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The Relative Persuasiveness of Gain-Framed and Loss-Framed Messages for Encouraging Disease Detection Behaviors: A Meta-Analytic Review

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A meta-analytic review of 53 studies (N = 9,145) finds that in messages aimed at encouraging disease detection behaviors, loss-framed appeals (which emphasize the disadvantages of noncompliance with the communicator's recommendation) are only slightly, but statistically significantly, more persuasive than gain-framed appeals (which emphasize the advantages of compliance); the difference corresponds to a correlation of −.04. Loss-framed appeals enjoy a small statistically significant advantage for messages advocating breast cancer detection behaviors, but not for any other kind of detection behavior (detection of skin cancer, other cancers, dental problems, or miscellaneous other diseases) nor for all other kinds of detection behaviors combined. Thus, in advocacy of disease detection behaviors, using loss-framed rather than gain-framed appeals is unlikely to substantially improve persuasiveness.

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Early detection of disease conditions can potentially have substantial benefits, such as reduced risk of death and availability of a larger number of (and less aggressive) treatment options (Smith, Cokkinides, & Eyre, 2007). The chances for early detection are enhanced by performance of appropriate screening behaviors—and hence one persuasive task facing health professionals is that of encouraging such behaviors. Even when mass screenings may not be justified by the benefits (e.g., Helfand, Mahon, Eden, Frame, & Orleans, 2001; U.S. Preventive Services Task Force, 2001), screenings targeted to specific populations may still be beneficial (e.g., Swetter, Waddell, Vazquez, & Khosravi, 2003).

Among the kinds of persuasive messages that might be used to encourage disease detection behaviors, loss-framed appeals have often been suggested to be especially

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effective. A loss-framed persuasive appeal emphasizes the disadvantages of failing to comply with the communicator's recommendation (e.g., "If you don't have regular mammograms, you reduce your chances of detecting breast cancer at an early, more treatable stage"); the contrast is a gain-framed appeal, which emphasizes the advantages of compliance (e.g., "If you have regular mammograms, you increase your chances of detecting breast cancer at an early, more treatable stage"). Previous primary research (e.g., Meyerowitz & Chaiken, 1987) and reviews and theoretical analyses (e.g., Edwards, Elwyn, Covey, Matthews, & Pill, 2001; Kuhberger, 1998; Rothman & Salovey, 1997; Salovey, Schneider, & Apanovitch, 2002; Wilson, Purdon, & Wallston, 1988) have suggested that loss-framed appeals will enjoy a significant persuasive advantage over gain-framed appeals in the domain of disease detection behaviors. The most common explanatory framework invokes prospect-theoretic reasoning to suggest that potential losses are more motivating than potential gains when risky actions are contemplated, whereas gains are more motivating than losses for low-risk behaviors (Kahneman & Tversky, 1979). The apparent implication is that for relatively high-risk actions such as disease detection behaviors, loss-framed messages should be more persuasive than gain-framed messages (see, e.g., Salovey et al., 2002; cf. O'Keefe & Jensen, 2006, p. 23).

However, a recent broad meta-analytic review concluded that loss-framed appeals are not significantly more persuasive than gain-framed appeals for encouraging disease detection behaviors (O'Keefe & Jensen, 2006). The observed direction of effect favored loss-framed appeals, but the mean effect size (across 34 studies) was $r = -.03$, a value not significantly different from zero.

Unfortunately, that review treated disease detection behaviors as a global category and did not distinguish effects for different specific detection behaviors; indeed, to date, no systematic review appears to have done so. Thus, the primary purpose of this review was to examine whether the relative persuasiveness of gain- and loss-framed appeals varies depending on the particular detection behavior being advocated. These behavior-specific analyses were motivated by two considerations. First, such analyses have considerable practical importance; the best evidence of whether loss-framed appeals are especially persuasive for encouraging (say) skin cancer detection behaviors comes from studies specifically concerning those behaviors, not from general research on disease detection behaviors. Second, a global nonsignificant effect might mask a substantial effect occurring for some particular detection behavior; without behavior-specific analyses, such effects could go unnoticed.

The secondary purpose was to explore potential moderating factors. A number of variables have been suggested as possible moderators of gain–loss message framing effects, although many candidates are unsuitable for meta-analytic examination. For example, it is not feasible to code studies for characteristics such as audience mood (e.g., Martin & Lawson, 1998; Mitchell, 2001), attitudinal ambivalence (e.g., Broemer, 2002; Matwin, 2007), self-efficacy (e.g., van 't Riet, Ruiter, Werrij, & de Vries, in press), and the like. However, one particular potential moderator merited attention, namely, a variation in the way in which the consequences (of adopting or not adopting the recommended behavior) were expressed.

As noticed by several commentators (e.g., Dillard & Marshall, 2003; Nan, 2007b; Rothman & Salovey, 1997; Wilson et al., 1988), gain-framed and loss-framed appeals can each take two forms, with the resulting four possibilities represented in a 2 × 2 array in which the contrasts are whether (a) the outcome described is a desirable or an undesirable one and (b) the outcome is described as one that is attained (acquired, made more likely) or avoided (averted, made less likely). That is, a gain-framed appeal might take the form "If you perform the advocated behavior, desirable outcome X will be obtained" or the form "If you perform the advocated behavior, undesirable outcome Y will be avoided." A loss-framed appeal might take the form "If you do not perform the advocated behavior, desirable outcome X will be avoided" or the form "If you do not perform the advocated behavior, undesirable outcome Y will be obtained."

This variation has previously been described as a matter of the appeals differing in the linguistic representation of the "kernel state" of the consequence under discussion (O'Keefe & Jensen, 2006). The kernel state is the basic, root state mentioned in the message's description of the consequence. For example, in an appeal such as "If you have regular skin exams, you will reduce your risk of skin cancer," the kernel state is "skin cancer," which is plainly an undesirable state; that is, this appeal emphasizes the desirable consequences of compliance by discussing an undesirable kernel state ("skin cancer") that will be avoided. By comparison, "If you have regular skin exams, you will increase your chances of having healthy skin" is an appeal describing a desirable kernel state ("healthy skin") that will be attained by compliance. A gain-framed appeal thus could be phrased entirely in terms of avoided undesirable outcomes ("heart disease," "skin cancer," "premature death," etc.) and a loss-framed appeal could be phrased entirely in terms of foregone desirable outcomes ("healthy heart," "attractive skin," "long life," etc.)—and hence to sort out these possible evaluative confoundings, examination of this moderator seemed warranted.

Method

Identification of relevant investigations

Literature search

Relevant research reports were located through personal knowledge of the literature, examination of previous reviews, and inspection of reference lists in previously located reports. In addition, articles were identified through computerized database searches through at least August 2008 of ABI-INFORM, Cumulative Index of Nursing and Allied Health Literature (CINAHL), Current Contents, Dissertation Abstracts, EBSCO, Educational Resources Information Center (ERIC), Linguistics and Language Behavior Abstracts, MEDLINE, and PsycINFO, using various appropriate combinations of terms such as framing, framed, frame, appeal, message, persuasion, persuasive, gain, positive, positively, benefit, loss, negative, negatively, threat, and valence.

Inclusion criteria

We included a study if it met three criteria. First, the study had to compare gain- and loss-framed persuasive messages. A gain-framed message emphasizes the desirable consequences of compliance with the advocated view; a loss-framed message emphasizes the undesirable consequences of noncompliance. Excluded by this criterion were studies with imperfect realizations of this message contrast, such as studies that compared one framing form with unframed information (Abood, Coster, Mullis, & Black, 2002; Kalichman & Coley, 1995). Second, the messages had to advocate disease detection behaviors (breast cancer detection behaviors such as mammography and breast self-examination [BSE], skin cancer detection behaviors such as skin examinations, etc.). We excluded studies in which the messages advocated other behaviors, such as disease prevention behaviors (e.g., Block, 1993, sun exposure condition) or behaviors unrelated to health (e.g., Thorsteinson & Highhouse, 2003). As noted by Salovey and Wegener (2003, p. 61), some health-related behaviors, such as Pap tests and colonoscopies, might plausibly be described as either (or both) a disease-detection behavior or a disease-prevention behavior; in the interest of focusing on clear-cut studies of disease detection behaviors, studies of such "dual-function" behaviors were excluded (Miles, Brotherstone, Robb, Atkin, & Wardle, 2005; Mullins, 2005; Rivers, Salovey, Pizarro, Pizzaro, & Schneider, 2005; Wilkin, 2004). Following similar reasoning, we excluded studies in which a message advocated both detection behaviors and prevention behaviors (Block & Keller, 1995, Study 1) and studies in which the disease condition to be detected was not the audience's (Nan, 2007a, other target condition; Nan, 2007c; Roberto, Goodall, West, & Mahan, in press).

Third, appropriate quantitative data relevant to persuasive effects (e.g., attitude change, intention, or behavior) had to be available; where it was not provided in the report, we made efforts to obtain information from authors. Excluded by this criterion were studies of effects on other outcome variables and studies for which appropriate quantitative information could not be obtained (e.g., Brunton, 2007; Miller et al., 1999; Salmon, Loken, & Finnegan, 1985; Steffen, Sternberg, Teegarden, & Shepherd, 1994; Trotto, 2001; Umphrey, 2001).

Outcome variable and effect size measure

Outcome variable

The outcome variable was persuasion, as assessed through attitude change, postcommunication agreement, behavioral intention, behavior, and the like. When multiple indices of persuasion (e.g., assessments of attitude and of intention) were available, we averaged the effects to yield a single summary.[1] Most studies reported only short-term effects; where both long- and short-term effect size information was available (e.g., Banks et al., 1995; Finney, 2001; Lalor, 1990), only short-term effects were included to maximize comparability across studies.

Effect size measure

Every comparison between a gain-framed message and its loss-framed counterpart was summarized using *r* as the effect size measure; differences indicating greater persuasion with gain-framed messages were given a positive sign. Results not reported as correlations were converted to *r* using formulas provided by Johnson (1993) and Rosenthal (1991). When correlations were averaged (e.g., across several indices of persuasive effect), we computed the average using the *r*-to-*z*-to-*r* transformation procedure, weighted by *n*. Wherever possible, multiple-factor designs were analyzed by reconstituting the analysis such that individual-difference factors (but not, e.g., other experimental manipulations) were put back into the error term (following the suggestion of Johnson, 1989, p. 16).

Moderator variables

Specific detection behavior

Cases were classified by the kind of detection behavior advocated, with five broad categories distinguished: Breast cancer detection (e.g., mammography), skin cancer detection (e.g., skin self-examination), detection of other cancers (e.g., colon cancer screenings), detection of dental problems (e.g., plaque detection), and miscellaneous detection behaviors (screenings for cholesterol, hypertension, etc.).

Kernel-state phrasing

The kernel states in each appeal were identified; as discussed above, a kernel state is the basic, root state mentioned in the message's description of the consequence under discussion. We coded each appeal as containing exclusively desirable kernel states (e.g., "healthy heart," "attractive skin"), exclusively undesirable kernel states (e.g., "heart disease," "skin cancer"), a combination of desirable and undesirable kernel states, or as indeterminate with respect to kernel-state phrasing (as when insufficient detail was available about the messages).

Coding reliabilities

Codings for these moderator variables were completed independently by the authors for a sample of 30 cases. Intercoder reliabilities (percent agreement and Cohen's kappa, respectively) were .97 and .95 for advocated behavior, .93 and .89 for kernel state phrasing in gain appeals, and .97 and .95 for kernel state phrasing in loss appeals. Discrepancies were resolved by discussion. D. J. O'K. coded the remaining cases.

Unit of analysis

The unit of analysis was the message pair, that is, the pair composed of a gain-framed message and its loss-framed counterpart. We recorded an effect size for each distinguishable message pair found in the body of studies. Most message pairs were used only in a single investigation, so only one effect size estimate was associated with the pair. However in three instances, message pairs were used in more than one study, with the result that several effect size estimates could be associated with a given message pair. These multiple estimates were averaged to yield a single summary estimate before inclusion in the analysis. Specifically, data from Experiment 1 and Experiment 2 in Keller, Lipkus, and Rimer (2003) were combined and reported as Keller et al. (2003); data from Meyerowitz and Chaiken (1987) and Lalor (1990) were combined and reported as Meyerowitz and Chaiken (1987) combined; and data from Rothman, Martino, Bedell, Detweiler, and Salovey (1999). Experiment 1 detection condition and Sanchez (2006) were combined and reported as Rothman letrolisus detection combined.

Whenever a study included more than one message pair and reported data separately for each pair, each pair was treated as providing a separate effect size estimate (e.g., Ying, 2001). Some studies included more than one message pair but did not report results in ways that permitted computing separate effect sizes for each pair (Apanovitch, McCarthy, & Salovey, 2003; Schmitt, 2004; Williams, Clarke, & Borland, 2001); we computed a single effect size in such cases, with the consequence that this analysis underrepresents the amount of message-to-message effect variability in these data.

In some cases, the same primary data served as the basis for multiple reports (e.g., both a dissertation and a subsequent publication). When a given investigation was reported in more than one outlet, it was treated as a single study and analyzed accordingly. The same research was reported (in whole or in part) in: Block (1993) and Block and Keller (1995, Study 2), recorded under the former; Brenes (1998) and Brenes (1999), recorded under the latter; Cothran, Schneider, and Salovey (1998) and Schneider et al. (2001), recorded under the latter; Finney (2001) and Finney and Iannotti (2002), recorded under the former; Lalor (1990) and Lalor and Hailey (1990), with, as noted above, results recorded under Meyerowitz and Chaiken (1987) combined; Nan (2006, Experiment 2) and Nan (2007b, Experiment 2), recorded under the former; Shen (2005, Study 2, glaucoma exam), and Shen and Dillard (2007, Study 2, glaucoma exam), recorded under the former.

Meta-analytic procedures

The individual correlations (effect sizes) were initially transformed to Fisher's *z*s; the *z*s were analyzed using random-effects procedures (specifically, those of Borenstein & Rothstein, 2005; see also Hedges & Vevea, 1998; Shadish & Haddock, 1994), with results then transformed back to *r*. A random-effects analysis was used in preference to a fixed-effects analysis because of an interest in generalizing across messages (for some discussion, see Hedges & Vevea, 1998; Raudenbush, 1994; Shadish & Haddock, 1994).

Results

Overall effect

Effect sizes were available for 53 cases, with a total of 9,145 participants. Details for each included case are contained in Table 1. Across all 53 cases, loss-framed appeals were significantly more persuasive than gain-framed appeals; mean $r = -.039$ ($p = .02$).[2]

Table 1 Cases Analyzed

Study	*r*	N	Codings[a]
Apanovitch et al. (2003)	.064	425	5/3/3/100
Arora (2000)	−.157	210	4/1/2/60
Banks et al. (1995)	−.011	133	1/3/1/100
Block (1993) self-exam	−.222	57	2/2/1/na
Brenes (1999)	−.037	142	1/4/4/100
Broemer (2004) Study 2	.167	60	1/3/3/100
Chang (2007) Study 1 disclosing gum	−.250	66	4/4/4/na
Chang (2007) Study 1 disclosing strips	.443	66	4/4/4/na
Cherubini et al. (2005) fear-evoking	−.164	60	3/3/3/0
Cherubini et al. (2005) reassuring	−.166	60	3/3/3/0
Consedine, Horton, Magai, and Kukafka (2007)	−.024	89	1/3/3/100
Cox, Cox, and Zimet (2006) Study 1 detection	−.078	142	2/3/3/41
Cox and Cox (2001) anecdotal	−.306	55	1/3/3/100
Cox and Cox (2001) statistical	.046	55	1/2/1/100
Finney (2001)	−.044	628	1/2/1/100
Fischer and Nabi (2001) skin exam	.144	87	2/3/1/54
Gintner, Rectanus, Achord, and Parker (1987)	.051	177	5/4/4/59
Hsiao (2002) testing-prevention	−.300	46	5/3/3/56
Hsiao (2002) testing-detection	.308	46	5/3/3/56
Keller et al. (2003)	−.024	162	1/3/3/100
Kostygina (2007)	−.066	119	3/3/3/na
Kwan (1996)	−.476	33	1/3/3/100
Kyriakaki (2007) diabetes health	−.182	63	5/3/3/na
Kyriakaki (2007) diabetes psychosocial	−.193	65	5/3/3/na
Lauver and Rubin (1990)	−.060	116	3/1/2/100
Lee, Brown, & Blood (2000) self-exam	−.106	137	2/1/2/54
Lerman et al. (1992)	−.011	203	1/4/4/100
Lotto, Tasso, Carnaghi, and Rumati (2006)	−.018	80	1/4/4/100
Maheswaran and Meyers-Levy (1990)	.023	98	5/3/3/na
Matwin (2007) strong	−.039	162	5/3/3/na
Matwin (2007) weak	−.018	160	5/3/1/na
Meyerowitz and Chaiken (1987) combined	−.219	91	1/3/3/100
Myers et al. (1991)	−.035	2,201	3/4/4/47
Nan (2006) Experiment 2 gain-positive loss-positive	.124	68	5/1/2/na
Nan (2006) Experiment 2 gain-positive loss-negative	.076	67	5/1/1/na
Nan (2006) Experiment 2 gain-negative loss-positive	.070	68	5/2/2/na
Nan (2006) Experiment 2 gain-negative loss-negative	.161	67	5/2/1/na
Nan (2007a) self-target	.105	64	5/3/3/100
Phelan (2003)	.000	60	1/4/4/100
Rothman letrolisus detection combined	−.218	321	5/3/3/87

(*continued overleaf*)

Table 1 (*Continued*)

Study	*r*	N	Codings[a]
Rothman et al. (1999) Experiment 2 detection	−.305	60	4/2/1/74
Ruiter, Kok, Verplanken, and van Eersel (2003)	−.099	110	1/4/4/100
Schmitt (2004)	−.055	150	5/4/4/100
Schneider et al. (2001) multicultural	−.138	264	1/4/4/100
Schneider et al. (2001) targeted	.047	264	1/4/4/100
Shen (2005) Study 2 glaucoma exam	.021	252	5/4/4/71
Spaderna (2003)	−.019	162	2/3/3/60
Umphrey (2003)	.085	128	3/3/3/0
Urban, Stout, Zimet, and Blake (2006)	.083	35	5/3/3/100
van 't Riet, Ruiter, Werrij, and de Vries (in press)	−.141	124	2/3/1/86
Williams, Clarke, and Borland (2001)	−.089	307	1/4/4/100
Ying (2001) concrete	−.021	140	2/3/3/na
Ying (2001) abstract	.069	140	2/3/3/na

Note: [a]The codings are the advocacy topic (1 = breast cancer, 2 = skin cancer, 3 = other cancer, 4 = dental problems, 5 = miscellaneous); gain kernel-state language (1 = desirable states, 2 = undesirable states, 3 = both desirable and undesirable states, 4 = indeterminate); loss kernel-state language (1 = undesirable states, 2 = desirable states, 3 = both desirable and undesirable states, 4 = indeterminate); and the percentage of female participants (na = not available).

Specific detection behaviors

As Table 2 indicates, gain- and loss-framed appeals did not significantly differ in persuasiveness for most kinds of detection behavior. A dependable advantage for loss-framed appeals was apparent for breast cancer detection behaviors (mean $r = -.056$), but not for any other category of detection behaviors.[3] For the 36 cases not focused on breast cancer detection behaviors, gain- and loss-framed appeals were not significantly different: Mean $r = -.029$, 95% confidence interval (CI) limits of $-.072$ and $.013$ ($p = .18$), power $> .99$; $Q(35) = 76.2$, $p < .001$.[4]

Kernel state phrasing

Relatively few studies used messages containing exclusively desirable or undesirable kernel states, as indicated in Table 2. In those studies, gain- and loss-framed appeals did not significantly differ in persuasiveness either when the gain-framed appeal had exclusively desirable kernel states (mean $r = -.057$) or when it had exclusively undesirable kernel states (mean $r = -.046$); these two mean effects were not significantly different ($p = .89$). Gain- and loss-framed appeals also did not differ significantly in persuasiveness either when the loss-framed appeal had exclusively desirable kernel states (mean $r = -.058$) or when it had exclusively undesirable kernel states (mean $r = -.031$); these two mean effects were not significantly different ($p = .67$).

Table 2 Summary of Results

	k	N	Mean r	95% CI	Power[a]	Q (df)
All cases	53	9,145	−.039	−.072, −.007	—	99.2(52)*
Specific detection behaviors						
Breast cancer	17	2,736	−.056	−.104, −.009	—	22.2(16)
Skin cancer	8	989	−.041	−.110, .029	.59	8.5(7)
Other cancer	6	2,684	−.037	−.075, .001	.95	3.9(5)
Dental	4	402	−.065	−.363, .246	.29	25.7(3)*
Miscellaneous	18	2,334	−.005	−.069, .059	.92	35.5(17)**
Gain message kernel language						
Desirable only	5	598	−.057	−.155, .042	.40	5.6(4)
Undesirable only	6	935	−.046	−.166, .074	.57	10.0(5)
Both	30	3,402	−.042	−.099, .016	.98	74.7(29)*
Indeterminate	12	4,210	−.034	−.064, −.004	—	8.2(11)
Loss message kernel language						
Desirable only	5	599	−.058	−.155, .040	.40	5.5(4)
Undesirable only	10	1,438	−.031	−.106, .044	.76	14.3(9)
Both	26	2,898	−.048	−.113, .018	.96	70.4(25)*
Indeterminate	12	4,210	−.034	−.064, −.004	—	8.2(11)

[a]These are power figures for detecting a population effect size of $r = .10$, assuming large heterogeneity, with a random-effects analysis, .05 alpha, and a two-tailed test (Hedges & Pigott, 2001).
$^{*}p < .001$. $^{**}p < .01$.

Discussion

Overall effect

O'Keefe and Jensen's (2006) review concluded that gain- and loss-framed appeals do not significantly differ in persuasiveness concerning disease detection behaviors. Because our search included studies appearing after that review, the current review had a larger number of cases (53) than did that earlier review (34). The addition of these new cases has revealed a statistically significant advantage for loss-framed appeals—but that advantage is quite small (a mean r of −.039). So on one hand, there is good evidence for some nonzero population effect, but on the other hand these results indicate that loss-framed appeals do not have some substantial advantage for encouraging disease detection behaviors. Indeed, the 95% CI around the mean effect indicates that a population value of −.08 is implausible.

This must be counted a rather disappointing conclusion. Given the benefits of early recognition of disease conditions, identifying effective means of encouraging disease detection behaviors is an important task, and loss-framed appeals have appeared to offer a promising tool in this regard. But sufficient research evidence has accumulated to make it plain that the use of loss-framed rather than gain-framed appeals will generally not make much difference to persuasive success in this enterprise. Those who have the responsibility of designing messages aimed at encouraging disease detection behaviors should not believe that choosing loss-framed appeals will substantially improve persuasiveness.

Kernel-state phrasing

These data give no reason to suppose that the relative persuasiveness of gain- and loss-framed appeals in this domain is systematically related to whether the appeals mention desirable or undesirable outcomes (as the outcomes that are obtained or foregone). No matter whether the appeals were expressed in terms of desirable or undesirable outcomes, no significant difference emerged between gain- and loss-framed appeals. There is not as much evidence as one might like on this matter, but the research in hand gives no hint that variations in kernel-state phrasing will produce systematic differences in the relative persuasiveness of gain-framed and loss-framed appeals for disease detection behaviors.

Specific detection behaviors

Loss-framed appeals were dependably more persuasive than their gain-framed counterparts concerning breast cancer detection behaviors.[5] This straightforwardly implies that those designing breast cancer detection messages should favor loss-framed appeals. However, even though statistically significant, the advantage for loss-framed appeals was relatively small ($r = -.056$); the 95% CI around the effect suggests that a population value as large as $r = -.11$ is implausible. So although these results recommend the use of loss-framed appeals for breast cancer detection messages, such appeals provide only a small persuasive advantage.[6]

With respect to the advocacy of other specific disease detection behaviors, however, the evidence in hand is insufficient to recommend the use of loss-framed appeals. For no other kind of detection behavior (other than breast cancer detection) was a statistically significant difference observed. And not even across studies of all other detection behaviors combined (i.e., excluding studies of breast cancer detection) was there a significant difference, despite considerable statistical power.

The challenge of generalization

The present results may offer a useful object lesson concerning generalizations in communication research (or, indeed, social-scientific research generally). Given any individual study, it is always an open question just what generalizations it underwrites. Two aspects of generalization can be usefully distinguished: The scope of the effect (the cases to which it applies) and the size of the effect.

The scope of the effect

One of the earliest investigations of gain–loss message framing effects was Meyerowitz and Chaiken's (1987) study of messages urging BSE, which reported a significant persuasive advantage for loss-framed appeals. This specific result might be used in the service of any number of different generalizations varying in scope.

Most broadly, Meyerowitz and Chaiken's (1987) research has sometimes been interpreted as suggesting that loss-framed appeals are more effective than gain-framed appeals in general: "Typically, loss frames are more persuasive than gain-frame messages (Meyerowitz & Chaiken, 1987)" (Johnson, Maio, & Smith-McLallen, 2005,

p. 640). But several reviews of gain-loss framing effects have concluded that there is no such general effect (e.g., Rothman & Salovey, 1997).

A narrower scope is represented by the suggestion that loss-framed appeals are more effective than gain-framed appeals for disease detection behaviors (e.g., Salovey et al., 2002). But the present results do not unambiguously underwrite such a conclusion. There is indeed a statistically significant overall effect favoring loss-framed appeals—but that effect is not statistically significant for any specific kind of detection behavior other than breast cancer detection. Nor is the effect statistically significant across all other detection behaviors combined (i.e., excluding breast cancer detection behaviors), despite excellent statistical power. Thus, the overall significant effect might properly be described as carried by the effects obtained in breast-cancer detection studies specifically.

So, more narrowly still, Meyerowitz and Chaiken's (1987) findings might be interpreted as indicating that loss-framed appeals are more effective than gain-framed appeals for breast cancer detection behaviors (only). Restricting the scope of the generalization in this way would naturally encourage attempts to account for the difference (between the effect for breast cancer detection studies and the effects for other particular detection behaviors) by identifying what is distinctive about breast cancer detection behaviors, or about the gain–loss messages used in these breast cancer detection studies, or about some other potentially differentiating factor. But these impulses would be mistaken. The advantage of loss-framed appeals is statistically significant for breast cancer detection but not for other behaviors—but this does not mean that the effect for breast cancer detection is significantly different from the effects for other behaviors. In fact, the breast cancer detection behavior effect is not significantly different from the effects for skin cancer detection behaviors ($p = .72$), behaviors to detect other cancers ($p = .54$), dental problem detection behaviors ($p = .96$), or miscellaneous detection behaviors ($p = .21$). Indeed, the effect for breast cancer detection behavior is not significantly different from the effect for all other detection behaviors combined ($p = .41$).

So perhaps the most appropriate conclusion—with respect to the scope of the effect—might be this: There is some very small nonzero overall population effect (as indicated by the dependably nonzero overall effect observed here), with potentially some similarly small population effect for each specific detection behavior. Examination of the confidence intervals in Table 2 reveals that these results are consistent with a belief that, for example, the population effect is $-.02$ both overall and for each specific kind of detection behavior; that is, a value of $-.02$ falls within the 95% CI both for all detection cases and for each of the specific behaviors broken out here. The sample effect emerges as dependable (statistically significant) only in the case of breast cancer detection behaviors, for which there is a relatively large number of studies, but the effect for breast cancer detection behaviors is not significantly different from that for other specific detection behaviors. Thus, both the overall population effect and each behavior-specific population effect might well be some very small (but nonzero) value.

There is one other potential scope condition worth considering, however. Given that Meyerowitz and Chaiken's (1987) participants were female (as were the participants in other breast-cancer detection studies), one might suspect that on disease detection topics, loss-framed appeals are more persuasive than gain-framed appeals for females but not for males. To explore this hypothesis, we examined the relationship between a study's effect size and the proportion of female participants in that study (see Table 1). The generalization under discussion implies that as the proportion of female participants increases, so should the observed advantage of loss-framed appeals. Thus, the correlation between proportion-of-female-participants and effect size is expected to be significantly negative; that is, larger proportions of female participants should be dependably associated with increasingly negative effect sizes (which indicate greater persuasive advantage to loss-framed appeals). Among the 36 cases concerning detection behaviors other than breast cancer detection, 15 did not provide information about the proportion of female participants. Across the remaining 21 studies (where the proportion-of-female participants ranged from .00 to 1.00), proportion-of-female-participants and effect size were not significantly related, $r(19) = .039$, $p = .87$.[7] Thus, there is little evidence that males and females are differentially susceptible to gain- and loss-framed appeals concerning disease detection behaviors.

In sum, concerning the scope of the effect, the evidence in hand is consistent with a belief that loss-framed appeals may have some very small persuasive advantage over gain-framed appeals for encouraging disease detection behaviors (both in general and for any specific sort of detection behavior). But because the various observed effects are so tightly clustered (and hence not significantly different from each other) and so small (and hence often not statistically significant), the scope of the effect cannot be identified confidently.

The size of the effect

Ioannidis (2005) has observed that in clinical medical research, it is not uncommon for initial studies of an intervention to report relatively strong effects but for subsequent research to yield smaller effects (for a similar conclusion about marketing research, see Hubbard & Armstrong, 1994). This pattern seems apparent here: Meyerowitz and Chaiken's (1987) early research produced an effect noticeably more powerful than many of those in later studies.[8]

There is an obvious moral to be drawn about the prudent interpretation of isolated research results. The need for replication of social-scientific research findings has been emphasized for quite some time (e.g., Hunter, 2001; Smith, 1970). But the research community should be interested in replications not only for whether they reproduce the direction of a previously observed effect, but also for the extent to which they reproduce the size of that effect.

In considering generalizations about effect size from isolated results, it will surely be useful to attend closely to sample size, as this affects the width of the confidence interval (i.e., the range of plausible population values). Where a small-sample study yields a large effect size—one sufficiently large to be statistically significant—the inevitably wide confidence interval will indicate some caution about the possible size of the population effect.

Notably, the four studies reporting a relatively large advantage for loss-framed appeals ($r \leq -.30$) averaged fewer than 50 participants: D. Cox and Cox (2001 anecdotal; $n = 55$), Hsiao (2002 testing-prevention; $n = 46$), Kwan (1996; $n = 33$), and Rothman et al. (1999 Experiment 2 detection; $n = 60$). By comparison, the mean sample size in the other 49 studies reviewed here was 183; the median was 119. That is to say, where large effects have been reported, they have commonly been obtained with relatively small samples. Larger-sample studies tended to produce smaller effects.

Plainly, large effects in small-sample studies, even if statistically significant, are invitations to misunderstandings. Attending to confidence intervals might minimize misinterpretations of small-sample effects because the wide CIs attendant to such results will indicate the considerable uncertainty about the location of the population effect.

Conclusion

When a research study yields statistically significant findings that promise some consequential practical application, there is an understandable eagerness to generalize the obtained results. Against the backdrop of Meyerowitz and Chaiken's (1987) early research finding, there has been no dearth of assertions that loss-framed messages are more persuasive than are gain-framed messages for encouraging disease detection behaviors. And although there is indeed a statistically significant difference in persuasiveness between gain- and loss-framed messages advocating disease detection behaviors, the difference is remarkably small—and apparently largely attributable to studies of messages aimed at encouraging breast cancer detection behaviors. It now seems clear that in advocacy of disease detection behaviors, the use of loss-framed rather than gain-framed appeals will not dramatically improve persuasiveness.

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Notes

1 Although these various persuasion outcomes are distinct, they are nevertheless appropriately combined as indicators of relative persuasiveness. Even if, for example,

attitudes are in general more easily changed than are behaviors, this does not mean that the difference in persuasiveness between one message form and another will vary between these outcomes. Notably, Witte and Allen's (2000) meta-analytic review of fear appeal research reported that the mean effects of variations in depicted threat severity (high vs. low depicted severity) were statistically indistinguishable for attitudinal (mean $r = .15$), intentional (mean $r = .14$), and behavioral (mean $r = .13$) outcomes. That is, conclusions about the relative persuasiveness of high- versus low-depicted-severity messages were identical regardless of whether attitude, intention, or behavior was the outcome variable. Witte and Allen reported a similar pattern of effects for variations in depicted threat susceptibility (mean *r*s of .12, .17, and .14, respectively), variations in depicted response efficacy (mean *r*s of .14, .17, and .13), and variations in depicted self-efficacy (mean *r*s of .12, .17, and .13). These results, albeit limited, suggest that when one's research question concerns the relative persuasiveness of two message forms (as in the present enterprise), one's conclusions may not be much affected by whether the persuasive outcome assessed is attitude, intention, or behavior—and hence effect sizes are appropriately combined across these indicators.

2 Because the unit of analysis was the message pair, multiple effect size estimates were obtained from a given study when the research design contained more than one message pair (e.g., Chang, 2007). In every case save one, the obtained effect sizes were based on different human samples and thus were statistically independent. The exception arises from Nan (2006), whose design contained two gain-framed appeals and two loss-framed appeals; this provided a total of four message pairs and hence four effect sizes, but some of these effect sizes had some participants in common. If one replaces those four estimates with the appropriate composite effect size that ensures complete statistical independence ($r = .143$, $N = 135$), the results (with $k = 50$, $N = 9,010$) are substantively unchanged: Mean $r = -.044$, 95% CI limits of $-.077$ and $-.010$ ($p = .01$); $Q(49) = 97.7$, $p < .001$.

3 The 18 cases coded as "miscellaneous" detection topics were a varied lot, precluding any useful further breakdown of these cases. Five concerned a hepatitis C test, but four of these were derived from Nan's (2006) design with overlapping participants, which recommended against separate examination of results for this topic; three concerned fictional (hypothetical) diseases; two each concerned diabetes testing, cholesterol screening, and cardiovascular disease testing; one each concerned screening for Chlamydia, glaucoma, HIV, and hypertension.

4 Throughout this report, power figures are those for detecting a population effect size of $r = .10$, assuming large heterogeneity, with a random-effects analysis, .05 alpha, and a two-tailed test (Hedges & Pigott, 2001).

5 These studies varied in the specific sort of breast cancer detection behavior being advocated, but there was no dependable difference (in observed gain–loss framing persuasiveness differences) between messages that advocated mammography and messages that advocated breast self-examination (BSE). Of the 17 breast cancer detection studies, 10 concerned mammography ($N = 1{,}986$, mean $r = -.041$, 95% CI limits of $-.086$ and .005 [$p = .08$], power = .88; $Q(9) = 9.2$, $p = .42$); five concerned BSE ($N = 354$, mean $r = -.116$, 95% CI limits of $-.292$ and .068 [$p = .22$], power = .26; $Q(4) = 11.2$, $p = .02$); and two advocated a variety of breast cancer detection behaviors (mammography, BSE, and clinical breast examination; $N = 396$, mean $r = -.075$, 95% CI limits of $-.172$ and .024 [$p = .14$], power = .29; $Q(1) = .29$, $p = .59$). The CIs for

mammography and BSE overlap substantially (the former is entirely contained in the latter), giving no reason to suspect that it makes much difference (to these effects) which breast cancer detection method was being advocated.

6 In Brunton's (2007) study of mammography appeals, the outcome assessment asked for participants' judgments about the relative likely persuasiveness of gain- and loss-framed appeals; our review excluded such cases on the grounds that assessments of observed persuasive effects are to be preferred over assessments of perceived or expected persuasiveness. If Brunton's (2007) results ($r = .438$, $N = 596$) had been added to the set of breast cancer detection cases, the advantage for loss-framed appeals on that topic would have become nonsignificant: $k = 18$, $N = 3,332$, mean $r = -.033$, 95% CI limits of $-.143$ and .077 ($p = .55$), power = .98; $Q(17) = 155.4$, $p < .001$. The point is not that the apparent advantage of loss-framed appeals for breast-cancer detection is not genuine, but rather that it can be dangerous to fail to distinguish assessments of observed persuasive effects from assessments of expected and perceived persuasive effects. Assessments of perceived and observed effects do covary (Dillard, Weber, & Vail, 2007), and there are circumstances in which assessment of perceived effects might appropriately and justifiably be used as a proxy for assessment of observed effects (as in formative-research circumstances where assessment of observed effects is impractical or impossible; Dillard et al., 2007), but where assessments of observed effects are available they should (ceteris paribus) be preferred.

7 These 21 cases included four studies of sex-specific topics, namely, prostate cancer screening (Cherubini, Rumiati, Rossi, Nigro, & Calabro, 2005, fear-evoking and reassuring), colposcopy (Lauver & Rubin, 1990), and testicle self-examination (Umphrey, 2003). Excluding these cases, across the remaining 17 studies (where the proportion of female participants ranged from .41 to 1.00), proportion-of-female-participants and effect size were not significantly related, $r(15) = -.038$, $p = .88$.

8 In Meyerowitz and Chaiken's (1987) study, the effect size was $r = -.280$ with $N = 44$. In Lalor's (1990) subsequent research using the same messages, the effect was $-.160$ with $N = 47$. The composite effect, as reported in Table 1, is $r = -.219$.

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