Chapter 4

The persuasive effects of variation in standpoint articulation

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1. Background[1]

1.1 Explicitness as a normative procedural obligation

Addressing the empirical relationship between normatively good argument and persuasive outcomes requires some independently-motivated normative account (O'Keefe & Jackson 1995). Among various ways of conceiving of the normative worth of arguments, the idea of focussing on advocates' conduct (rather than some disembodied abstract representation of argument) has attracted recent attention. For example, the pragma-dialectical approach (van Eemeren & Grootendorst 1984) identifies rules of conduct appropriate for (different stages of) argumentative discussions. From this perspective, normative standards for argument consist of procedural obligations for advocates. Thus normatively good argumentation will be understood not as a matter of (e.g.) true premises and valid form, but as a matter of argumentative practice that satisfies specified procedural standards.

Obviously, developing a full-fledged description and defense of a set of procedural obligations for normatively good argumentation is a substantial undertaking. However, even without a finished analysis of all the procedural obligations associated with normatively good argumentation, one might nevertheless say with some confidence that one normative good in the conduct of advocates is (in some way or other) argumentative explicitness. That is, it is normatively desirable for advocates to explicitly state their viewpoints, without concealing relevant aspects of their views or reasoning. "Evasion, concealment, and artful dodging ... are and should be excluded from an ideal model of critical discussion" (van Eemeren, Grootendorst, Jackson & Jacobs 1993: 173). Argumentation that is more explicit is better (normatively speaking) than argumentation that is less explicit, precisely because greater explicitness opens the advocated view for critical scrutiny.

Understood as a procedural obligation, explicitness in argumentation naturally takes a diversity of concrete instantiating forms. That is to say, a normative

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directive to be 'explicit' can naturally be realized in different ways. The focus of the present report is the explicitness with which the message articulates the overall advocated position (that is, the degree of articulation of the message's overall conclusion, recommendation, standpoint). This focus may be contrasted with a focus on the degree of articulation of the message's *support* for its conclusion (e.g., the degree to which the message explicitly lays out each of its individual supporting arguments). This latter facet of explicitness is the one most commonly taken up in discussions of the normative value of explicitness, as when it is suggested that advocates have a responsibility to spell out the premises of their arguments if called on to do so (see, e.g., van Eemeren & Grootendorst 1984: 168-169).

But there plainly is a normative responsibility to be clear about one's overall conclusion (although this sometimes seems almost taken for granted in analyses of normative argumentation principles). If an advocate's standpoint is not sufficiently clear at the outset, then, broadly speaking (expressed in pragma-dialectical terms) the confrontation stage cannot be genuinely successful; the parties may misunderstand what opinions are the subject of dispute, advocates might evade subsequent criticism (e.g., by objecting to hearers' reconstruction of the standpoint), and thus resolution of disputes can be obstructed (see van Eemeren & Grootendorst 1984: 154-158).

1.2 Explicitness as threatening persuasive effectiveness

Advocates might understandably fear that standpoint explicitness can threaten persuasive effectiveness. Such fears have warrant. Most generally, explicitness enlarges the 'disagreement space,' in the sense that it puts more claims on the table for discussion – claims to which objections might be raised (for discussion of the idea of disagreement space, see van Eemeren et al. 1993, esp. 95-96; Jackson & Jacobs 1980). Each further articulation of an advocate's viewpoint invites closer scrutiny, counterargument, objection, rejection. Failing to be fully explicit might minimize the space for disagreement and thereby enhance persuasion.

Advocates might also fear that explicitness could produce "boomerang" persuasive effects in which the audience changes in ways opposite to those sought by the advocate. Being too explicit might insult the audience (because the message would state the obvious) or anger it (because the message would seem too aggressive, too insistent, too directive), and perhaps induce reactance, a motivational state aimed at reestablishing threatened freedom of action (Brehm & Brehm 1981).

Even if explicitness does not backfire, an advocate might think that less-explicit messages could be more persuasive because they invite the audience's active participation (in enthymematic persuasion). If message receivers have to 'fill in the blanks' to complete the message's reasoning process, the act of spelling out the message's conclusion might lead them to be more persuaded than if the conclusion had been explicitly provided by the advocate.

Given such fears, an advocate might well decide to be less-than-fully explicit in articulating his or her overall conclusion. Broadly, there are two main ways in which advocates can be inexplicit about their overall standpoint. The first is simply to omit a statement of one's overall conclusion; that is, although the message contains appropriate materials to support the advocate's overall position, the message nevertheless leaves that conclusion unstated. The second is to state the overall conclusion, but to do so in a global (general, nonspecific) way; for instance, the advocate might identify the desired action, but describe it in a general rather than specific fashion.

These two means correspond to two distinct lines of empirical research examining the persuasive effects of variation in conclusion articulation. In one line (labelled here studies of 'conclusion omission'), investigators have compared messages with or without a statement of the advocate's overall conclusion. That is, what is compared are messages containing some explicit statement of the advocate's overall point with messages in which the conclusion is omitted (not explicitly stated). For example, Struckman-Johnson and Struckman-Johnson (1996) compared AIDS public service announcements with and without an explicit recommendation to use condoms.

The other body of studies concerns the degree of specificity of the description of the advocated action (here termed studies of 'conclusion specificity'). The comparison contrasts messages that provide only a general description of the advocate's recommended action with messages that provide a more specific (detailed) recommendation; that is, both messages contain an explicitly-stated conclusion (in the form of an explicitly-identified desired action), but one conclusion is more detailed. For example, Evans, Rozelle, Lasater, Dembroski, and Allen (1970) compared messages giving relatively general and unelaborated dental-care recommendations with messages giving more detailed, specific recommendations.

Taken together, these two bodies of research speak to the broad question of the persuasive effects of variations in argumentative conclusion explicitness. But each is also of interest in its own right as an examination of a factor potentially influencing advocacy outcomes. The two message variations are distinguishable (though related), and might have distinctive effects, with distinctive explanations, moderators, and so on. Thus it will be useful to consider previous discussions of these two variations.

1.3 Prior discussions of conclusion articulation effects

1.3.1 Conclusion omission

Many summary discussions of the persuasion effects research literature include some treatment of studies of the effects of including or omitting message conclusions (e.g., Burgoon 1989; Cohen 1964; McGuire 1985; O'Keefe 1990; Perloff 1993; Petty & Cacioppo 1981; Reardon 1981). These discussions commonly suggest that the relative effectiveness of stating and omitting conclusions varies as a function of other (moderator) variables. The most frequently mentioned possible

moderating factors involve the hearer's ability and willingness to draw the appropriate conclusion when left unstated; hence variables such as the receiver's intelligence (which bears on ability) and initial opinion (which bears on willingness) are often mentioned as possible moderators (see, e.g., Burgoon 1989: 144-145; Cohen 1964: 6-8; McGuire 1985: 271-272; O'Keefe 1990: 159-161; Perloff 1993: 169-170; Petty & Cacioppo 1981: 76; Reardon 1981: 142). The general expectation is that explicit conclusions may not be necessary to, and might even impair, persuasive success for intellectually more capable audiences and for audiences initially favorable to the advocated view (since such audiences should be able and willing to reason to the advocated conclusion). However, these discussions commonly consider only a small portion of the relevant literature; typically, two or three studies are mentioned, even though (as will be seen) many more relevant investigations exist. Indeed, even Cruz's (1998) review discussed fewer than half the relevant cases.

1.3.2 Conclusion specificity

Surprisingly, conventional summary descriptions of the persuasion effects literature typically do not discuss research concerning the effects of a message's providing a detailed, specific (as opposed to general) description of the recommended action. The summaries mentioned above, for instance, do not address this subject.

Indeed, studies of this message variation evince little recognition of related work. For example, among the relevant studies (as listed in Table 2), Mann and Hill (1984) cite Geller (1973) and Geller, Witmer, and Orebaugh (1976), but no other relevant papers; Stout and Sego (1994a) cite Leventhal, Singer, and Jones (1965), but no other relevant paper; no paper cites either Piccolino (1966) or Wright (1979); Frantz (1994) cites none of the others. In short, even to investigators studying the effects of variations in recommendation specificity, this body of research has been largely invisible.

Perhaps, then, it is unsurprising that there is no developed analysis of how this message variation might influence persuasive effects. That is, this is a largely untheorized domain. But it is easy to see how greater specificity might reduce persuasive effectiveness through the same mechanisms as could influence conclusion-omission effects: greater specificity widens the disagreement space by providing a more detailed description of the advocate's view; it invites reactance because it may suggest increased directiveness; and so forth.

Thus a meta-analytic review was undertaken to address two broad questions: (1) whether there is any general persuasive advantage or disadvantage to articulated message conclusions, and (2) what the individual effects are of the two different realizations of conclusion articulation. Meta-analysis is a family of procedures for producing a systematic quantitative summary of a set of studies, and may be contrasted with more familiar traditional ('narrative' or 'qualitative') research-synthesis methods.

Traditional means of synthesizing a set of research studies have emphasized statistical significance. For example, traditional research summaries commonly try

to identify factors that distinguish studies reporting significant effects from studies not finding significant effects. But this can be misleading; two studies might find exactly the same size of effect (e.g., the same correlation), but one might be statistically significant and the other nonsignificant simply because one study had a larger sample.

By contrast, meta-analytic research synthesis methods commonly focus on the size of the effect obtained in each study (for a general introduction to meta-analysis, see Rosenthal 1991). An effect size is extracted from each relevant study, and these are combined to yield an observed average effect (with an affiliated confidence interval). Even in a circumstance in which no individual study found a dependable (statistically significant) effect, a meta-analytic summary might reveal the existence of such an effect (once data are combined across studies). Where potential moderator factors vary between studies, studies can be grouped by levels of the moderator, and the mean effect sizes within these subgroups compared. In short, meta-analytic methods offer the prospect of a systematic quantitative summary of research findings, affording a far more substantial basis for confident generalization than any single primary-research study.

2. Method

2.1 Identification of relevant investigations

2.1.1 Literature search

Relevant research reports were located through personal knowledge of the literature, examination of previous reviews and textbooks, and inspection of reference lists in previously-located reports. Additionally, searches were made through databases and document-retrieval services using such terms as 'explicit conclusion,' 'explicit message,' 'conclusion omission,' and 'specific recommendation' as search bases; these searches covered material at least through October 2001 in PsycINFO, Current Contents, Dissertation Abstracts Online, Medline, ABI/Inform, and ERIC (Educational Resources Information Center).

2.1.2 Inclusion criteria

To be included in the analysis, a study had to meet two criteria. First, the study had to compare two messages varying in the articulation of the message's overall conclusion. This included studies comparing a message with an explicitly-stated conclusion (i.e., a message that explicitly specified what the advocate wanted the audience to do or believe) and one without such a conclusion and studies comparing messages varying in the specificity with which the advocated view or action was described. Second, the investigation had to contain appropriate quantitative data pertinent to the comparison of persuasive effectiveness across experimental conditions.

Excluded were studies that varied the explicitness of the supporting argumentation (e.g., Gutteling 1993; Kardes 1988), studies that varied simultaneously the explicitness of both the overall claim and the supporting arguments (Cruz 1991) or, more generally, that confounded the manipulation of interest with other manipulations (e.g., Dixon, Parr, Yarbrough, & Rathael 1986; Gravell, Zapka, & Mamon 1985), conclusion-omission studies in which messages lacked argumentation underwriting the conclusion sought (e.g., Geller, Witmer, & Tuso 1977), studies of outcomes other than persuasion (e.g., Sigrell 1995), and studies that did not provide appropriate quantitative information about effects (e.g., Ahearne, Gruen, & Saxton 2000, CD player message; Robinson 1981).

2.2 Dependent variables and effect size measure

2.2.1 Dependent variable

The dependent variable of interest was persuasiveness (as assessed through measures such as opinion change, postcommunication agreement, behavioral intention, and the like). When a single study contained multiple indices of persuasion, these were averaged to yield a single summary.

2.2.2 *Effect size measure*

Every comparison between a relatively explicit (i.e., more articulated) message and its relatively inexplicit (less articulated) counterpart was summarized using r (correlation) as the effect size measure. Differences favoring explicit messages were given a positive sign; differences favoring inexplicit messages were given a negative sign.

When correlations were averaged across several dependent measures, the average was computed 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).

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 Greene (1992), in Greene, Rubin, and Hale (1993), and in Greene, Rubin, and Hale (1995), recorded here under the former; and in Stout and Sego (1994a), Stout and Sego (1994b), and Stout and Sego (1995), recorded here under Stout and Sego (1994a).

2.3 Analysis

The unit of analysis was the message pair (that is, the pair composed of an explicit message and its inexplicit counterpart). In one case in which a message pair was used in more than one investigation (Ahearne et al. 2000, razor message; Sawyer

& Howard 1991), results were combined (and reported under Sawyer & Howard 1991). When a study contained multiple message pairs but either did not report results separately (Sawyer & Howard 1991; Struckman-Johnson & Struckman-Johnson 1996) or did not obtain data from independent groups of participants (Slater, Karan, Rouner, Murphy & Beauvais 1998), it was treated as having only one pair; the consequence is that the present analysis underrepresents any message-to-message variability in these data.

The individual correlations (effect sizes) were initially transformed to Fisher's zs; the zs were analyzed using random-effects procedures described by Shadish and Haddock (1994), with results then transformed back to r. A random-effects analysis was employed in preference to a fixed-effects analysis because of an interest in generalizing across messages.

Meta-analysts of message effects research face a circumstance parallel to that of primary researchers whose designs contain multiple instantiations of message categories. Such multiple-message designs can be analyzed treating messages either as a fixed effect or as a random effect. The relevant general principle is that replications should be treated as random when the underlying interest is in generalization. This reflects the fact that fixed-effects and random-effects analyses test different hypotheses: a fixed-effects analysis tests a hypothesis concerning whether the responses to a fixed, concrete group of messages differ from the responses to some other fixed, concrete group of messages, whereas a random-effects analysis tests whether responses to one category of messages differ from responses to another category of messages (see, e.g., Jackson 1992: 110). A meta-analysis involves a collection of replications (parallel to the message replications in a multiple-message primary research design), and similar considerations (including whether the analyst is interested in generalization) bear on the choice between a fixed- and a random-effects meta-analysis (for some discussion, see Jackson 1992: 123; Shadish & Haddock 1994). In the present review, the interest is naturally not in the concrete messages studied by past investigators, but in the larger classes of messages of which the studied messages are instantiations; hence a random-effects analysis was the appropriate choice. In a random-effects analysis, the confidence interval around an obtained mean effect size reflects not only the usual human-sampling variation, but also between-studies variance; this has the effect of widening the confidence interval over what it would have been in a fixed-effects analysis (see Hedges & Vevea 1998; Shadish & Haddock 1994: 275).

3. Results

3.1 Overall effects

Effect sizes were available for 35 cases with a total of 14,215 participants. Details for each included case appear in Tables 1 and 2.

Across these cases, the random-effects weighted mean correlation was .101 [$Q(34) = 212.6$, $p < .001$]. The bounds of the 95% confidence interval for this

mean were .050 and .153, indicating a significantly positive overall effect favoring explicit messages. However, this analysis includes two related effect sizes. Nova's (1990) design contained a no-recommendation condition, a general recommendation condition, and a specific recommendation condition, and thus – uniquely among the studies reviewed here – supplied an effect size both for conclusion omission (comparing the no-recommendation condition against the combination of the two recommendation conditions) and for conclusion specificity (comparing the general against the specific recommendation condition). Including both effects thus produces a set of cases that are not wholly independent. However, an analysis deleting these two cases yields similar overall results: k = 33, mean r = .103, 95% CI = .048, .157; Q (32) = 211.4, p < .001.

3.2 *Conclusion omission*

Effect sizes were available for 17 cases with a total of 3,110 participants. Details for each included case appear in Table 1.

Study	r	n
Biddle (1966)	.418	160
Chebat et al. (2001) ATM card	-.120	57
Chebat et al. (2001) student loan	.026	55
Cope & Richardson (1972)	.128	278
Feingold & Knapp (1977)	.144	148
Fine (1957)	.089	339
Hewitt (1972)	.304	96
Hovland & Mandell (1952)	.235	235
Leventhal, Watts, & Pagano (1967)	.162	99
Nova (1990)	.050	163
Sawyer & Howard (1991)	-.137	252
Simonson et al. (1987)	-.062	81
Slater et al. (1998)	.059	241
Struckman-Johnson & Struckman-Johnson (1996)	.080	219
Thistlethwaite, de Haan, & Kamenetsky (1955)	.011	428
Tubbs (1968)	.376	52
Weiss & Steenbock (1965)	-.023	99

Table 1
Conclusion omission cases

Across these cases, the random-effects weighted mean correlation was .103 [Q (16) = 63.8, p < .001]. The bounds of the 95% confidence interval for this mean were .025 and .180, indicating a significantly positive overall effect favoring messages with an explicit statement of the overall conclusion.

The significant heterogeneity among these effect sizes might invite a search for possible moderator variables. However, the number of effect sizes makes it

unlikely that possible moderator variables can be identified by locating dependably different subsets of effect sizes (and, relatedly, any subset of effect sizes appearing to be homogeneous might reflect low power for detecting heterogeneity). Still, it may be useful to examine these effect sizes for what they might reveal about the two moderator variables mentioned in previous discussions of this research area.

3.2.1 Audience intellectual capability

Across the studies, there was little relevant variation in audience intellectual capability. For example, in all but three studies, the participants were college undergraduates (Feingold and Knapp (1977) studied high school students; Slater et al. (1998) studied broadly representative population sample; Thistlethwaite, de Haan, and Kamenetzky (1955) studied Air Force recruits). However, Hovland and Mandell's (1952) study provided a within-study comparison involving receiver intelligence. In both the high-intelligence and low-intelligence conditions, explicit conclusions enjoyed a persuasive advantage: respectively, r = .299 (n = 99) and .198 (n = 53). These correlations are not significantly different (z = .62).

3.2.2 Audience initial position

Across studies, there was little relevant variation in the audience's initial position; in most cases, the audience's initial position could not be confidently classified as favorable or unfavorable to the advocated view. Three studies (Cope & Richardson 1972; Hewitt 1972; Leventhal, Watts & Pagano 1967) used audiences with initially unfavorable attitudes; in one study (Feingold & Knapp 1977) the audience's initial attitude was favorable to the advocated view; and two studies provided sufficient information to permit retrieval of within-study comparisons involving initial audience position (Fine 1957; Weiss & Steenbock 1965). Combining effects across these cases, the average correlation was .059 for receivers with initially favorable attitudes (k = 3, n = 356, 95% CI = -.184, .301) and .141 for those with initially unfavorable attitudes (k = 5, n = 703, 95% CI = .055, .226). These two mean correlations are not significantly different.

3.3 Conclusion specificity

Effect sizes were available for 18 cases with a total of 11,105 participants. Details for each included case appear in Table 2.

Study	r	n
M. C. Anderson (1981)	.107	700
Evans et al. (1970)	.319	158
Frantz (1994)	.417	80
Geller (1973) college lobby	.120	81
Geller (1973) grocery store	.160	400
Geller (1973) theatre B	-.123	130
Geller, Witmer, & Orebaugh (1976)	.281	3323
Greene (1992) passage A	-.079	252
Greene (1992) passage B	.016	240
Leventhal, Jones, & Trembly (1966)	.102	417
Leventhal, Singer, & Jones (1965)	.084	117
Mann & Hill (1984)	.095	3520
Nova (1990)	.111	111
Piccolino (1966)	.059	1080
Stout & Sego (1994a) Study 1	.005	116
Stout & Sego (1994a) Study 2	-.197	95
Tanner, Day, & Crask (1989)	.110	193
Wright (1979)	.160	92

Table 2
Conclusion specificity cases

Across these cases, the random-effects weighted mean correlation was .100 [Q (17) = 138.8, $p < .001$]. The bounds of the 95% confidence interval for this mean were .030 and .170, indicating a significantly positive overall effect favoring messages with more specific descriptions of the recommended view.

4. Discussion

As a preliminary observation, it might be noticed that these results nicely illustrate the potential value of meta-analytic methods. Most individual studies found no significant differences in persuasiveness between explicit and nonexplicit conclusions, and yet (as the meta-analytic findings reveal) there is in fact an underlying dependable effect.

4.1 Overall effects

The observed overall effects suggest that, on the basis of the empirical evidence to date, advocates have little to fear from being explicit about their overall standpoint. On the contrary, clearly articulating one's overall conclusion appears to dependably enhance persuasive effectiveness. Indeed, only 7 of the 35 observed effect sizes are negative (4 of 17 for conclusion omission, 3 of 18 for conclusion specificity).

There is, to be sure, significant variability among the observed effect sizes. And there might well be circumstances in which greater standpoint articulation would impair persuasive success; identifying such circumstances would be a useful goal of future research. But broadly speaking, advocates would appear to be well-advised – for both normative and instrumental reasons – to be explicit in articulating their standpoint.

4.2 Conclusion omission

The generally positive effect for conclusion articulation obtains in the specific case of conclusion-omission variations: messages with explicitly-stated conclusions are significantly more persuasive than those omitting such conclusions.

Previous discussions of this message variation have suggested that if a message omits its overall conclusion, then receivers who cannot (because of intellectual inability) or will not (because of holding opposing views) reason to the conclusion themselves will not understand (comprehend) the conclusion, and so will not be persuaded. But there is no evidence that the general advantage of explicitly-stated conclusions reverses with receivers of greater intellectual ability or with receivers holding more favorable initial opinions. To be sure, for neither moderator is there much empirical evidence available, making any conclusions necessarily tentative. Notably, however, given the predominance of college students as participants, the finding of an overall advantage for explicitly-stated conclusions suggests that even with these (relatively) intellectually more capable receivers, explicitly-stated conclusions enjoy a persuasive advantage. This evidence suggests that the audience's inability to reason to the message's conclusion does not underlie the observed relative persuasive disadvantage of messages omitting explicit conclusions.

The earlier accounts of conclusion-omission effects seem to reflect a deductive conception of the relevant underlying process, in which a receiver, given some premises (the materials in the body of the message), must deduce some consequence (the advocate's overall conclusion). Such a conception naturally draws one's attention to factors concerning the receiver's ability and willingness to reason to the advocate's conclusion. But given little empirical evidence to support moderating roles for such factors, other approaches might be considered.

One possibility is that the observed effects reflect the operation of assimilation and contrast effects in the perception of persuasive messages, as first described clearly by social judgment theory (C. W. Sherif, Sherif & Nebergall 1965; M. Sherif & Hovland 1961). Assimilation and contrast effects are perceptual effects concerning the judgment of what position is being advocated by a message. An assimilation effect occurs when the receiver perceives the message to advocate a view closer to his or her own than it actually does; a contrast effect occurs when the receiver perceives the messages to advocate a position more discrepant from his or her own than it actually does. Assimilation and contrast effects reduce persuasive effectiveness -contrast effects because they make the message appear to urge an even more unacceptable (more discrepant) viewpoint, assimilation effects

because they reduce the amount of change apparently sought by the advocate (as when a receiver mistakenly thinks the message is advocating the receiver's current position). What makes these effects of interest in the present context is the research evidence indicating that only relatively ambiguous messages (that is, messages ambiguous about what position is being advocated) are subject to assimilation and contrast effects (Granberg & Campbell 1977; C. W. Sherif et al. 1965: 153; M. Sherif & Hovland 1961: 153).

Thus one mechanism that might explain the reduced persuasive success of messages omitting explicit conclusions is that such messages are relatively more subject to assimilation and contrast effects. When an advocate fails to be explicit about his or her overall standpoint, receivers may misperceive the advocated view – misperceive it as similar or identical to their own (and so as requiring little change in viewpoint), or misperceive it as advocating some rather more discrepant view (which again would reduce persuasiveness). It will be useful for future research to explicitly examine the possibility that assimilation and contrast effects might underlie the observed persuasive effects of conclusion-omission variations.

4.3 Conclusion specificity

The positive effect for conclusion articulation also obtains in the specific case of conclusion specificity variations: messages with more detailed, specific descriptions of the recommended action are significantly more persuasive than those providing more general, nonspecific descriptions.

Explanations for this previously-unnoticed general effect will necessarily be speculative, but two (not entirely unrelated) lines of research might be illuminating. The first is research concerning the effects of persons imagining themselves performing particular behaviors. At least under some conditions, imagining performing a hypothetical future behavior can lead to increased (perceived and actual) likelihood of performing that behavior (Anderson 1983; Gregory, Cialdini & Carpenter 1982; Sherman & Anderson 1987). Although the underlying processes are not entirely clear yet, it appears that imagining such behavioral performance 'makes more available relevant behavioral scenarios or causal reasons [for performing the behavior], either of which may be used to guide subsequent behavioral choices' (Sherman & Anderson 1987: 302). It may be that messages with more specific descriptions of recommended actions make it easier for receivers to imagine themselves performing that action, which in turn enhances persuasive effectiveness.

The second body of research concerns the role of behavioral self-efficacy (or perceived behavioral control) in action. A number of different theoretical perspectives, including the theory of planned behavior (Ajzen 1991) and social cognitive theory (Bandura 1986, 1991), have suggested that one key factor influencing a person's (intended or actual) performance of a given behavior is that person's belief in his or her ability to engage in the behavior. For example, persons who think that they don't have the ability to engage in a regular exercise program (because they lack the time, lack equipment, etc.) are unlikely to undertake such behavior, even if they have positive attitudes toward exercising.

Self-efficacy can be influenced in various ways (see Bandura 1986: 399-409). Most powerfully, of course, successful performance of the behavior in question can enhance perceived self-efficacy. But in the present context, it is of some interest that vicarious experience – seeing or visualizing another person (a model) successfully perform the action – can also enhance self-efficacy beliefs (for discussion and some applications, see Bandura 1986: 399-400 and 403-405; Cleaveland 1994; Eden & Kinnar 1991; Hagen, Gutkin, Wilson & Oats 1998; Mahler, Kulik & Hill 1993). It might be that conclusion-specificity variations have their observed persuasive effects through influencing perceived self-efficacy. That is, akin to the effects of observing models, receivers who encounter a detailed description of the recommended action may become more convinced of their ability to perform the behavior. Such an explanation can only be speculative, but obviously suggests a possible focus for future research.

Plainly, additional research will be necessary to identify the limits of conclusion-specificity effects (identifiable circumstances under which the effect does not obtain, or even reverses) and to specify how and why the effects come about. But, on the basis of the evidence in hand, it seems clear that persuasive effectiveness is generally enhanced through more detailed descriptions of the advocated actions.

5. Conclusion

Normative and instrumental considerations potentially conflict when advocates consider how to construct persuasive messages. Where normatively-good argumentative practice interferes with persuasive success, advocates will face the unhappy circumstance of having to choose between being good and being effective. But with respect to conclusion articulation, the research evidence to date suggests that arguers do not face such a choice. On the contrary, better-articulated message conclusions enjoy a dependable persuasive advantage over less-articulated ones.

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