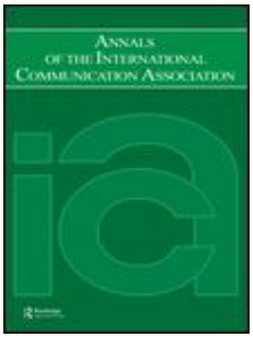

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8 The Relative Persuasiveness of Different Message Types Does Not Vary as a Function of the Persuasive Outcome Assessed

Evidence from 29 Meta-Analyses of 2,062 Effect Sizes for 13 Message Variations

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Experiments that compare the persuasiveness of two message types (e.g., strong vs. weak fear appeals) characteristically examine persuasive impact using attitudinal, intention, or behavioral outcomes. The equivalence of these three outcomes as indices of relative persuasiveness is assessed by re-analyzing data from 2,062 effect sizes in 29 meta-analyses of 13 different message variations, including one-sided and two-sided messages, negative political advertising, and several fear appeal variations. The relative persuasiveness of alternative message types is found to be largely invariant across these different outcomes: If message type A is more persuasive than message type B with attitudinal outcomes, it is also—and equally—more persuasive with intention and behavioral outcomes. Methodological and theoretical implications are discussed.

One recurring question in communication research concerns the relative persuasiveness of alternative message types, in general or under specified conditions. For instance: Are stronger fear appeals more persuasive than weaker ones? Are examples more persuasive than statistics? Are implicit conclusions more persuasive than explicit conclusions for audiences initially opposed to the advocated view? And so forth.

Such questions are commonly addressed through randomized trials (experiments) in which participants are exposed to one or another message condition, with persuasive outcome variables assessed subsequently. The most commonly-studied persuasive outcome variables are attitude (overall evaluation: attitude toward the advocated policy, the advertised product, and so forth), intention (e.g., purchase intention or voting intention), and behavior (either self-reported or observed). These are not the only possible indicators of persuasive effect, but attitudinal, intention, and behavioral outcomes are the most frequently employed outcome variables in persuasion effects research.

A potential problem arises, however, because different studies of a given message variation can employ different indices of persuasive effect. One

study might examine attitudinal outcomes, another intention outcomes, a third behavioral outcomes. In comparing or synthesizing findings across a set of such studies, one might naturally wonder whether studies assessing one index of persuasiveness ought to be unproblematically lumped with studies using other assessments.

This problem arises especially acutely in the context of meta-analysis, that is, quantitative research synthesis aimed at providing (inter alia) an estimate of the size of the effect associated with the message variation. When the primary research being summarized contains more than one of these persuasive outcomes, a question naturally arises as to how to proceed. Meta-analytic summaries of persuasion research have followed three different general approaches with respect to this issue. One has been to report results (mean effect sizes, etc.) separately for attitudinal, intention, and behavioral outcomes (e.g., Witte & Allen, 2000). A second is to review only studies using a single kind of outcome; for example, Sopory and Dillard (2002) examined only attitudinal outcomes, Keller and Lehman (2008) examined only effects on intentions, and Noar, Benac, and Harris (2007) examined only behavioral outcomes. A third is to combine results indiscriminately across the three outcomes (e.g., O'Keefe & Jensen, 2006).

These different approaches to persuasion meta-analysis will have different weaknesses depending on whether effect sizes (ESs) vary across persuasive outcomes. If ESs do not vary across outcomes, then the first two approaches—reporting results separately for different outcomes or restricting the review to one kind of outcome—will suffer needlessly diminished statistical power and needless vulnerability to outlier effects (e.g., false positives). By contrast, if ESs do vary across outcomes, then the last approach—combining ESs across the different outcomes—will be entirely inappropriate.

Several commentators have suggested that persuasion ESs should be expected to vary depending on which outcome variable is assessed (e.g., Floyd, Prentice-Dunn, & Rogers, 2000, p. 421; Reinhart, 2006, pp. 17–18). Two related rationales suggest themselves. One is the differential ease of influencing these outcomes; attitudes are presumably easier to change than are intentions, and intentions in turn easier to change than behaviors. The other is the presumable causal sequence in which messages influence attitudes, which influence intentions, which influence behaviors. Taken together, these two considerations might suggest that ESs are likely to vary across persuasive outcomes, progressively weakening as one moves from attitude to intention to behavior.

Thus the broad question addressed in the present project is whether, in assessing the relative persuasiveness of alternative message types, effect sizes vary across attitudinal, intention, and behavioral outcomes. This is a question of considerable importance when it comes to establishing sound generalizations about the persuasive effects of message variations. If, on the one hand, the relative persuasiveness of two message types is roughly similar across attitudinal, intention, and behavioral indicators, then (for example) two studies that use different outcome variables may nevertheless be appropriately compared.

On the other hand, if the relative persuasiveness of two message forms varies considerably across these different indicators, then one will want different generalizations about effects on attitude, on intention, and on behavior.

Plainly, no individual experiment can provide very decisive evidence on this question. For example, even if the ES associated with a given message variable was, in a given study, identical across attitudinal, intention, and behavioral outcomes, that would not provide evidence that the same pattern would occur in other studies of that message variable or in studies of other message variables.

Hence to address this question, data from existing meta-analyses were reanalyzed. By way of brief overview, suitable meta-analyses of message variables studied for their effects on persuasive outcomes were identified. Each meta-analysis's ESs were separated on the basis of the outcome variable assessed (attitude, intention, and behavior), and the resulting mean ESs (for the different outcomes for that message variable) were compared. This analysis permits one to see whether, for the purposes of assessing the relative persuasiveness of two message types, attitudinal outcomes, intention outcomes, and behavioral outcomes yield similar ESs and so are functionally equivalent or yield different ESs and so are functionally distinct.

Methods

Literature Search and Inclusion Criteria

Potential meta-analyses of interest were initially identified by searches through January 2012 of ERIC, Medline, ProQuest Dissertations and Theses, PsycEXTRA, PsycINFO, and Web of Science combining *meta-analysis* with such terms as *persuasion*, *message*, and *attitude*. Additional candidates were located through examination of textbooks and through personal knowledge of the literature.

The analysis was restricted to meta-analyses of the effects of a persuasive message variation on attitudinal, intention, or behavioral outcomes, where at least two of those outcomes were assessed (across the studies reviewed) and where appropriate information was available to permit the necessary reanalyses (information about the ES, sample size, and outcome variable for the cases included in the meta-analysis). These criteria thus excluded meta-analyses that did not examine message-variation effects (e.g., Milne, Sheeran, & Orbell, 2000), meta-analyses involving only one kind of persuasive outcome (e.g., Argo & Main, 2004; Cruz, 1998; Hamilton & Hunter, 1998; Reinard, 1998), and meta-analyses for which necessary additional information was not available (Allen & Preiss, 1997; Boster & Mongeau, 1984; Floyd et al., 2000; Grewal, Kavanoor, Fern, Costley, & Barnes, 1997; Mongeau, 1998). Additionally, to provide a modicum of statistical power, in each meta-analysis, analyses involving a given outcome variable were excluded unless at least five ESs were available for that outcome for both initial and any follow-up analyses (described below); all or part of several meta-analyses were excluded for this

reason (Burrell & Koper, 1998; Eisend, 2009; Gayle, Preiss, & Allen, 1998; Hale, 1998; Hornikx and O'Keefe, 2009; O'Keefe, 1998, 1999, 2000, 2002; Reinhart, 2006). If multiple suitable meta-analytic datasets were available for a given message variable, each such dataset was analyzed.

Included Meta-Analyses

These inclusion criteria yielded a total of 29 meta-analyses, with 2,062 ESs in all, concerning the effects of 13 diverse persuasive message variations: gain-loss framing, message sidedness, threat severity, fear appeal strength, threat vulnerability, cultural value adaptation, humor in advertising, response efficacy, negative political advertising, self-efficacy, conclusion explicitness, legitimizing paltry contributions, and recommendation specificity. Details about each message variation and its corresponding meta-analytic data follow.

Gain-Loss Framing. This message variation contrasts gain-framed messages, which emphasize the advantages of compliance with the recommended action, and loss-framed messages, which emphasize the disadvantages of not complying with the recommended action. Meta-analyses of this message variation have often been less interested in framing effects across all topics (public policy issues, consumer advertising, health-related topics, and so forth) than in framing effects concerning specifically disease detection topics (e.g., mammography) and disease prevention topics (e.g., using sunscreen). Correspondingly, re-analyses of these datasets were conducted for all topics, for detection topics only, and for prevention topics only. The datasets of Akl et al. (2011), Gallagher and Updegraff (2012), and Kyriakaki (2007) contributed to all three analyses.[1] The datasets of O'Keefe and Jensen (2006, 2007, 2009) contributed to, respectively, the all-topics analysis, the prevention-topic analysis, and the detection-topic analysis. For the all-topics analysis, across all datasets, a total of 526 ESs were available; for the prevention-topic and detection-topic analyses, 294 and 187 ESs were available, respectively. Positive ESs indicated a persuasive advantage for gain-framed appeals.

Message Sidedness. This message variation contrasts one-sided messages, which present only supporting arguments, and two-sided messages, which both present supporting arguments and discuss opposing arguments. The datasets of Eisend (2006, 2007) and O'Keefe (1999) were analyzed. Across the two datasets, a total of 222 ESs were available. Positive ESs indicated a persuasive advantage for two-sided messages.

Threat Severity. This fear appeal-related message variation contrasts messages varying in the depicted severity of a potential threat; the contrast is thus between messages suggesting that the threat is relatively more severe (high threat severity) and messages suggesting that the threat is relatively less severe (low threat severity). The datasets of de Hoog, Stroebe, and de Wit

(2007) and Witte and Allen (2000) were analyzed. Across the two datasets, a total of 192 ESs were available. Positive ESs indicated a persuasive advantage for high depicted threat severity.

Fear Appeal Strength. This message variation contrasts messages varying in the explicitness and vividness of the depictions of the threatened consequences; the contrast is thus between strong (relatively more explicit and vivid) and weak (relatively less explicit and vivid) fear appeals. The datasets of Sutton (1982) and Witte and Allen (2000) were analyzed. Across the two datasets, a total of 126 ESs were available. Positive ESs indicated a persuasive advantage for strong fear appeals.

Threat Vulnerability. This fear appeal-related message variation contrasts messages varying in the depicted vulnerability (susceptibility) of the message recipient to the potential threat; the contrast thus is between messages that depict the receiver as relatively more vulnerable to the threat (high threat vulnerability) and messages that depict the receiver as relatively less vulnerable to the threat (low threat vulnerability). The datasets of de Hoog et al. (2007) and Witte and Allen (2000) were analyzed. Across the two datasets, a total of 118 ESs were available. Positive ESs indicated a persuasive advantage for messages with high depicted threat vulnerability.

Cultural Value Adaptation. This message variation contrasts consumer advertisements varying in the degree to which the appeals are adapted to the audience's cultural values, as when Chinese and American audiences receive either collectivistic appeals (adapted to Chinese audiences) or individualistic appeals (adapted to American audiences); the contrast is thus between culturally-adapted and culturally-unadapted appeals. The dataset of Hornikx and O'Keefe (2009), providing a total of 96 ESs, was analyzed. Positive ESs indicated a persuasive advantage for messages with culturally-adapted appeals.

Humor in Advertising. This message variation contrasts humorous and non-humorous consumer advertisements. The dataset of Eisend (2009), providing a total of 95 ESs, was analyzed. Positive ESs indicated a persuasive advantage for humorous advertisements.

Response Efficacy. This fear appeal-related message variation contrasts messages with differing depictions of the efficaciousness (effectiveness) of the recommended course of action; the contrast thus is between messages that depict the recommended action as relatively more effective (high response efficacy) and messages that depict the recommended action as relatively less effective (low response efficacy). The datasets of de Hoog et al. (2007) and Witte and Allen (2000) were analyzed. Across the two datasets, a total of 65 ESs were available. Positive ESs indicated a persuasive advantage for messages with high depicted response efficacy.

Negative Political Advertising. This message variation contrasts political messages varying in evaluative tone; the contrast thus is between negative and positive political advertising. Lau, Sigelman, and Rovner's (2007) data for net affect (attitude), vote intention, and actual vote choice (behavior), providing a total of 53 ESs, were analyzed. Positive ESs indicated a persuasive advantage for negative campaigning.

Self-Efficacy. This fear appeal-related message variation contrasts messages with differing depictions of the message recipient's ability to adopt or engage in the advocated action; the contrast thus is between messages that depict the action as one that is relatively easy to adopt (high self-efficacy) and messages that depict the action as one that is relatively difficult to adopt (low self-efficacy). The dataset of Witte and Allen (2000), providing a total of 40 ESs, was analyzed. Positive ESs indicated a persuasive advantage for messages with high depicted self-efficacy.

Conclusion Explicitness. This message variation contrasts messages varying in the explicitness of the message's overall conclusion; the contrast thus is between messages in which the conclusion is stated overtly (explicit conclusion) and messages in which that conclusion is left unstated (implicit conclusion). The dataset of O'Keefe (2002), providing a total of 18 ESs, was analyzed. Positive ESs indicated a persuasive advantage for messages with explicit conclusions.

Legitimizing Paltry Contributions. This research examines the effectiveness of a donation-request strategy that explicitly legitimizes making a small contribution, as compared to a control-condition request without such legitimization. These studies examined either intention outcomes (pledges to donate) or behavioral outcomes (actual donations). Andrews, Carpenter, Shaw, and Boster's (2008) data for face-to-face implementations, providing a total of 18 ESs, were analyzed. Positive ESs indicated a persuasive advantage for the experimental condition (legitimizing paltry contributions).

Recommendation Specificity. This message variation contrasts messages on the basis of the specificity of the description of the recommended action; the contrast thus is between messages that provide only a general description of the recommended action and messages that provide a more detailed recommendation. The dataset of O'Keefe (2002), providing a total of 12 ESs, was analyzed. Positive ESs indicated a persuasive advantage for messages with more specific recommendations.

Effect Sizes and Analyses

Effect Size. Correlation (r) was used as the ES metric. Many of the meta-analyses recorded ESs in terms of r; for those that did not, ESs were converted to r using widely available formulas (e.g., Borenstein, Hedges, Higgins, & Rothstein, 2009, pp. 45–49; Card, 2012, pp. 118–119).

Outcome Variables. For each meta-analysis, for each ES, the outcome variable involved (attitude, intention, behavior) was identified. Where composite ESs (based on more than one outcome) were originally reported, information was acquired to obtain a separate ES for each different outcome variable. Thus, each included meta-analysis yielded a list of cases, where each case provided an ES (with some associated sample size) for one of the three outcome variables. The ESs were accepted as given in each meta-analytic dataset; that is, ESs were not recomputed, adjusted, deleted, or otherwise altered (save, as indicated above, where a composite ES was replaced by separate ESs for different outcomes). (Thanks to Mike Allen, Kyle Andrews, Natascha de Hoog, Martin Eisend, Kristel Gallagher, Rick Lau, and Kim Witte for providing additional information about their meta-analytic datasets.)

Analysis. For each meta-analysis's list of cases, a meta-analytic re-analysis was undertaken that separated cases on the basis of the outcome variable involved (attitude, intention, behavior). This yielded two or three groups of cases for each meta-analysis, depending on how many different outcomes were available. The average ES in each group of cases was computed. Random-effects meta-analytic procedures were used to produce the means and confidence intervals of interest and to test the significance of differences between mean effect sizes (with a separate estimate of τ^2, the variance of true effect sizes across studies, for each group: Borenstein et al., 2009, pp. 164–171; Borenstein & Rothstein, 2005). Random-effects analyses were used because generalization beyond the cases in hand is of interest (Borenstein, Hedges, Higgins, & Rothstein, 2010; Card, 2012, pp. 233–234; Hedges & Vevea, 1998).

For any comparison that yielded a significant difference between the mean ESs for two outcome variables within a meta-analysis, a follow-up analysis was conducted to consider between-studies differences as a possible explanation. If a given message variable's mean effect on (for example) behavioral outcomes was significantly different from the mean effect on attitudinal outcomes, one possible explanation would be that studies that assessed behavioral outcomes differed in some way from studies that assessed attitudinal outcomes; this between-studies difference could be responsible for the observed variation of ESs across different outcomes. Hence, when such significant differences appeared in the initial analysis, a follow-up analysis was undertaken that was limited to studies that obtained data on both outcomes of interest (provided at least five ESs were available for each outcome); analysis of data from such within-study comparisons permits one to assess extraneous between-studies differences as a possible explanation for overall differences.

Results

Table 8.1 provides a summary of the mean effect sizes. Across the 29 meta-analyses of the 13 message variations, the data afforded a total of 63 comparisons between mean effects. Of these, 59 were nonsignificant. Of the four significant differences, only two remained significant in follow-up analyses restricted to within-study comparisons.

Table 8.1 Mean Effect Sizes (*r*) and 95% Confidence Intervals for Message Variations across Different Outcomes

Message Variation	*Attitude*	*Intention*	*Behavior*
Gain-Loss Framing: All Topics			
Akl et al. (2011)		–.027 (*k* = 23) [–.103, .048]	–.058 (*k* = 16) [–.126, .011]
Gallagher & Updegraff (2012)	.024 (*k* = 59) [–.026, .075]	.004 (*k* = 78) [–.036, .043]	.030 (*k* = 52) [–.001, .062]
Kyriakaki (2007)	–.014 (*k* = 19) [–.062, .034]	–.000 (*k* = 35) [–.038, .037]	.028 (*k* = 19) [–.027, .083]
O'Keefe & Jensen (2006)	$.040_{a}$ (*k* = 82) [.004, .075]	$.027_{b}$ (*k* = 101) [–.006, .061]	$–.022_{ab}$ (*k* = 42) [–.050, .006]
Gain-Loss Framing: Prevention			
Akl et al. (2011)		.027 (*k* = 13) [–.073, .126]	–.092 (*k* = 5) [–.296, .121]
Gallagher & Updegraff (2012)	.037 (*k* = 46) [–.016, .090]	.022 (*k* = 47) [–.024, .067]	.078 (*k* = 32) [.036, .121]
Kyriakaki (2007)	.018 (*k* = 14) [–.025, .062]	.020 (*k* = 24) [–.027, .067]	.087 (*k* = 10) [.008, .165]
O'Keefe & Jensen (2007)	.088 (*k* = 30) [.024, .152]	.032 (*k* = 58) [–.004, .068]	.021 (*k* = 15) [–.046, .088]
Gain-Loss Framing: Detection			
Akl et al. (2011)		.016 (*k* = 6) [–.128, .159]	–.039 (*k* = 10) [–.088, .009]
Gallagher & Updegraff (2012)	–.040 (*k* = 14) [–.174, .096]	–.024 (*k* = 30) [–.102, .055]	–.038 (*k* = 20) [–.081, .004]
Kyriakaki (2007)	–.122 (*k* = 5) [–.256, .018]	–.041 (*k* = 11) [–.098, .017]	–.025 (*k* = 9) [–.098, .048]
O'Keefe & Jensen (2009)	–.027 (*k* = 33) [–.078, .024]	–.051 (*k* = 34) [–.107, .004]	–.039 (*k* = 15) [–.080, .003]
Message Sidedness			
Eisend (2006)	.117 (*k* = 65) [.067, .166]	.082 (*k* = 37) [–.027, .190]	
O'Keefe (1999)	–.010 (*k* = 94) [–.049, .028]	–.012 (*k* = 26) [–.081, .057]	
Threat Severity			
de Hoog et al. (2007)	.128 (*k* = 40) [.074, .182]	.116 (*k* = 55) [.079, .152]	.094 (*k* = 41) [.044, .143]
Witte & Allen (2000)	.152 (*k* = 14) [.064, .238]	.147 (*k* = 26) [.085, .207]	.120 (*k* = 16) [.057, .183]

Message Variation	*Attitude*	*Intention*	*Behavior*
Fear appeal strength			
Sutton (1982)		.154 (k = 13) [.110, .196]	.149 (k = 8) [.031, .262]
Witte & Allen (2000)	.144 (k = 34) [.104, .183]	.147 (k = 43) [.095, .198]	.159 (k = 28) [.081, .236]
Threat vulnerability			
de Hoog et al. (2007)	$-.041_{ab}$ (k = 19) [–.138, .057]	$.162_{a}$ (k = 31) [.085, .236]	$.188_{b}$ (k = 19) [.089, .284]
Witte & Allen (2000)	.104 (k = 11) [–.017, .222]	.167 (k = 27) [.096, .235]	.132 (k = 11) [.058, .205]
Cultural value adaptation			
Hornikx & O'Keefe (2009)	.067 (k = 65) [.021, .113]	.096 (k = 31) [.022, .170]	
Humor in advertising			
Eisend (2009)	.189 (k = 49) [.086, .288]	.192 (k = 46) [.110, .272]	
Response efficacy			
de Hoog et al. (2007)		.119 (k = 12) [.071, .166]	.123 (k = 6) [.045, .199]
Witte & Allen (2000)	.178 (k = 11) [.074, .277]	.198 (k = 24) [.126, .268]	.137 (k = 12) [.073, .200]
Negative political ads			
Lau et al. (2007)	–.073 (k = 10) [–.216, .073]	–.010 (k = 27) [–.093, .074]	–.036 (k = 16) [–.073, .001]
Self-efficacy			
Witte & Allen (2000)	.188 (k = 8) [.036, .332]	.199 (k = 21) [.124, .272]	.145 (k = 11) [.066, .222]
Conclusion explicitness			
O'Keefe (2002)	.102 (k = 13) [–.002, .203]		.137 (k = 5) [–.060, .323]
Legitimizing paltry contributions			
Andrews et al. (2008)		.157 (k = 5) [–.011, .316]	.179 (k = 13) [.125, .232]
Recommendation specificity			
O'Keefe (2002)	.001 (k = 5) [–.087, .089]	–.041 (k = 7) [–.119, .038]	

Note: Within a row, means with a common subscript are significantly different ($p < .05$).

Gain-Loss Framing: All Topics

In studies of gain-loss framing across all topics, in Akl et al.'s (2011) dataset, the mean ES for intention outcomes (–.027) did not differ significantly from the mean ES for behavior outcomes (–.058; $Q(1) = .349$, $p = .555$).

In Gallagher and Updegraff's (2012) dataset, the mean ES for attitude outcomes (.024) did not differ significantly from the mean ES for intention outcomes (.004; $Q(1) = .405$, $p = .525$) or from the mean ES for behavior outcomes (.030; $Q(1) = .041$, $p = .839$). The mean ESs for intention outcomes and for behavior outcomes did not differ significantly ($Q(1) = 1.105$, $p = .293$).

In Kyriakaki's (2007) dataset, the mean ES for attitude outcomes (–.014) did not differ significantly from the mean ES for intention outcomes –.000; $Q(1) = .187$, $p = .666$) or from the mean ES for behavior outcomes (.028; $Q(1) = 1.256$, $p = .262$). The mean ESs for intention outcomes and for behavior outcomes did not differ significantly ($Q(1) = .698$, $p = .404$).

In O'Keefe and Jensen's (2006) dataset, the mean ESs for attitudinal outcomes (mean $r = .040$) and intention outcomes (.027) did not significantly differ ($Q(1) = .259$, $p = .611$). The mean ESs for attitudinal outcomes (.040) and behavioral outcomes (–.022) were significantly different ($Q(1) = 7.260$, $p = .007$); however, in a follow-up analysis restricted to studies affording the relevant within-study comparison ($k = 11$), the mean ESs (for attitude, mean $r = –.030$, 95% C [–.113, .052]; for behavior, mean $r = –.017$, 95% C [–094, .060]) did not significantly differ ($Q(1) = .052$, $p = .819$). The mean ESs for intention outcomes (.027) and behavioral outcomes (–.022) were significantly different ($Q(1) = 4.910$, $p = .027$); however, in a follow-up analysis restricted to studies reporting both intention and behavioral outcomes ($k = 15$), the mean ESs (for intention, mean $r = –.015$, 95% C [–.075, .045]; for behavior, mean $r = –.002$, 95% C [–.084, .079]) did not significantly differ ($Q(1) = .060$, $p = .806$).

Gain-Loss Framing: Prevention

In studies of gain-loss framing in disease prevention messages, in Akl et al.'s (2011) dataset, the mean ES for intention outcomes (.027) did not differ significantly from the mean ES for behavior outcomes (–.092; $Q(1) = .974$, $p = .324$).

In Gallagher and Updegraff's (2012) dataset, the mean ES for attitude outcomes (.037) did not differ significantly from the mean ES for intention outcomes (.022; $Q(1) = .187$, $p = .665$) or from the mean ES for behavior outcomes (.078; $Q(1) = 1.430$, $p = .232$). The mean ES for intention outcomes (.022) did not differ significantly from the mean ES for behavior outcomes (.078; $Q(1) = 3.191$, $p = .074$).

In Kyriakaki's (2007) dataset, the mean ES for attitude outcomes (.018) did not differ significantly from the mean ES for intention outcomes (.020; $Q(1) = .002$, $p = .962$) or from the mean ES for behavior outcomes (.087; $Q(1) = 2.252$, $p = .133$). The mean ES for intention outcomes (.020) did not differ significantly from the mean ES for behavior outcomes (.087; $Q(1) = 2.071$, $p = .150$).

In O'Keefe and Jensen's (2007) dataset, the mean ES for attitude outcomes (.088) did not differ significantly from the mean ES for intention outcomes (.032; $Q(1) = 2.234$, $p = .135$) or from the mean ES for behavior outcomes (.021; $Q(1) = 2.003$, $p = .157$). The mean ES for intention outcomes (.032) did not differ significantly from the mean ES for behavior outcomes (.021; $Q(1) = .080$, $p = .777$).

Gain-Loss Framing: Detection

In studies of gain-loss framing in disease detection messages, in Akl et al.'s (2011) dataset, the mean ES for intention outcomes (.016) did not differ significantly from the mean ES for behavior outcomes (–.039; $Q(1) = .502$, $p = .479$).

In Gallagher and Updegraff's (2012) dataset, the mean ES for attitude outcomes (–.040) did not differ significantly from the mean ES for intention outcomes (–.024; $Q(1) = .040$, $p = .841$) or from the mean ES for behavior outcomes (–.038; $Q(1) = .000$, $p = .985$). The mean ES for intention outcomes (–.024) did not differ significantly from the mean ES for behavior outcomes (–.038; $Q(1) = .104$, $p = .747$).

In Kyriakaki's (2007) dataset, the mean ES for attitude outcomes (–.122) did not differ significantly from the mean ES for intention outcomes (–.041; $Q(1) = 1.114$, $p = .291$) or from the mean ES for behavior outcomes (–.025; $Q(1) = 1.447$, $p = .229$). The mean ES for intention outcomes (–.041) did not differ significantly from the mean ES for behavior outcomes (–.025; $Q(1) = .105$, $p = .746$).

In O'Keefe and Jensen's (2009) dataset, the mean ES for attitude outcomes (–.027) did not differ significantly from the mean ES for intention outcomes (–.051; $Q(1) = .401$, $p = .527$) or from the mean ES for behavior outcomes (–.039; $Q(1) = .120$, $p = .729$). The mean ES for intention outcomes (-.051) did not differ significantly from the mean ES for behavior outcomes (–.039; $Q(1) = .130$, $p = .718$).

Message Sidedness

In studies of message sidedness, in Eisend's (2006, 2007) dataset, the mean ESs for attitude outcomes (.117) and intention outcomes (.082) did not significantly differ ($Q(1) = .318$, $p = .573$).

In O'Keefe's (1999) dataset, the mean ESs for attitudinal outcomes (–.010) and intention outcomes (–.012) did not significantly differ ($Q(1) = .002$, $p = .966$).

Threat Severity

In studies of threat severity, in de Hoog et al.'s (2007) dataset, the mean ES for attitudinal outcomes (.128) did not significantly differ from the mean ES for intention outcomes (.116; $Q(1) = .135$, $p = .714$) or from the mean ES for

behavioral outcomes (.094; $Q(1) = .844$, $p = .358$). The mean ESs for intention outcomes and behavioral outcomes did not significantly differ ($Q(1) = .496$, $p = .481$).

In Witte and Allen's (2000) dataset, the mean ES for attitudinal outcomes (.152) did not significantly differ from the mean ES for intention outcomes (.147; $Q(1) = .011$, $p = .917$) or from the mean ES for behavioral outcomes (.120; $Q(1) = .342$, $p = .559$). The mean ESs for intention outcomes and behavioral outcomes did not significantly differ ($Q(1) = .348$, $p = .555$).

Fear Appeal Strength

In studies of fear appeal strength, in Sutton's (1982) dataset, the mean ES for intention outcomes (.154) and the mean ES for behavior outcomes (.149) did not differ significantly ($Q(1) = .006$, $p = .941$.

In Witte and Allen's (2000) dataset, the mean ES for attitudinal outcomes (.144) did not significantly differ from the mean ES for intention outcomes (.147; $Q(1) = .009$, $p = .923$) or from the mean ES for behavioral outcomes (.159; $Q(1) = .124$, $p = .725$). The mean ESs for intention outcomes and behavioral outcomes did not significantly differ ($Q(1) = .069$, $p = .793$).

Threat Vulnerability

In studies of threat vulnerability, in de Hoog et al.'s (2007) dataset, the mean ES for intention outcomes (.162) did not differ significantly ($Q(1) = .177$, $p = .674$) from that for behavioral outcomes (.188), but each differed (for intention, $Q(1) = 10.159$, $p = .001$; for behavior, $Q(1) = 10.315$, $p = .001$) from the mean ES for attitudinal outcomes (–.041). In an analysis restricted to studies affording the relevant within-study comparison, when studies had both attitudinal and intention outcomes ($k = 12$), the mean effects (for attitude, mean $r = -.004$, 95% C [–.093, .085]; for intention, mean $r = .270$, 95% CI [.160, .374]) significantly differed ($Q(1) = 14.152$, $p < .001$). When studies had both attitudinal and behavioral outcomes ($k = 8$), the mean effects (for attitude, mean $r = .013$, 95% C [–.059, .084]; for behavior, mean $r = .250$, 95% CI [.123, .368]) significantly differed ($Q(1) = 10.080$, $p = .001$).

In Witte and Allen's (2000) dataset, the mean ES for attitudinal outcomes (.104) did not significantly differ from the mean ES for intention outcomes (.167; $Q(1) = .781$, $p = .377$) or from the mean ES for behavioral outcomes (.132; $Q(1) = .153$, $p = .696$). The mean ESs for intention outcomes and behavioral outcomes did not significantly differ ($Q(1) = .443$, $p = .506$).

Cultural Value Adaptation

In studies of cultural value adaptation, in Hornikx and O'Keefe's (2009) dataset, the mean ESs for attitudinal outcomes (.067) and intention outcomes (.096) did not significantly differ ($Q(1) = .424$, $p = .515$).

Humor in Advertising

In studies of humor in advertising, in Eisend's (2009) dataset, the mean ESs for attitudinal outcomes (.189) and intention outcomes (.192) did not significantly differ ($Q(1) = .002$, $p = .963$).

Response Efficacy

In studies of depicted response efficacy variations, in de Hoog et al.'s (2007) database, the mean ES for intention outcomes (.119) and the mean ES for behavior outcomes (.123) did not differ significantly ($Q(1) = .007$, $p = .933$).

In Witte and Allen's (2000) dataset, the mean ES for attitudinal outcomes (.178) did not significantly differ from the mean ES for intention outcomes (.198; $Q(1) = .103$, $p = .748$) or from the mean ES for behavioral outcomes (.137; $Q(1) = .435$, $p = .509$). The mean ESs for intention outcomes and behavioral outcomes did not significantly differ ($Q(1) = 1.552$, $p = .213$).

Negative Political Advertising

In studies of negative political advertising, in Lau et al.'s (2007) dataset, the mean ES for attitudinal outcomes (–.073) did not significantly differ from the mean ES for intention outcomes (–.010; ($Q(1) = .544$, $p = .461$) or from the mean ES for behavioral outcomes (–.036; ($Q(1) = .232$, $p = .630$). The mean ESs for intention outcomes and behavioral outcomes did not significantly differ ($Q(1) = .315$, $p = .575$).

Self-Efficacy

In studies of depicted self-efficacy variations, in Witte and Allen's (2000) dataset, the mean ESs for attitudinal outcomes (.188) did not significantly differ from the mean ES for intention outcomes (.199; $Q(1) = .017$, $p = .898$) or from the mean ES for behavioral outcomes (.145; $Q(1) = .254$, $p = .614$). The mean ESs for intention outcomes and behavioral outcomes did not significantly differ ($Q(1) = .982$, $p = .322$).

Conclusion Explicitness

In studies of conclusion explicitness, in O'Keefe's (2002) dataset, the mean ESs for attitudinal outcomes (.102) and behavioral outcomes (.137) did not significantly differ ($Q(1) = .096$, $p = .757$).

Legitimizing Paltry Contributions

In studies of legitimizing paltry contributions, in Andrews et al.'s (2008) dataset, the mean ESs for intention outcomes (.157) and behavioral outcomes (.179) did not significantly differ ($Q(1) = .061$, $p = .805$).

Recommendation Specificity

In studies of recommendation specificity, in O'Keefe's (2002) dataset, the mean ESs for attitudinal outcomes (.001) and intention outcome (–.041) did not significantly differ ($Q(1) = .494$, $p = .482$).

Discussion

The General Pattern

The general picture that emerges from these data is that the relative persuasiveness of alternative message forms does not vary much as a function of whether attitudinal, intention, or behavioral outcomes are assessed. A glance across each row in Table 8.1 makes it plain that, in general, these different persuasive outcomes yield quite similar conclusions concerning the relative persuasiveness of the message variations reviewed here. For 11 of the 13 message variables, none of the mean effects is statistically significantly different from another.

One apparent exception emerged in one aspect of the analyses of gain-loss message framing effects. No significant differences were found between message framing mean ESs based on different outcomes for studies of disease prevention messages (across four meta-analyses) or studies of disease detection messages (again, across four meta-analyses). And in analyses across all topics, three of the four meta-analytic reviews also found no significant differences (Akl et al., 2011; Gallagher & Updegraff, 2012; Kyriakaki, 2007). But in one meta-analysis (O'Keefe & Jensen, 2006), the behavioral-outcome mean ES was significantly different from the mean ESs for attitudinal and intention outcomes. However, those differences evaporated when other between-studies differences were removed.

The other apparent exception emerged in the analyses of threat vulnerability effects. One meta-analytic database yielded significant differences that persisted in the follow-up analysis (de Hoog et al., 2007), but a second meta-analytic database produced no such differences (Witte & Allen, 2000). This result is discussed in more detail below.

In all other analyses, no significant differences were found. In general, then, these results are strikingly consistent. The available meta-analytic databases afforded 63 comparisons between mean ESs involving different outcome variables, and only two such comparisons were statistically significant in a follow-up analysis (in the threat vulnerability data of de Hoog et al., 2007)—and even those two differences did not appear in another meta-analysis of that same message variable (by Witte & Allen, 2000).

The general consistency of this pattern is especially notable given the diversity of the evidentiary base. A range of message variables was represented in this analysis; some consist of substantive variation in the appeals advanced (as in the contrast between one-sided and two-sided messages, which present

different arguments), as opposed to what might seem to be more superficial variations (such as gain-loss framing, where the same argument is framed differently). And the meta-analytic databases that were re-analyzed arose from a variety of procedural decisions about which studies to include, how to compute effect sizes, and so forth. The present analysis did not, for example, recompute effect sizes, or alter the inclusion criteria, or re-do the literature retrieval procedures of the individual meta-analyses; whatever procedures yielded the original meta-analytic databases were accepted at face value, even when these differed from one meta-analysis to another. The consistency of the obtained results thus suggests a certain robustness of effect, in the sense of being impervious to these various substantive and methodological dissimilarities.

To be sure, when multiple meta-analyses have been conducted concerning a given message variable—with at least some of the same studies contributing to each meta-analytic dataset—a finding of consistent results across those meta-analyses may not be entirely surprising. But even those meta-analyses can and do have procedural differences (different search procedures, different inclusion criteria, different procedures for computing effect sizes, and so forth), which means that the observed consistency of results was not guaranteed. In such circumstances, the replication of the result in multiple meta-analyses of a given message variation only strengthens the conclusion that attitudinal, intention, and behavioral assessments generally provide functionally equivalent indices of relative persuasiveness.

A compelling example is provided by the two meta-analyses of message sidedness. Eisend's (2006, 2007) review was restricted to studies of consumer advertising, and the dataset included multiple ESs from a given study for a given type of outcome (e.g., if a study had two assessments of intention, two separate ESs were entered in the dataset).[2] O'Keefe's (1999) review included studies of both advertising and other topics (public policy questions, health behaviors, and so forth), and the dataset collapsed ESs from a given study for a given type of outcome (so that if two intention assessments were available in a study, the ESs were averaged to create a single intention ES). Despite these procedural differences, and despite mean ESs that look rather different (e.g., a mean ES for attitudinal outcomes of .12 in one meta-analysis and -.01 in the other), within each meta-analysis there were no significant differences between the mean ESs for different outcomes.

Persuasiveness and Relative Persuasiveness

It is important to be clear about what the present results do and do not show. These results do not show that attitudinal, intention, and behavioral assessments are equivalent indices of *persuasiveness*. They show that in experiments comparing two messages, attitudinal, intention, and behavioral assessments are equivalent indices of *relative persuasiveness*.

This distinction can be embodied in two questions. The first is: How persuasive is a given message? Examination of attitudinal, intention, and behavioral

outcomes might yield different answers to such a question. For example, a given message might appear quite persuasive when attitudes are assessed, but produce little persuasion when behavioral outcomes are examined.

The second question is: What is the difference in persuasiveness between two messages? That is, what is the relative persuasiveness of two messages? The present results indicate that examination of attitudinal, intention, and behavioral outcomes will yield substantively identical answers to such a question. If message A is more persuasive than message B when attitudes are assessed, then message A will also be more persuasive than message B—and more persuasive to the same degree—if either intentions or behaviors are assessed.

So, although the *persuasiveness* of a single message might vary across attitudinal, intention, and behavioral outcomes, the present results indicate that the *relative persuasiveness* of two messages does not vary across those outcomes.

Implications

These results have implications for primary persuasion research, for research synthesis in persuasion, and for practical message pretesting. First, in primary research concerning specifically questions of the relative persuasiveness of two message forms, these results suggest that a research design need not assess all three kinds of outcome discussed here. The present results give considerable confidence that, in general, the same conclusion (both about the direction of effect and about the magnitude of effect) will be given by attitudinal outcomes, intention outcomes, and behavioral outcomes. In that sense, these three outcomes are functionally equivalent with respect to the assessment of relative persuasiveness.

To be clear: The claim advanced here is not that the difference between the population effects for any two kinds of outcome is literally zero (the statistical "null hypothesis"). The claim is simply that these different outcome measures are functionally interchangeable with respect to research questions concerning relative message persuasiveness. And these results do not show that attitude, intention, and behavior are functionally equivalent variables for all research questions (i.e., do not show that these variables will always give the same answer to any question whatever—such as questions about the persuasiveness of a single message). These results indicate that when the research question concerns specifically the relative persuasiveness of two message types, the same answer to that question will be given by any of these kinds of outcome.

Second, research synthesis (e.g., meta-analysis) aimed at assessing the relative persuasiveness of two message forms should be conducted in ways that recognize this functional equivalence. For example, when two studies with different outcome assessments yield different conclusions about the persuasive effects of a given message variable, those conflicting results should presumably not be ascribed to the different outcome assessments, but rather to other causes. In meta-analytic research, when more than one of these three kinds of outcome are available, a meta-analyst appropriately can and should combine

ESs across outcomes. Analyzing these three outcomes separately or restricting a meta-analysis to one kind of outcome incurs all the costs associated with smaller-sample studies, such as enhanced vulnerability to false positives and reduced ability to detect either main effects (simple differences between the two message types) or moderator-variable effects (differences between subsets of studies).

Third, these results have implications for practical persuasive-message pretesting, as when formative persuasive campaign research compares two or more possible messages with the purpose of identifying the most effective one. For the specific goal of pinpointing which message is likely to be most persuasive, campaign planners need not collect attitudinal, intentional, and behavioral outcome data. Any one of these three kinds of assessment will suffice to identify the most persuasive message.

Inconsistent with Previous Findings?

One might suspect that the present results must somehow be inaccurate because they appear to be inconsistent with various well-established findings and theoretical frameworks. In particular, these results might seem inconsistent with (a) the differential ease of influencing these three outcomes, (b) the causal sequence of the three outcome variables, and (c) Gallagher and Updegraff's (2012) meta-analytic results. Each of these is discussed in turn.

Differential Ease of Influence. Attitudes are presumably easier to change than are intentions, and intentions in turn easier to change than behaviors. Thus (one might reason) effect sizes should be largest for attitudes, smaller for intentions, and smaller still for behaviors—but the present results are inconsistent with this expectation. So one might think that something must be amiss with the present results.

But this reasoning is not sound. Even if these three outcomes are differentially easy to change, effect sizes could still be constant across them. This mistake in reasoning may arise from a confusion concerning what effect sizes represent in the meta-analyses reviewed here—and specifically a confusion between persuasiveness (of a single message) and relative persuasiveness (of two messages).

In these meta-analyses, the effect size (ES) for a given study represents the difference in persuasive effect between the two message conditions being compared (the relative persuasiveness of the two messages), not the persuasive effect (persuasiveness) of any one message in that study or the overall persuasive effect across message conditions in that study. For instance, an ES of zero in these meta-analyses indicates that there was no difference in persuasiveness between the two message conditions—but this does not necessarily mean that the messages were not individually persuasive. In fact, both of a study's two messages could be highly persuasive, but if the messages were *equally* highly persuasive then the ES would be zero.

So even if the absolute amount of persuasion varies by outcome (with some outcomes more easily influenced than others), the difference in persuasiveness between the two message conditions—the effect size—could be identical for the three outcomes. To see this concretely, imagine having an index of message persuasiveness ranging from zero (no persuasion) to 10 (complete persuasion). Suppose that in a given experiment, message A produces mean persuasion scores of 9.0 on attitude, 6.0 on intention, and 3.0 on behavior; message B produces mean persuasion scores of 8.0, 5.0, and 2.0, respectively. Each message is progressively less persuasive as one moves from attitudinal to intention to behavioral outcomes, but the *relative* persuasiveness of the two messages is the same no matter which outcome is examined.[3]

Hence, for example, the apparent equivalency of the ESs for response efficacy variations across the different persuasive outcomes in Witte and Allen's (2000) meta-analysis (mean *r*s of .18, .20, and .14, for attitude, intention, and behavior, respectively) does not mean that the three outcomes were equally affected by message exposure (does not mean that attitudes, intentions, and behaviors were equally influenced by the messages). Rather, it means that the size of the persuasive advantage enjoyed by the high-efficacy message over the low-efficacy message was the same (more carefully: statistically indistinguishable) across the three outcomes.

Thus the present results are not inconsistent with a belief that attitudes, intentions, and behaviors are differentially easy to influence. That belief has implications for expectations about how the persuasiveness of a single message might differ for different outcomes (e.g., an expectation that a given message will affect attitudes more than it will affect behaviors), but it does not underwrite expectations about whether the relative persuasiveness of two messages—the effect size—will differ for different outcomes.

Causal Sequence. There is a clear presumable causal sequence among attitudes, intentions, and behavior. As depicted in theoretical approaches such as the theory of planned behavior (TPB; Ajzen, 1991) and the theory of reasoned action (TRA; Fishbein & Ajzen, 1975, 2010), attitudes influence intention, and intentions influence behavior. These theoretical frameworks have appeared to receive extensive empirical confirmation, in the form of the expected positive correlations between these variables. (For a review of several relevant meta-analyses, see Hale, Householder, & Greene, 2002. For other relevant meta-analyses, see Albarracin, Johnson, Fishbein, & Muellerleile, 2001; Armitage & Conner, 2001; Cooke & French, 2008; Hagger, Chatzisarantis, & Biddle, 2002. For complexities, see Weinstein, 2007.) The correlations are far from perfect and can vary considerably depending on a number of moderating variables (e.g., Cooke & Sheeran, 2004; Glasman & Albarracin, 2006; Wallace, Paulson, Lord, & Bond, 2005), but the general pattern of relationship is that of positive correlations.

This causal chain implies that effects will progressively weaken as one moves from attitude to intention to behavior. If V1-V2-V3-V4 is a chain of

imperfectly-causally-related variables, variations in V1 will generally be manifest in relatively large effects on V2, smaller effects on V3, and still smaller effects on V4. That is, the relationship of an earlier variable in the chain to later variables weakens as one moves down the chain. If message-attitude-intention-behavior is such a chain, then the effect of a message variation on attitude should presumably be larger than its effect on intention, which in turn should be larger than the message variation's effect on behavior. Because the present results seem to indicate no such weakening of effect, these data seem inconsistent with the presumed causal chain—and so one might think there must be something amiss with the present results.

In fact, however, these data can be completely consistent with the presumed causal sequence. The reason is that the transmission of causal effect through the chain occurs for both messages in an experimental design. That is, message A would produce some given effect transmitted through the chain of outcome variables, and message B would similarly transmit its effect through the chain. But even if the *effect* of a given message weakens as it is transmitted down the chain, the *effect sizes* associated with each outcome variable (i.e., the differences between the effect of message A and the effect of message B) can be similar provided that the relationships between the outcome variables were identical for the two message conditions. Concretely: If attitudes are imperfectly correlated with intentions, and intentions are imperfectly correlated with behaviors, then the effect of message A on attitudes would be larger than its effect on behaviors by virtues of the imperfect causal relationships along the chain. But for the same reason, the effect of message B on attitudes would also be larger—and larger to the same degree—than its effect on behaviors. This process thus can produce identical differences between message A and message B in effectiveness at each point in the causal sequence (i.e., for each different outcome variable).

Gallagher and Updegraff's (2012) Results. Several readers have pointed to Gallagher and Updegraff's (2012) message framing meta-analytic results as a potential counterexample to the present conclusion. Gallagher and Updegraff reviewed published studies of prevention messages (messages urging actions to prevent disease or illness) and detection messages (e.g., messages concerning cancer screening), and they distinguished cases on the basis of whether attitudes, intentions, or behaviors were assessed. They found no significant framing effect (i.e., no significant differences between gain-framed and loss-framed appeals) for detection messages no matter which outcome was assessed. For prevention messages, however, they reported finding a statistically significant framing effect when behavioral outcomes were assessed but not when attitudinal or intention outcomes were assessed. This might be taken to be evidence that, contrary to the results reported above, distinguishing attitudinal, intention, and behavioral outcomes is important for assessing questions of relative persuasiveness.

But in fact, Gallagher and Updegraff's (2012) data are completely consistent

with the general conclusion offered earlier. As indicated in the re-analysis reported earlier, in Gallagher and Updegraff's data, there are no differences (in mean ESs) between attitudinal outcomes, intention outcomes, and behavioral outcomes, either for detection messages or for prevention messages.[4]

Gallagher and Updegraff's (2012) discussion of their results obscures this fact. For example, that discussion appears to contemplate various alternative possible explanations for why, among prevention messages, the mean ES for behavioral outcomes is larger than the mean ESs for attitudinal and intention outcomes (see p. 111)—but those mean ESs in fact are statistically indistinguishable. That is, the mean effect for prevention behaviors is not actually significantly larger than the mean effect for prevention attitudes or intentions, so there is no difference to explain. Gallagher and Updegraff's discussion of such explanations was thus inappropriate—and potentially quite confusing.

It is important not to be misled by Gallagher and Updegraff's (2012) finding of a statistically significant mean framing effect for prevention messages with behavior outcomes but non-significant mean framing effects for prevention messages with attitude or intention outcomes. The fact that one mean effect is significantly different from zero, while another mean effect is not significantly different from zero, does not show or imply that the two mean effects are significantly different from each other.[5] In fact, in this case, the mean effects for attitude, intention, and behavior are *not* significantly different from each other—replicating the general pattern observed earlier.

One might somehow have nagging doubts on this point. After all, in Gallagher and Updegraff's (2012) prevention-message dataset, there was a difference between outcomes in whether a statistically significant effect was observed: The mean ES for behavioral outcomes was statistically significant and those for attitudinal and intention outcomes were not. So, one might think that somehow this shows some consequential difference between outcomes, despite the lack of any statistically significant difference between the mean ESs.

To quell such doubts, consider the 95% confidence intervals for the mean ESs in question (see Table 8.1). The 95% CI for behavioral outcomes excludes zero (hence that effect is statistically significant); the 95% CIs for attitudinal and intention outcomes do not exclude zero (those mean ESs were not statistically significantly different from zero). However, those three 95% CIs overlap such that a common population effect could underlie all three. For example, a population effect of .04 falls within the 95% CI for each of the three outcomes. So even though (on the basis of the sample data in hand) one outcome's population effect can confidently be said to be nonzero and the other two cannot, it is nevertheless possible that all three have an identical population effect.[6]

In short, properly analyzed and interpreted, Gallagher and Updegraff's (2012) data concerning gain-loss framing effects in prevention messages display a pattern exactly like those of other meta-analytic reviews of message-variation persuasion effect sizes: Attitudinal, intention, and behavioral outcomes yield functionally equivalent assessments of the relative persuasiveness of message types.

Summary. The present results are not inconsistent with a belief that attitudes, intentions, and behaviors are differentially easy to influence; they are not inconsistent with a belief that attitudes, intentions, and behaviors form a causal sequence; and they are not inconsistent with Gallagher and Updegraff's (2012) meta-analytic results.

Artifactual Results?

One might worry that the similarity of the behavioral-outcome ESs to the attitude-outcome and intention-outcome ESs has been artificially inflated by the use of self-report data for behavioral assessments. Self-report behavioral data might be subject to processes that incline respondents to offer reports more consistent with their attitudes and intentions than would be revealed by direct behavioral observation. Thus it might be suspected that the use of self-report behavioral data artificially influences the observed results.

This suggested artifact cannot account for all of the observed consistency, of course, because it cannot explain the consistency between attitude-outcome ESs and intention-outcome ESs. At most, it suggests that the consistency of behavior-outcome ESs with those based on the other two outcomes might be artificial.

But, more fundamentally, this concern is based on a misunderstanding. The present results do not address the consistency of attitudinal measures and behavioral measures (consistency that indeed might be artificially increased by the use of self-report behavioral measures) or the consistency of intention measures and behavioral measures (which also might be increased by the use of self-report behavioral data). The present results concern the consistency of message-variation effect sizes across attitudinal, intention, and behavioral measures—and that sort of consistency is not straightforwardly affected by the use of self-report behavioral data.

To concretize this matter: Imagine an experimental message-variable persuasion study that has both self-reported behavioral data and direct-observation behavioral data. Suppose that participants exaggerate the consistency of their behaviors with their attitudes. If participants in the two message conditions exaggerate equally, then (*ceteris paribus*) the effect size—the comparison between message A and message B—will be similar for self-reported behavioral data and for direct-observation behavioral data. The absolute values of the two behavioral indices will be different (such that the self-reported data will make people look more consistent with their attitudes than the direct-observation data do), but if all the participants are distorting their self-reported behavioral data in the same way, then the difference between the two message conditions will be similar for the two kinds of behavioral data.

So even if participants' behavioral self-reports exaggerate the consistency of their behaviors with their attitudes and intentions, such distortion could not explain the consistency of effect sizes (differences between message conditions) across these three outcomes.

The Anomalous Result

For the message variable of threat vulnerability, one meta-analysis, but not a second, yielded anomalous effects in both the overall analysis and the follow-up (within-studies) analyses. In de Hoog et al.'s (2007) review, ESs were significantly smaller with attitudinal outcomes than with either intention or behavioral outcomes (with these latter two not differing significantly from each other). However, this pattern of results did not appear in Witte and Allen's (2000) meta-analysis of threat vulnerability effects.

The locus of the difference between these two meta-analytic results is the mean ES concerning attitudinal outcomes. Similar mean effects were obtained by the two reviews for intention outcomes (Witte and Allen's was .167, de Hoog et al.'s was .162) and for behavioral outcomes (Witte and Allen's was .132, de Hoog et al.'s was .188). But quite different mean effects appeared for attitudinal outcomes: Witte and Allen (2000) reported a mean r of .104, whereas de Hoog et al. (2007) had a mean r of –.041.

The mystery deepens if one compares the attitude-outcome studies included in the two meta-analyses. Some differences are to be expected by virtue of the later review's being able to include studies appearing subsequent to the earlier review. But of Witte and Allen's (2000) 11 attitude-outcome cases, de Hoog et al. (2007) included only one (Dziokonski & Weber, 1977). And of de Hoog et al.'s eight attitude-outcome cases with a publication date of 1999 or earlier, only one—Dziokonski and Weber (1977)—was included in Witte and Allen's (2000) analysis.

One hypothesis might be that differing inclusion criteria gave rise to the divergent results. Witte and Allen's (2000) review included both published and unpublished studies, whereas de Hoog et al.'s (2007) review was limited to published studies. Given that published and unpublished studies may differ in some ways (e.g., because of the familiar bias toward publication of statistically significant effects: Gerber & Malhotra, 2008a, 2008b; Levine, Asada, & Carpenter, 2009), one might suspect that some publication-bias-related mechanism could be at work.

However, for other message variables, such differences in inclusion criteria did not produce divergent results. For example, O'Keefe and Jensen's (2009) review of gain-loss message framing variations in disease detection messages included both published and unpublished studies, whereas Gallagher and Updegraff's (2012) review of that same research area was restricted to published studies—but neither dataset contained any significant differences between mean ESs based on different outcomes. As another example, Witte and Allen's (2000) review of fear appeal strength variations included both published and unpublished studies, whereas Sutton's (1982) review included only published studies—but neither dataset had any significant differences between mean ESs for different outcomes. Indeed, with respect to threat severity variations, both de Hoog et al.'s (2007) review and Witte and Allen's (2000) review yielded no significant differences between mean ESs based on different

outcomes, despite the difference in inclusion criteria. In short, the difference in whether unpublished studies were included does not seem like a plausible account of the observed divergence.

This is not the place to attempt to sort out the details of how and why these two meta-analyses came to yield such different results for the effects of variations in depicted threat vulnerability on attitudes.[7] For present purposes, the appropriate conclusion would seem to be that it is not entirely clear whether variations in depicted threat vulnerability produce ESs for attitudinal outcomes that are different from the ESs produced for intention and behavioral outcomes. If such differences are indeed genuine, they would have considerable interest, precisely because such differences would represent a singular departure from the general pattern of effects for other message variables. But the evidence in hand is unhappily ambiguous on this score.

Limitations and Cautions

Limitations. As with any secondary data analysis, the present report's conclusions are circumscribed by the available research literature. One cannot know how the results might have been different if, for example, additional meta-analytic databases had been available or if additional meta-analytic reviews had been performed. And some of the meta-analytic comparisons were based on relatively small numbers of studies; for example, O'Keefe's (2002) dataset for the effects of variations in recommendation specificity had only 12 ESs. But other datasets were substantially larger (e.g., the 225 ESs for gain-loss framing variations in the dataset of O'Keefe & Jensen, 2006). And, as noted earlier, the datasets in hand provide a strikingly consistent picture—consistent across different kinds of message variables and across different meta-analytic procedures (different inclusion criteria, different ways of computing effect sizes, and so on).

Cautions. Some readers have been alarmed by the apparent implications of the present results. In particular, concerns have been raised that these results might be taken to underwrite avoidance of collection of behavioral outcome data in persuasion research (and collection of such data is taken to be an unquestionable good).

Such concerns are partly, but not entirely, misplaced. They are partly misplaced in the following way: Given the present results, collecting behavioral outcome assessments is indeed unnecessary for answering research questions concerning *specifically* the relative persuasiveness of message types. If a message designer wants to know whether message A or message B will be more persuasive in influencing behavioral outcomes and so conducts a randomized trial as part of pre-campaign message testing, the designer does not need to collect behavioral outcome data. The question of relative persuasiveness can be confidently answered by collecting attitude or intention assessments.

But such concerns are not entirely misplaced, because behavioral outcome assessments can be useful—indeed, crucial—for answering *other* questions. Two general kinds of such questions can be identified.

First, questions about the persuasiveness of a given message in influencing behavior will require behavioral assessments. The present results indicate that a campaign planner can learn that message A is more persuasive than message B with respect to behavioral outcomes without having to assess behavioral outcomes themselves. But a campaign planner might well want to know just how large the absolute effect of message A will be on behaviors—and for answering that question, assessment of behavioral outcomes is essential.

Second, theoretical questions concerning the relationships—and especially the causal relationships—of message variations, attitudes, intentions, and behaviors will require behavioral assessments. As noted above, attitude, intentions, and behaviors are commonly taken to form a causal sequence, as in theoretical perspectives such as TRA and TPB. The evidence that is often adduced to support such theories is based on cross-sectional correlational analyses (e.g., indicating that attitude and intention are strongly correlated at a given point in time). However, as Weinstein (2007) has pointed out, such analyses can produce misleading tests of the causal claims embedded in such theories. Better evidence would be provided by experimental studies of the longitudinal effects of interventions (such as alternative persuasive messages) on all three persuasive outcomes. For example, appropriate longitudinal data would permit cross-lagged correlations (to help clarify the causal relationship of two variables that are positively correlated in cross-sectional data) and would provide information about both relative and absolute amounts of persuasion at different points in time with different outcomes. Hence even though, as indicated earlier, a researcher might need to assess only one of the three outcomes in order to answer specific questions about the relative persuasiveness of two message forms, assessing all three outcomes—and assessing them longitudinally—will provide richer information.

Conclusion

These results underwrite a general presumption that the relative persuasiveness of message types will be substantively identical if compared using attitudinal, intention, or behavioral outcomes. Where research questions are specifically focused on the relative persuasiveness of alternative message types, these three outcome variables are functionally equivalent, in the sense of giving the same answer to that research question. If message type A is more persuasive than message type B with attitudinal outcomes, it is also—and equally—more persuasive with intention and behavioral outcomes.

Notes

1. The dataset in Gallagher and Updegraff's Table 1 had two errors (K. M. Gallagher, personal communication, April 4, 2012), which were corrected for this

analysis. The two effect sizes for Block and Keller's (1995) Study 2 high-efficacy condition were recoded as prevention (not detection) cases, and the effect size for Rothman, Salovey, Antone, Keough, and Martin's (1993) Study 2 was entered as .09 (not .28).

2. Readers with concerns about this procedure can put them aside in the present context. As with the other meta-analytic datasets re-analyzed here, Eisend's (2006, 2007) dataset was accepted at face value.
3. These imaginary numbers are potentially misleading. For each outcome, the raw difference between the two means is the same (1.0), but whether the ES for each outcome is the same depends on the various standard deviations. If one adds the simplifying assumption that the standard deviation is the same for each outcome, then the three ESs would be identical.
4. The mean ESs reported here differ slightly from those in Gallagher and Updegraff's (2012) Table 2, in part because of the two corrections described in footnote 1, and in part because the ESs in Gallagher and Updegraff's (2012) Table 1—the ones re-analyzed here—may have been rounded for reporting purposes.
5. As a parallel illustrative case, imagine a study that found that variables X and Y were statistically significantly correlated in male participants ($r = .198$, $N = 100$; $p < .05$, two-tailed), but were not statistically significantly correlated in female participants ($r = .196$, $N = 100$; *ns*). This would not mean that the correlations for men and women were statistically significantly different. (In fact, these two correlations are not significantly different.)
6. To put this more abstractly: If, in a persuasion meta-analysis that (contrary to the present recommendation) reports a separate mean ES for each outcome, the results are such that the various mean ESs are not significantly different from each other even though some—but not all—of them are significantly different from zero, then such results should be interpreted by remembering that (a) the research goal is presumably estimation of the population effect, (b) the CI associated with each mean ES specifies the range of plausible population effects, and (c) the overlap of the CIs can suggest a possible common population effect.
7. One notices that each meta-analytic result is especially congenial with the theoretical framework associated with its authors. The extended parallel process model (EPPM; Witte, 1992, 1994) provides a basis for expecting that variations in depicted threat vulnerability will influence attitudes (e.g., Witte & Allen, 2000, p. 603). The stage model of fear appeal messages (de Hoog, Stroebe, & de Wit, 2005) makes many of the same predictions as does the EPPM, but—quite distinctively—hypothesizes that "attitudes toward a protective action … should be unaffected by feelings of vulnerability" (de Hoog et al., 2007, p. 264). Perhaps a disinterested review would be helpful.

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