be drawn from the fact that medium effects were absent in both meta-analyses regarding entertainment-education messages (Shen & Han, 2014; Tukachinsky & Tokunaga, 2013). However, we emphasize that prior studies, as well as our own, have limited their focus to main effects of medium. It is possible that the medium through which a narrative is presented may interact with other variables to affect persuasion. For instance, one primary study showed an interaction between medium and need for cognition (NFC) on transportation (Green et al., 2008). Whereas high NFC individuals were more transported when reading, low NFC persons reported higher transportation when watching a film. Additional primary studies would be needed to determine the durability of these findings. To date, however, *existing data support the conclusion that medium of presentation is unimportant for inducing persuasion via narrative*.

Other possible influences of the magnitude of narrative effect

To understand the generality of narrative effects, we tested several potential moderator variables, including research design, belief type, and intention orientation. *None of the three proved useful in terms of accounting for variations in the magnitude of the narrative effect*. Using the 80% credibility standard, no trustworthy differences were observed (with the possible exception of efficacy vs. expectation belief types). This might be viewed as a failure to identify the variables that enhance or inhibit the persuasive power of narrative. However, these no-difference findings are also theoretically important. They demonstrate that narrative effects are robust across belief type, research design, and intention type. Although more research is needed to isolate the variables that *do* moderate narrative effects, the current project makes a contribution to the literature by effectively ruling out these three.

Strengths, limitations, and the nature of our knowledge claims

As with any investigation, this one possesses certain strengths and weaknesses. On the positive side of the ledger, we posed clear theoretical questions, utilized a sizeable database, and with the possible exception of the results related to fictionality, produced unequivocal answers.

Our basic comparison—narrative conditions versus a zero baseline—might be viewed as both a strength and a weakness. Its value lies in its straightforward simplicity: It is a pure estimate of the effects of narrative. Still, one can question its internal validity. At the extreme, it could be argued that the results are not due to narrative per se, but could result from features of the stimuli other than their narrative structure (e.g., priming). It is impossible to rule this out entirely, but an example from another context may help to clarify the inferential risks. Assume that a clinical trial of a new anti-cancer drug shows an effect size of .17 when comparing the experimental and control arms of the study. The result could be due to the drug, although it might be the result of the way in which the drug was administered, the attention given to patients during follow-up, or the phase of the moon during which the trial was conducted. The inescapable fact is that there are always threats to validity that are uncontrolled. Whether or not these threats constitute real dangers to inference is a judgment call that researchers and readers must make for themselves.

Some readers of an earlier version of this manuscript believed that these inferential hazards would be diminished or eliminated altogether if we had compared narrative to some other message form (e.g., statistical argument) rather than no message. In reality, that comparison makes the situation even more complex. This is so because either narrative or argument could potentially interact with other, unknown variables. But, this concern also misses a fundamental difference in the nature of the knowledge claims that are possible from the different comparisons. We sought to generate understanding of the absolute effects of narrative, whereas comparisons of narrative versus statistical argument offer knowledge about the relative effects of the message forms. Both are valuable, but they are distinct, one being no better than the other except insofar as it satisfies the goals of the researchers.

Conclusions

This study makes three key contributions to our knowledge of narrative effects. First, it demonstrates that exposure to narratives can affect message recipients' beliefs, attitudes, intentions, and behaviors such that they move into closer alignment with viewpoints espoused in those narratives. More work is needed to isolate message features that enhance or inhibit narrative potency, but the basic relationship is not in doubt. Second, the data were ambiguous with regard to the question of fictionality. Thus, there is a need to examine more closely the idea the narratives are fully accepted at the time of processing. Third, the results support the position that narrative form is uninfluenced by medium of presentation. Unless researchers anticipate interactions between medium and some other variable, it appears unlikely that additional research on medium will produce a desirable return on investment. Although much remains to be done, this trio of results extends our basic understanding of whether, how, and the degree to which narratives persuade.

Notes

1. More recent studies have provided some evidence for the validity of a newer version of the transportation scale (i.e., its short form (TS-SF); Appel, Gnambs, Richter, & Green, 2015), which other studies have successfully implemented (e.g., Gnambs, Appel, Schreiner, Richter, & Isbener, 2014).
2. For example, in a moderator analysis designed to test whether the type of belief targeted (i.e., world vs. efficacy vs. normative vs. expectation beliefs) affected the relationship between narrative exposure and changes in beliefs, we found that a study we included (Schofield & Pavelchak, 1989) contained a total of three relevant effects, each of a different type (world, efficacy, and expectations). Because the authors estimated these effect sizes using the same sample ($N = 179$), we could not incorporate all three effects into our meta-analysis because doing so would artificially increase the total N . Similarly, we could not take the mean of the three effects and maintain a sample size of 179; doing so would erroneously confound effect types for which we sought to identify differences. To avoid these difficulties, we eliminated the world and expectation belief effects from this particular moderator analysis for two reasons. First, the number of world and expectation belief effects in this moderator analysis was greater than the number of efficacy beliefs; eliminating the world and expectation effects and retaining the efficacy-related effect brought the number of world, expectation, and efficacy belief effects into closer numerical balance. We viewed this as desirable in the same way that equal cell sizes are desirable. Second, by retaining the one efficacy effect, we were able to retain all 179 participants in the moderator analysis without concern for double-counting participants from this particular study.
3. In addition to inspecting the scatterplots visually, we also used Egger et al. (1997) and Tang and Liu's (2000) statistical test of plot asymmetry. This test produces metrics for gauging the degree to which the plot adheres to the inverted funnel shape. In this test, the product of the studies' effect sizes and the square roots of their weighted sample sizes ($E = r' \times \sqrt{N_w}$) are regressed on the square roots of their weighted sample sizes ($\sqrt{N_w}$). If the resulting regression equation ($E = \alpha + \beta(\sqrt{N_w})$) produces an intercept (α) near zero and a corresponding p -value of greater than .10, then the assumption of plot asymmetry is rejected. Moreover, a negative α indicates that imprecise studies have a larger effect than more precise studies, indicating further bias in the meta-analysis results (Tang & Liu, 2000). For the belief, attitude, and intention data, the respective intercepts (and corresponding p -values) were 2.71 (.07), 1.84 (.21), and .14 (.37). Taken together, these results and a visual inspection of the scatterplots demonstrate that the assumption of asymmetry was not significant for the attitude and intention data, indicating that their plots are symmetrical and shaped as an inverted funnel. Results associated with the belief data were marginal ($p = .07$). However, when coupled with a visual inspection of the scatterplot and a consideration of the positive α derived from the regression analysis, the results seem to indicate general symmetry and lack of publication bias in the belief data as well. Use of this method for calculating asymmetry is inadvisable when there are fewer than 10 studies in a meta-analysis, as the power of the test would be too low to distinguish true asymmetry from the illusion of asymmetry as a function of chance (Higgins & Green, 2011). Because the behavior data consist of fewer than 10 data points, we did not test it for plot asymmetry. More research relating narrative exposure to behavior change must be performed to determine if publication bias exists among studies that explore that particular relationship.
4. For this moderator analysis, we identified one effect size based on a video game-based narrative stimulus (i.e., Sangalang, Johnson, & Ciancio, 2013). Because it was the only effect derived from a video game stimulus, we were unable to perform standard meta-analytic procedures on it. The attitude and intention data also included this study, but because it was again the only video-game-based effect in these respective datasets, we could not include it in their moderator analyses related to medium.

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