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The Persuasive Effects of Message Sidedness Variations: A Cautionary Note Concerning Allen's (1991) Meta-Analysis

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There are several reasons to be cautious about accepting the claims contained in Allen's (1991) meta-analysis of the research literature concerning the relative persuasive effectiveness of one-sided and two-sided messages. The classification of the message types involved in a number of studies is questionable, and the literature search underlying the review appears to be incomplete. These problems indicate that it would be imprudent to accept the reviewer's conclusions at face value, but, more important, they also underscore the importance of evolving standards for the assessment of meta-analytic research.

THERE IS A SUBSTANTIAL RESEARCH LITERATURE CONCERNING the relative persuasive effectiveness of "one-sided" and "two-sided" persuasive messages. As commonly conceived, a one-sided message simply presents arguments supporting the advocated position; a two-sided message, in addition to presenting supporting arguments, also discusses opposing arguments.

Allen (1991) presented a meta-analysis of sidedness research. Briefly, in meta-analysis each individual study contributes an estimate of the size of the relevant relationship or effect (in this case, the effect of sidedness variations on persuasive outcomes); these effect sizes can then be averaged, analyzed for their internal consistency (homogeneity), divided into subgroups of interest for analysis and comparison, and so on (for a general introduction, see Rosenthal, 1991).

Allen (1991) recognized two different ways in which two-sided messages have been created by researchers. Some two-sided messages undertook to refute the opposing arguments they discussed, criticizing them directly, pointing to weaknesses in evidence or reasoning, and so forth; other two-sided messages, while discussing opposing arguments, did not explicitly refute those arguments (in some cases, for instance, the opposing arguments were simply mentioned as opposing considerations, without any direct attack on the arguments). Since these two forms of two-sided messages might produce different effects, Allen's (1991)

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meta-analysis classified studies on the basis of whether the two-sided messages involved were "refutational" or "nonrefutational" messages, thereby permitting separate analyses of refutational and nonrefutational studies.

THE ADEQUACY OF ALLEN'S (1991) META-ANALYSIS

A preliminary note: Allen's (1991) report contains some computational inconsistencies. Specifically, the text's reported mean correlations and total Ns (as given on pp. 395-396) for the 26 studies overall, for the 6 studies with nonrefutational two-sided messages, and for the 19 studies with refutational two-sided messages are not consistent with the corresponding mean correlations and total Ns computed from the information provided in Table 2 (p. 397). This matter will be addressed further in Allen's remarks in this issue.

Computational inconsistencies aside, there are two central reasons for concern about the adequacy of Allen's (1991) meta-analysis: the classification of a number of studies as refutational or nonrefutational is questionable and the literature search underlying the review appears to be incomplete.

Misclassification of Studies

An examination of the primary research reports reviewed in Allen's (1991) meta-analysis raises some questions about the classification of studies as involving refutational or nonrefutational two-sided messages. Of the 26 studies reviewed, 19 were classified by Allen (1991) as refutational, 6 as nonrefutational, and 1 was described as unclassifiable. But several of the classification decisions are questionable, if not simply mistaken. In particular, at least 6 studies that Allen (1991) classifies as "refutational" appear not to involve two-sided messages that explicitly refute opposing arguments.¹ These studies are: Bettinghaus and Baseheart (1969), Etgar and Goodwin (1982), Hovland, Lumsdaine, and Sheffield (1949), Kanungo and Johar (1975), Kaplowitz and Fisher (1985), and Koballa (1984a).²

Bettinghaus and Baseheart's (1969) study had two one-sided messages—one with arguments favorable to a given policy (the "pro" one-sided message), the other with arguments opposed to that policy (the "con" one-sided message). "The two-sided messages were constructed using both favorable and opposed arguments. The same arguments and evidence employed in the one-sided pro and con messages were employed in the two-sided messages" (Bettinghaus & Baseheart, 1969, p. 231), with pro and con arguments simply interwoven. Thus there were two two-sided messages, differing only "in the concluding paragraph of the messages where either acceptance or rejection of the topic under consideration was called for" (p. 231). Since the same message materials (same arguments, same evidence) appeared

in the one-sided and two-sided messages, it cannot be that the two-sided messages contained added refutational matter that was absent from the one-sided messages. Hence if the two-sided messages had actually been "refutational," then the one-sided messages would have had to have been equally "refutational."

Etgar and Goodwin's (1982) experimental messages were advertisements for brands of cold remedies and beer. In the one-sided message "the promoted brand was described as more appealing than three leading (named) competing brands along *all* the attribute dimensions described," whereas in the two-sided message "the communication recognized that on one or more attributes the promoted brand was less appealing than the competing brands" (p. 461, emphasis in original). Thus the sidedness manipulation consisted of varying whether the ad contained only positive information about the brand (one-sided) or both positive and negative information (two-sided). For instance, the two-sided message for "Crick Premium Beer," in addition to mentioning positive attributes of the brand, acknowledged that the "calorie content per 12-oz. servings is higher than other premium beers tested" and that "Crick Premium's price is also premium." Plainly, the "two-sided" messages did not involve denial or refutation of the negative characteristics.

Hovland et al.'s (1949) messages argued that the Pacific war would be lengthy. The one-sided version "presented *only* the arguments indicating that the war would be long," and the two-sided version added material "devoted to considering arguments for the other side of the picture" (p. 202, emphasis in original). The discussion of these opposing arguments "rarely took the form of trying to *disprove* or *deny* the truth of an important argument; rather, the truth of the argument was admitted but its force was weakened by immediately bringing in additional relevant facts" (p. 206, emphasis in original). That is, the two-sided message "mentioned the opposite arguments" (p. 206) without necessarily attacking them. Indeed, Hovland et al. specifically indicated that in the two-sided message "refutations of the opposed arguments were in general *avoided*" (p. 206, emphasis added).

Kanungo and Johar (1975) studied "the effects of qualified (two-sided) versus non-qualified (one-sided) slogans" for consumer products. "The qualified slogans always contained both a more salient positive and a less salient negative feature of the product, whereas non-qualified slogans contained only the positive feature of the product" (pp. 127-128). For instance, one "two-sided" slogan was "VIT toothpaste: you may not like its color, but you'll certainly love its nice fresh taste" (p. 130; Kanungo and Johar's Table 1 gives a complete list of slogans). Thus this manipulation is akin to that of Etgar and Goodwin (1982): in the two-sided message, negative product characteristics are acknowledged, but not refuted or denied.

Kaplowitz and Fisher's (1985) messages solicited contributions supporting a nuclear freeze referendum. There were two experimental manipulations of message content. The "nonexclusivity" manipulation consisted of including or excluding the following sentence: "If the Nuclear Freeze campaign succeeds, you will of course, realize the benefits whether or not you have contributed to the Freeze" (p. 55). The "efficacy" manipulation consisted of including one or the other of the following two passages: (a) "In a state as large as Michigan, it is very unlikely that one contribution will make the difference in whether or not the Referendum passes. And even if the Referendum passes, it may make little difference in whether or not a Nuclear Freeze comes about" vs. (b) "Each contribution to the Freeze increases the chance that the Nuclear Freeze campaign will succeed. Your contribution *will* make a difference" (p. 55, emphasis in original). Nowhere in either manipulation is there some explicit statement and refutation of possible counter-arguments.³

Finally, concerning Koballa (1984a): There is no indication whatever that Koballa's (1984a) two-sided message contained any refutation of opposing arguments. On the contrary, it is clear that the two-sided message simply added some opposing arguments to the one-sided message; thus the one-sided communication contained "only supportive arguments, while the two-sided communication contained both supportive- and counter-arguments" (Koballa, 1984b, p. 100). That is to say, Koballa's manipulation paralleled that of Bettinghaus and Baseheart (1969). Interested readers can inspect Koballa's (1984a) messages themselves as the texts appear in Koballa (1984b).

In fact, Koballa (1984a), Bettinghaus and Baseheart (1969), Hilyard (1966), and Rosnow (1968) all used the same form of two-sided message, a form in which both supporting and opposing arguments were included, without the opposing arguments being attacked directly. But this form of experimental manipulation was not classified consistently by Allen (1991): Bettinghaus and Baseheart (1969) and Koballa (1984a) were classified as refutational, Hilyard (1966) and Rosnow (1968) were classified as nonrefutational.

Altering the classification of the 6 studies is consequential. This can be seen by comparing the present Tables 1 and 2; Table 1 depicts Allen's (1991, p. 397) classification, and Table 2 depicts the reclassification. As displayed in Table 1, according to Allen (1991) the association between type of two-sided message and direction of effect is perfect: there are 6 nonrefutational studies and 19 refutational studies, there are 6 negative effect sizes and 19 nonnegative effect sizes—and these are alleged to match up perfectly: all 6 negative effect sizes are from the 6 nonrefutational studies, and all 19 nonnegative effect sizes are from the 19 refutational studies.⁵

TABLE 1
Allen's (1991) Classification of 25 Message Sidedness Studies

	<i>r</i>	<i>n</i>
<i>Refutational cases (k = 19)</i>		
Bettinghaus & Baseheart (1969)	.052	120
Chu (1967)	.004	273
Dipboye (1977)	.008	84
Dycus (1976)	.293	26
Eggar & Goodwin (1982)	.118	120
Ford & Smith (1991)	.117	220
Hass & Linder (1972)	.306	40
Hovland, Lumsdaine, & Sheffield (1949)	.000	428
Jaksa (1963)	.012	1028
Kanungo & Johar (1975)	.058	96
Kaplowitz & Fisher (1985)	.059	1600
Koballa (1984a)	.264	58
Koehler (1968)	.062	360
Koyama (1981)	.225	66
Ley, Whitworth, Woodward, & Yorke (1977)	.184	188
Lumsdaine & Jamis (1953)	.055	86
McCroskey, Young, & Scott (1972)	.229	98
Weston (1967)	.122	240
Winkel (1984)	.125	189
<i>Nonrefutational cases (k = 6)</i>		
Belch (1981)	-.085	100
Hilyard (1966)	-.175	240
Jones & Brehm (1970)	-.203	84
Paulson (1952, 1954)	-.050	978
Rosnow (1968)	-.017	197
Sinha & Dhawan (1971)	-.002	100

But the picture is much less "clean" when the 6 studies are reclassified. As Table 2 indicates, when the 6 (previously "refutational") studies are reclassified as nonrefutational, the refutational effect sizes are all positive, but the nonrefutational effect sizes are both positive and negative. Whereas Allen's text reported mean correlations of $-.060$ for nonrefutational studies ($k = 6$) and $.076$ for refutational studies ($k = 19$), the reclassification yields mean correlations of $.004$ for nonrefutational studies ($k = 12$) and $.073$ for refutational studies ($k = 13$).⁶ That is, with the reclassification of 6 investigations, both refutational and nonrefutational studies have positive mean correlations, indicating an advantage for two-sided messages over one-sided messages, a finding rather at odds with Allen's (1991) discussion. Although the mean correlation for nonrefutational studies is only trivially positive, the difference between refutational and nonrefutational two-sided messages is (given the reclassification) unquestionably smaller than one would have thought on the basis of Allen's review.⁷

TABLE 2
A Reclassification of Allen's (1991) 25 Message Sidedness Studies

	<i>r</i>	<i>n</i>
<i>Refutational cases (k = 13)</i>		
Chu (1967)	.004	273
Dipboye (1977)	.008	84
Dycus (1976)	.293	26
Ford & Smith (1991)	.117	220
Hass & Linder (1972)	.306	40
Jaksa (1963)	.012	1028
Koehler (1968)	.062	360
Koyama (1981)	.225	66
Ley, Whitworth, Woodward, & Yorke (1977)	.184	188
Lumsdaine & Janis (1953)	.055	88
McCroskey, Young, & Scott (1972)	.229	96
Weston (1967)	.122	240
Winkel (1984)	.125	189
<i>Nonrefutational cases (k = 12)</i>		
Belch (1981)	-.085	100
*Bettinghaus & Baseheart (1969)	.052	120
*Etgar & Goodwin (1982)	.118	120
Hilyard (1966)	-.175	240
*Hovland, Lumsdaine, & Sheffield (1949)	.000	428
Jones & Brehm (1970)	-.203	84
*Kanungo & Johar (1975)	.058	96
*Kaplowitz & Fisher (1985)	.059	1600
*Koballa (1984a)	.264	58
Paulson (1952, 1954)	-.050	978
Rosnow (1968)	-.017	197
Sinha & Dhawan (1971)	-.002	100

NOTE: "*" indicates a study whose classification in Table 2 differs from its classification in Table 1.

Incomplete Literature Search

Grounds for doubt exist concerning the completeness of the literature search underlying Allen's (1991) review. There are a number of studies that would seem to be relevant to the general topic of message sidedness, but that were not included in the review nor mentioned in the review as studies that had been excluded on principled grounds. No pretense is made of giving a complete list of such studies here, but it would have seemed prudent for any review of the message sidedness literature to have at least examined studies such as: Alpert and Golden (1982), Halverson (1974), Hastak and Park (1990), Kamins and Marks (1987), Kohn and Snook (1976), Ley, Bradshaw, Kinsey, Couper-Smartt, and Wilson (1974), Lilienthal (1972), Schanck and Goodman (1939), and Swinyard (1981). The claim is not that all these studies would have, in the end, been appropriately included in the review; it may be, for instance, that effect sizes cannot be extracted from some of these

studies, or that in other ways some of these studies would not appropriately be included.⁸ But certainly some of these investigations can shed light on the questions addressed in the review.⁹ In any case, the lack of mention of a number of potentially relevant studies—and these do not exhaust the uncited relevant literature—provides grounds for concern about the adequacy of the literature search.

CONCLUSIONS

There is a narrow point to be made here, concerning conclusions about the persuasive effects of variations in message sidedness. That point is that conclusions based on Allen's (1991) meta-analysis should not be accepted incautiously. The doubtful message-type classifications and the apparent incompleteness of the literature search suggest that it would be imprudent to accept Allen's (1991) claims at face value. Obviously, more confident conclusions about the persuasive effects of message sidedness variations will need to await the completion of more adequate reviews of this research area. It may be that, upon closer review, all or most of Allen's general conclusions will be confirmed, or perhaps very few will be; but at present the prudent course of action appears to be to withhold judgment.

There is also a broader matter to be raised, concerning the critical scrutiny of meta-analytic research reviews. Several general discussions of this topic have already suggested a number of desiderata for meta-analytic reviews (e.g., Becker, 1991; Cooper, 1982). For example, the comprehensiveness of the literature review is an obviously relevant criterion—though the present case suggests that the obstacles to literature retrieval may be greater than commonly supposed.

But there is one specific criterion that is particularly relevant to the present discussion, namely, the use of intersubjective checks on meta-analytic coding decisions. Where studies are classified or coded in some way, then (when appropriate) such checks may be desirable or even necessary. In this regard, meta-analytic research might well be treated rather like primary research. In both sorts of work, simple error—not to mention investigator bias—can be minimized by asking that coding decisions be independently checked.¹⁰ Although in both sorts of research, not all coding decisions will need such checking, it can nevertheless be helpful to keep in mind the possibility of obtaining intercoder reliability estimates. And even where reliability estimates are not computed, the double-checking of study classifications, effect size computations, statistical tests, and the like can help minimize errors.

In sum, there are good reasons for withholding assent from the claims and conclusions of Allen's (1991) meta-analysis of the persuasive effects of message sidedness variations. However, this case illustrates the importance of evolving standards for the assessment of meta-analytic work, and particularly underscores the desirability of independent checks of meta-analytic decisions.

NOTES

1. The present classification of these 6 studies as nonrefutational is based on two independent coders' classifications of the sidedness manipulations in a larger sample of message sidedness studies. These two coders independently classified 32 investigations, with 88% agreement concerning the refutational-versus-nonrefutational classification of the studies; disagreements were resolved through discussion.

2. These are *not* the only studies whose classification in Allen (1991) is questionable (though they provide a convenient focus for the current discussion). For instance, for at least 3 other studies, grounds for doubt exist about Allen's (1991) classification of those studies as refutational: in Koyama (1981) and Weston (1967), it is not in fact clear that one-sided and two-sided messages are compared in the research; and Lumsdaine and Janis's (1953, p. 313) report indicates that in discussing opposing arguments "in some instances no attempt was made to refute them," which suggests that the two-sided message was only partially refutational. As another example: whereas Allen (1991) was unable to classify McGinnies (1966) as refutational or nonrefutational, there is at least some reason to think this study is appropriately described as nonrefutational (see McGinnies, 1966, p. 89). The point is that the discussion in the text focuses on relatively clear-cut cases that can be discussed briefly, and so should not be taken as comprehensive.

3. Indeed, one might wonder whether there is even some *nonrefutational* form of two-sided message here. Perhaps version (a) of the efficacy manipulation acknowledges, where version (b) does not, some possible counterarguments; thus the efficacy manipulation—akin to the manipulations of Etgar and Goodwin (1982) and Kanungo and Johar (1975)—might be taken to involve a nonrefutational form of a two-sided message. This matter becomes complicated, however, because Kaplowitz and Fisher's (1985) Table 2 (p. 57) indicates that the effect size (expressed as r) associated with the efficacy manipulation is .013, and that associated with the nonexclusivity manipulation is .059. Allen (1991) reports an r of .059 for this study, suggesting that it was the nonexclusivity manipulation that was taken to represent a sidedness manipulation such that including the quoted sentence represented the "two-sided" condition. Frankly, it is difficult to see how including the nonexclusivity sentence represents refutation of counterarguments.

4. These tables and the accompanying analysis use the effect sizes and sample sizes in Allen's (1991, p. 397) Table 2; that is, for the purpose of these analyses, Allen's computation of the direction and magnitude of the effect sizes and his report of the sample sizes are assumed to be correct. Additionally, for the sake of comparability, all mean r s were computed following the sample-size weighting procedure employed by Allen (1991, p. 395). In both these tables, and in the following discussion, the study of McGinnies (1966) has been omitted because Allen (1991) was unable to classify McGinnies's manipulation as refutational or nonrefutational. Obviously, if (as suggested in note 2) McGinnies's (1966) investigation is treated as nonrefutational, or if the studies of Koyama (1981), Lumsdaine and Janis (1953), or Weston (1967), are treated differently, still other changes would result. But the relevant point can be seen by considering simply the six studies discussed in the text.

5. Notably, of the 4 previously-discussed studies that had similar sidedness manipulations that were classified inconsistently by Allen (1991), the ones reported by Allen to have positive effect sizes were classified by Allen as refutational (Bettinghaus & Baseheart, 1969; Koballa, 1984a), whereas the ones reported to have negative effect sizes were classified as nonrefutational (Hilyard, 1966; Rosnow, 1968).

6. As indicated in note 4, this analysis has assumed the accuracy of Allen's computations of the direction of effect, magnitude of effect, and sample size. This is a counterfactual assumption. For instance, Allen (1991) misrepresents the direction of effect for Hass and Linder (1972): Allen reports that for this study, the direction of effect favors the two-sided message; however, inspection of Hass and Linder's Experiment 2 (the only experiment for which an N of 40, as Allen employs, could be correct) reveals that in the "forewarned" condition (the only one containing two-sided messages) in fact the one-sided message (the "pro-only" message, mean = 38.14, n = 14) was more effective than either individual version of the two-sided message (for the "refutation-pro" order message, mean = 28.92, n = 13; for the "pro-refutation" order message, mean = 37.08, n = 13, with higher means

indicating greater acceptance of the advocated position), and so of course was more effective when results are combined across the two versions of the two-sided message (combined two-sided n = 26, combined mean = 33.00); see Hass & Linder (1972, Table 2, p. 224). Or, as another example, Allen (1991) reports the sample size for Ley et al. (1977) as 188, but (as Ley et al. make clear in Table 3 on p. 69) the most relevant questionnaire item (35b, desire to participate) was answered by only 177 of the participants (a fact that could have been discerned either from the raw numbers of participants given in Table 3 or from the degrees of freedom given in the ANOVA in Table 4; the only other questionnaire item for which data are reported (item 35a, desire for further information) was answered by 182 participants).

7. If (following note 6) the sign of the correlation for Hass and Linder (1972) is reversed and an n of 177 is employed for Ley et al. (1977), the mean correlation for refutational studies becomes .064, further shrinking the difference between refutational and nonrefutational studies.

8. Some of these uncited items are dissertations, but this is not a basis for their exclusion; Allen's (1991) review included several dissertations (e.g., Jaksa, 1963). One of these uncited items appeared in 1990, relatively close to the publication date of the review, but this is not a reason for its exclusion; Allen's (1991) review included an even more recent article, from a July/August 1991 journal issue (Ford & Smith, 1991). Some of these uncited items lack attitudinal data; Ley, Bradshaw, et al. (1974) report behavioral data, and Swinyard (1981) reports assessments of behavioral intention and acceptance of key claim. But this is not a basis for exclusion; the review included the studies of Kaplowitz and Fisher (1985), who report only results for a behavioral measure, and Winkel (1984), who reports results for behavioral intention and behavioral measures.

9. The omissions of Ley, Bradshaw, et al. (1974) and Swinyard (1981) are of particular concern. These are cited in the reference lists of articles appearing in Allen's (1991) bibliography: Swinyard (1981) is cited by Kaplowitz and Fisher (1985); Ley, Bradshaw, et al. (1977) is cited by both Ley, Whitworth, Woodward, and Yorke (1977) and by Skilbeck, Tulips, and Ley (1977). Since the literature review involved "searching all of the references and bibliographies of located studies and reviews" (Allen, 1991, p. 393), these studies should have been identified and retrieved (as it was claimed that "no study was unobtainable," p. 393). Inspection of these articles suggests that they would appropriately have been included in the review: each studies the persuasive effects of sidedness variations, an effect size index appears to be computable for each study, and so on.

10. It will not have escaped the careful reader's notice that the errors discussed here (misclassification of studies, incorrect effect sizes, etc.) display an unsettling pattern. The errors make the results cleaner than they would otherwise appear because the errors (a) maximize the difference between the refutational and nonrefutational categories and (b) minimize any variation within these categories. This direction of error is congenial with a belief that the point of meta-analysis is the establishment of dependably different but internally homogeneous categories for effect sizes.

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