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Message Properties, Mediating States, and Manipulation Checks: Claims, Evidence, and Data Analysis in Experimental Persuasive Message Effects Research

This article addresses the conceptualization and definition of message variables in persuasion effects research. Two central claims are advanced. First, effect-based message variable definitions (in which a message variation is defined in terms of effects on psychological states, as when fear appeal variations are defined on the basis of differences in aroused fear) impede progress in understanding persuasion processes and effects and hence should be avoided in favor of definitions expressed in terms of intrinsic message features. Second, when message variations are defined in terms of intrinsic features, message manipulation checks, under that description, are unnecessary but similar measures may usefully be understood and analyzed as assessments of potential mediating states.

One enduring question in communication research is how and why persuasive messages have the effects they do. But some important conceptual aspects of this subject seem to have suffered from inattention, with resulting needless confusion and impaired research progress. The particular focus of concern in this paper is the set of complex relationships among experimental message variations, message manipulation checks, persuasive outcomes, and mediating states. The purpose is to point to some systematically different sorts of research claims that arise in the context of studying persuasion effects, with an eye to clarifying the different burdens of proof—and corresponding data-analytic treatments—appropriate for each and thereby to untangling some of the complexities and confusions that have arisen in this research domain.

Two central claims will emerge from this analysis: First, effect-based message variable definitions impede progress in understanding persuasion processes and effects and hence should be avoided in favor of definitions expressed in terms of intrinsic message features. Second, when message variations are defined in terms of intrinsic features, message

manipulation checks are unnecessary, but similar measures may usefully be treated as assessments of potential mediating states. Although the overt focus of attention will be persuasion research, the problems and issues encountered appear quite general to message effects research. Thus it is hoped that this treatment may prove helpful in the broader enterprise of developing understandings of the distinctive aspects of message-centered research.

Backdrop: Message Manipulation Check Curiosities

As an entry point to the problems of interest, imagine a study of the following sort. A researcher wants to investigate the persuasive effects of variation in the length of (number of words in) a message. Such a study might be motivated by the possibility that message length could serve as a peripheral cue that engages some heuristic (e.g., “longer messages probably have more or better supporting reasons”). So the researcher constructs two messages that differ in length and, following customary procedures, randomly assigns participants to receive one message or the other. In addition to assessments of persuasive impact, the design also naturally includes a manipulation check, in the form of assessments of participants’ perceptions of message length.

This manipulation check, however, cannot genuinely be an assessment of, or a check on, whether the message property has been properly manipulated. No matter what participants thought about the length of the message that they encountered, the messages did differ in length. The manipulation check might serve some other purpose (more on that shortly) but it cannot actually be a check on the adequacy of the manipulation—whether the message varied in length is not a matter of participant perceptions. After all, if the manipulation check “failed,” that is, if participants did not perceive differences in message length, no one would conclude that the researcher must have miscounted the number of words. So, at least upon reflection, this first example represents a pretty plain mistake. An experimental variation of message length does not need this sort of manipulation check.

Now consider a second study, one in which a researcher wants to investigate the effect of strong versus weak fear appeals on persuasive outcomes. The researcher constructs two messages that differ in the intensity of fear-arousing content. The research design includes a manipulation check in the form of assessments of participants’ aroused fear levels.

The procedure in this second study somehow does not seem quite as problematic as that of the first study; here it seems sensible to see whether the two messages differentially aroused fear. On the other hand, the two

studies do seem parallel—each has a message manipulation and a corresponding manipulation check involving some psychological state of participants. So, if there is a mistake in the first (message length) study, as surely there is, then perhaps there’s a mistake in the second (fear appeal) study as well. That is, the questions raised by these two cases are these: Exactly what mistake, if any, is embodied in the first design? And is there complete parallelism between these two cases, or is there some distinction to be noticed?

On the surface, these questions concern the appropriateness of message variable manipulation checks involving participant psychological states.¹ That question is in fact a convenient starting point for this enterprise, but it will become apparent that a number of larger issues are involved concerning the conceptualization of message properties, mediating states, and persuasive outcomes, and of the relationships among these. The treatment of these matters turns out to have important implications for the design, analysis, and interpretation of persuasion effects research. So, although the following discussion will give attention to the specific question of the appropriateness of message manipulation checks, broader and more significant issues underlie this surface focus.

The relevant issues can most clearly be sorted out by identifying a number of distinct classes of claim (or hypothesis or research question) that arise in the study of persuasive message effects and then considering the appropriate role of manipulation checks—and more broadly the appropriate means of data analysis—within each.

Three Classes of Claims

In studies of the effects of message variations on persuasive outcome variables (e.g., attitude change), three different kinds of research claim can be of interest to theorists and researchers (see Figure 1). One general kind of claim (labeled a Class I research claim in Figure 1) concerns the relationship of a psychological state to a persuasive outcome; a second kind of claim (a Class II claim) concerns the effect of a message variation on persuasive outcomes; and a third category (Class III claims) concerns both the effect of a message variation on persuasive outcomes and the potential mediating role (in such effects) of a psychological state.

Psychological States and Outcomes

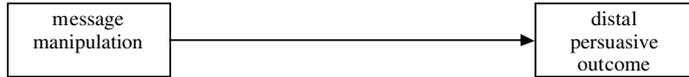
The Research Claim. One class of research claim concerns the relationship of a psychological state to a persuasive outcome. As a concrete example, in research on empathy-based persuasive appeals, a researcher might study the relationship between aroused empathy and message recommendation acceptance (e.g., in charitable advertisements). One straightforward way of investigating this relationship would have the

Figure 1.
Three
Classes of
Research
Claim
Concerning
Persuasive
Message
Effects

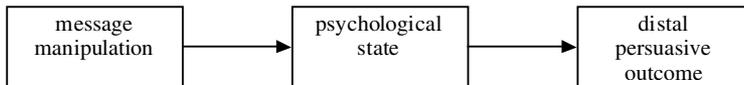
Class I research claim: Relationship of a psychological state and persuasive outcomes



Class II research claim: Effect of a message variation on persuasive outcomes



Class III research claim: (a) Effect of a message variation on persuasive outcomes and (b) potential explanation via mediating psychological state



researcher create two different messages, a high-empathy message and a low-empathy message, and then assess (postexposure) the degree of aroused empathy and the degree of persuasive success. The key question in such a design is whether greater aroused empathy is associated with greater persuasive success.

Message Manipulation Checks. In such a study, the message variation is simply a device to create variance in aroused empathy. If there is no variance in aroused empathy, then it will be impossible to assess the covariation between aroused empathy and persuasiveness, so some means must be found of producing such variance. Such a design will of course include an assessment of aroused empathy. (If there were not such an assessment, one could not examine the relationship between aroused empathy and persuasive success.) It would be wrong, however, to think of this assessment as a “check” on the message manipulation. The assessment might be described as providing a check on the manipulation of aroused empathy—in the form of reassurance that there was in fact variance in aroused empathy—but the message variation itself needs no check.

To see this point clearly, consider that if the effect of the message variation on aroused empathy were the opposite of that expected (i.e., if the low-empathy message created significantly more empathy than the high-empathy message), the message variation would still have fulfilled its purpose. In this sort of study, the relationship of interest is that be-

tween aroused empathy and persuasive effects, and the purpose of the experimental message manipulation is simply to create variance in aroused empathy.

To put the matter more abstractly: In this first class of research claim, the relationship of interest is that between a psychological state and a persuasive outcome.² Variation in the psychological state of interest is created by exposure to different message conditions, but the message variations are not themselves of interest; they are simply a methodological device for creating variance in the psychological state. (If there were no variance in the psychological state, then it would be impossible to assess that state's covariation with the outcome.) In such a design there is no need for a message manipulation check. One might want to be reassured that there was in fact variation in the relevant psychological state and so check to ensure that such variance was created, but this would not appropriately be described as a check on the message manipulation itself.

Data Analysis. Research questions in this first class are, speaking generally, most appropriately addressed through data-analytic procedures that concern specifically the relationship of the psychological state to the outcome. So, for example, it generally would not be appropriate to use the different message-manipulation conditions as representing different levels of an independent variable (e.g., as a factor in an ANOVA); rather, the data analysis should directly examine the relationship between the relevant psychological state and the outcome of interest.

To concretize the possible misstep here, consider again the hypothetical empathy-and-persuasion study discussed previously. Imagine that the two message conditions, high-empathy appeal and low-empathy appeal, produced the expected variation in aroused empathy, so that persons exposed to the high-empathy message evinced greater aroused empathy than did persons exposed to the low-empathy message. To address the question of the relationship of aroused empathy to persuasive outcomes—the kind of claim of interest in this first class of research claims—the most useful evidence would not come from a comparison of the persuasive effectiveness of the two messages. The most appropriate analysis would examine directly the relationship between aroused empathy and persuasive effects, ignoring the message conditions.³

Illumination of Communication. Studies of this first class of research question do not offer as much illumination of communication processes as one might like. There need be no careful attention given to the relationship between message variation and variation in the psychological state, because the fundamental interest is not in seeing how messages influence the psychological state. Once the messages have been used to

create the desired variance in the psychological state, the researcher is no longer interested in the message manipulation because it was only a methodological device for inducing psychological-state variation, not something of independent interest. Because the message manipulation is not of independent interest, it is not systematically theorized, making it difficult to learn much about how message properties are related to effects.

Imagine, for example, that in the hypothetical empathy-study example, aroused empathy was found to be significantly related to persuasive success: As receivers felt more empathy, they were correspondingly more persuaded. Plainly this would suggest that empathy arousal might serve as a mechanism of influence. One would not yet have learned much, though, about exactly how to arouse empathy—that is, about which particular message variations might lead to corresponding variations in aroused empathy. In this first class of research claim, the focus is the relationship between the psychological state (aroused empathy, in this example) and the outcome (persuasive success)—not the properties of the message. Thus investigations of this first sort of claim shed rather less light than one might like on communication.

As an illustration of this shortcoming, consider Skumanich and Kintsfather's (1996) study concerning how to persuade people to sign organ-donor cards. The central focus of interest was the set of relationships between, on the one hand, empathy arousal, issue involvement, and values (the psychological states of interest) and, on the other hand, attitudes and behavioral intentions concerning signing organ-donor cards (the outcome variables). The design contained a message variation (presence or absence of an "empathy arousal cue"), but this was ignored in the data analysis—and appropriately so, given the research's focal interest. The message variation was apparently meant simply to produce some variability in empathy arousal; for example, the reported data analysis addressed the relationship between aroused empathy and attitudes, but ignored the message variation.

Such research cannot offer much illumination of the role of message variations in persuasion. In Skumanich and Kintsfather's (1996) report, little attention was given to describing the message variation, and the reported data analysis did not address whether the message variation actually influenced empathy arousal (or attitudes or intentions). Hence no matter what the findings might reveal about the relationship of psychological states (such as empathy arousal) to persuasive outcomes (willingness to sign an organ-donor card), these findings can give little help to message designers and can provide no information for those interested in understanding or theorizing about how message variations influence persuasive effects.

Message Variations and Outcomes

The Research Claim. A second class of research claim concerns the impact of a message variation on persuasive outcomes. For example, a researcher might examine the effect on persuasion of including an introductory metaphor in the message. The research question would be whether messages containing such a metaphor would be more persuasive than messages without one.

Message Manipulation Checks. For addressing this research question, there is no need to check the manipulation of metaphor inclusion by assessing participant perceptions. The messages either did or did not contain an introductory metaphor, quite independent of participant perceptions. Put abstractly, when the research question concerns the effect of a message variation on a persuasive outcome, no message manipulation check is required. The investigator will naturally want to be careful in creating the experimental messages, but the adequacy of the manipulation of the message property is not appropriately assessed by inquiring about participant perceptions of the message.

Data Analysis. Data analysis for this second class of claim is straightforward. One simply compares the effect of the different message conditions on the outcome variable(s). For example, Mitchell, Badzinski, and Pawlowski (1994) examined the degree to which metaphorical and non-metaphorical advertisements produced differences in brand name recall.

Illumination of Communication. Research addressing this second class of research claim obviously does shed light on the role of message variations in persuasion. If, for example, it turned out that messages with introductory metaphors were generally more persuasive than their nonmetaphorical counterparts, message designers would have a useful general principle to apply, and theorists of persuasion effects would have a phenomenon to be explored—with moderating factors to be identified, alternative explanations to be tested, and so forth.

Message Variations, Mediating States, and Outcomes

The Research Claim. A third class of research claim concerns jointly (a) the impact of a message variation on an outcome and (b) a hypothesized explanatory mechanism involving some mediating psychological state; in a sense, this represents the conjunction of instances of the first two classes. As a concrete example, consider the circumstance of a researcher who wishes to examine the effects on recommendation acceptance of variation in citation of sources of evidence for the message's claims about the consequences of the advocated policy. That is, the message variation is whether the message explicitly identifies the sources of the information it offers concerning its claims about policy consequences. The researcher's expectation is that such effects are mediated by perceived

likelihood of the policy consequences (i.e., mediated by the receiver's perception of the likelihood that the various claimed consequences would actually occur). So the research hypothesis is that variations in information source citation make for variations in perceived likelihood of outcomes, which in turn influence recommendation acceptance. To address these questions, the researcher will need to assess the mediating state (perceived likelihood of outcomes) and the persuasive outcome of interest (recommendation acceptance) in order to see whether the message variation (information source citation) influences the mediating state and whether the mediating state influences the outcome variable.

Message Manipulation Checks. For addressing these sorts of research questions, there is no need for a message manipulation check. To continue the example, it is unnecessary to check the adequacy of the manipulation of information source citation by assessing participant perceptions (e.g., participant perceptions of information source citation)—the messages either did or did not differ in their citation of information sources quite independent of participant perceptions.

Put more generally: When the research claim involves the mediated impact of a message variation on an outcome, no message manipulation check is required. The investigator will need to assess the mediating state and the outcome of interest and will need to take care to ensure that the message variation has been experimentally realized in the desired way. As with the second class of claims, however, the adequacy of the manipulation of the message property is not appropriately assessed by inquiring about participant message perceptions or psychological states.

Data Analysis. Because research claims of this third kind represent a conjunction of the first two kinds of claims, the relevant data analysis involves a corresponding conjunction. That is, an investigator will (a) examine the impact of the message variation on the mediating state, (b) examine the relationship between the mediating state and the distal persuasive outcome variable, and (c) consider whether the impact of the message variation on the persuasive outcome is mediated by the hypothesized mediating state. (For some discussion of statistical analyses appropriate to such claims, see Baron & Kenny, 1986; Kenny, Kashy, & Bolger, 1998, pp. 258–263; Shrout & Bolger, 2002.)

Illumination of Communication. Research examining this last class of claim obviously can provide considerable illumination of communication processes. In particular, such research can go beyond the insight provided by studies of the second class of claim by examining the potential role of a specified mediating state. Such examination provides information useful in considering possible explanations for any observed effects of message variations on outcomes.

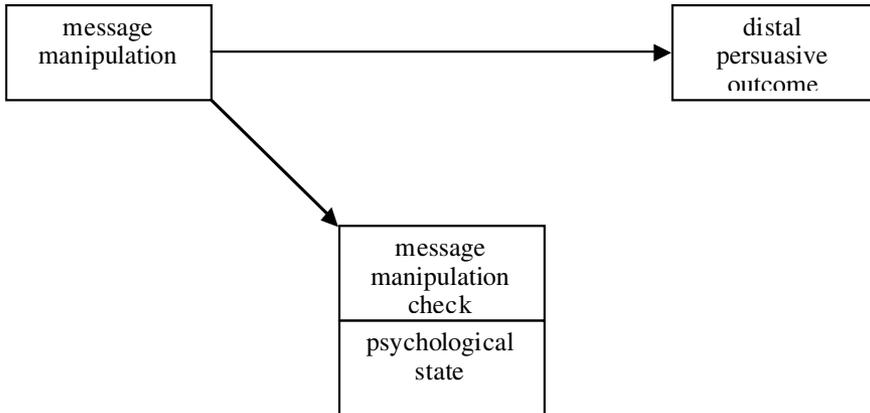


Figure 2. Psychological-State Assessments Treated as Message Manipulation Checks

Confusions Among Claims

These three different kinds of research claim are plainly distinct and correspondingly invoke different argumentative burdens and different data-analytic strategies. Even so, persuasion researchers have not always appreciated these distinctions, with unhappy consequences. Two areas of persuasion research—guilt appeals and protection motivation theory—provide convenient examples.

Guilt Appeal Research. A number of studies have examined the persuasiveness of guilt-based persuasive appeals (see O’Keefe, 2000, 2002). Broadly put, this research is motivated by questions about whether and how guilt appeal messages persuade. A common research format in these studies involves the creation of a guilt appeal message variation (e.g., a high-guilt message and a low-guilt message) and the postexposure assessment of both aroused guilt and persuasive effects. But the assessment of guilt is typically employed as a check on the message manipulation, not as an assessment of a possible mediating state. For example, research reports commonly indicate the relationship between the message variation and aroused guilt (this reported as a manipulation check, meant to confirm that the different messages aroused different levels of guilt) and the relationship between the message variation and persuasive outcomes—but do not report the relationship between aroused guilt and persuasive outcomes (e.g., Bozinoff & Ghingold, 1983; Dembroski & Pennebaker, 1972; Zemach, 1966; see Figure 2 for a representation of this practice.)

Plainly, this research practice means that rather less is known than might be about exactly how guilt appeals work. In particular, this way

of proceeding obscures the causal pathways involved in guilt appeal effects, not because the relevant data were not collected but because researchers have not analyzed extant data in ways that shed light on these questions. Even though the relationship between aroused guilt and persuasive outcomes is of obvious interest, nevertheless—and rather surprisingly—research reports commonly fail to examine it.

Protection Motivation Theory. Research on protection motivation theory (Rogers, 1983; Rogers & Prentice-Dunn, 1997) exhibits data-analytic practices precisely parallel to those in guilt appeal research. That is, researchers collect data about potential mediating states, but then treat such data as providing nothing more than message manipulation checks and hence do not report data analyses in ways that illuminate underlying processes.

Protection motivation theory (PMT) aims at explaining persons' motivations to undertake protective actions (e.g., health-protective actions such as adopting an exercise program). PMT identifies a number of perceptual states (such as perceived threat severity, perceived threat vulnerability, perceived response efficacy, and perceived self-efficacy) as influences on decisions about adopting protective behaviors. PMT research thus commonly involves the creation of message variations aimed at influencing such perceptual states (e.g., messages that vary in their depiction of the severity of the threat) and the postexposure assessment of both the perceptual state and persuasive outcomes. Parallel to data-analytic practices common in guilt appeal research, however, the perceptual-state assessment is often treated simply as a message manipulation check. That is, researchers commonly report the relationship between the message variation and the perceptual state (in the form of a reported manipulation check, meant to confirm that the different messages aroused different levels of the perceptual state) and the relationship between the message variation and persuasive outcomes, but fail to report the relationship between the perceptual state and persuasive outcomes.

The commonality of this way of analyzing data can be seen quite clearly in meta-analytic reviews of research relevant to PMT. These reviews report substantial numbers of studies examining the relationship of experimental message variations to various PMT-relevant perceptual states (e.g., for different perceptual states, between 24 and 33 cases were reviewed by Witte & Allen, 2000) and substantial numbers of studies examining the relationship of experimental message variations to persuasive outcomes (for different variations, between 21 and 41 cases were reviewed by Floyd, Prentice-Dunn, & Rogers, 2000, and between 40 and 56 cases by Witte & Allen, 2000). Noticeably fewer studies, however, have provided information about the relationship of PMT's perceptual states to persuasive outcomes (for different percep-

tual states, between 10 and 25 cases were reviewed by Milne, Sheeran, & Orbell, 2000). In fact, neither Floyd et al.'s (2000) review nor Witte and Allen's (2002) review even reported an analysis of studies of the relationship between PMT perceptual states and persuasive outcomes. In other words, although researchers have collected data concerning both PMT perceptual states and persuasive outcomes, they commonly have not provided information about the relationships between these.

Plainly, this research practice means that rather less is known than might be about exactly how protection motivation processes work. In particular, this practice obscures the causal pathways involved in PMT message effects—not because the relevant data were not collected but because researchers have not analyzed extant data in ways that shed light on these questions. Even though the relationship between PMT-specified perceptual states (perceived vulnerability, perceived response efficacy, and so on) and persuasive outcomes is of obvious interest, nevertheless research reports commonly, and surprisingly, do not examine it despite having the relevant data in hand.

Explaining These Practices. What accounts for these curious data-analytic practices in guilt appeal and PMT research? In both research areas, investigators have gathered research data that would permit examination of the relationships among message variations (guilt appeal and PMT message variations), potential mediating states (aroused guilt, perceived vulnerability, and so forth), and persuasive effects (attitude, intention, and so on), and yet these investigators have not exploited the data as fully as they might have. In particular, they have overlooked examination of the relationship between a potential mediating state, such as aroused guilt or perceived vulnerability, and persuasive outcomes. It cannot be that this relationship is of no interest to these researchers. After all, research on guilt appeals and PMT is stimulated precisely by the possibility that the arousal of these states (guilt, perceived vulnerability, etc.) may be a means of motivating message acceptance. So why don't researchers routinely report this relationship?

Surely one is entitled to suspect that at least part of the answer is to be found in conventional training in statistical analysis of experimental data (e.g., ANOVA). The relationship between a possible mediating state and an outcome does not have a comfortable place in the standard statistical analysis of experimental data. A canonical ANOVA design has independent variables (causes, represented as factors in a factorial design) and dependent variables (effects), and the statistical analysis is meant to assess the relationship between these. A mediating state complicates the picture because it is at once an effect and a cause. But the mediating state's causal role is not easily examined within an ANOVA framework because that state is not represented as an independent vari-

able (the mediating state is not a factor in the factorial)—and hence the relationship between the mediating state and the outcome is easily ignored. Thus, as seen in guilt appeal and PMT research, the message variation (the independent variable) is first examined for its effect on the mediating state (this being described as a manipulation check) and then for its effect on the outcome variable, while the relationship between the mediating state and the outcome goes unexamined (see Figure 2).

So perhaps because of conventional statistical training, a misunderstanding of the evidentiary role of manipulation-check data, or an unconsidered belief that a manipulation check is automatically required for every experimental manipulation, some persuasion researchers have plainly been thinking about their message manipulation check assessments in unhelpful ways. They could, and should, have been thinking about those assessments as potentially providing information about mediating states. Instead, they treated those assessments simply as manipulation checks—and so, for instance, disregarded those data after the data had been used to show that the message manipulation had the desired effects on the relevant psychological state.

The Argument Thus Far

To sum up the argument to this point: There are various distinguishable research claims that arise in the study of persuasive message effects, with correspondingly different appropriate data-analytic procedures. Researchers have often been inattentive to these differences, however, and consequently research progress has been unnecessarily impeded. One symptom of the problem can be briefly described as a misuse of message manipulation check assessments. Such assessments are actually not satisfactorily understood as assessments of the adequacy of the manipulation of message variables, but commonly are better seen as potential assessments of states that might mediate persuasive effects.

Indeed, it should now be apparent that the practice of routinely, much less automatically, including a message manipulation check in persuasion message effects research is not well-founded.⁴ As an illustration, consider Artz and Tybout's (1999) study of, *inter alia*, the effects of variation in the use of quantification in persuasive messages—the contrast between, for example, “most” and “75%.” The research report describes a corresponding “manipulation check on message format” (p. 55) in the form of a seven-item scale to assess quantitiveness—that is, participants' perceptions of message quantitiveness were assessed and reported as a check on the manipulation—but of course this assessment is not genuinely a check on whether the experimental messages varied in quantification. Obviously they did, no matter the state of participant perceptions. This assessment might provide a basis for testing ideas about the role of perceived quantitiveness in persuasion effects, but it is not

appropriately described or analyzed as a check on the message variable manipulation.

In fact, one implication of the argument thus far is that researchers should never report a message manipulation check—at least not under that description.⁵ The reason is that anything reported as a message manipulation check is actually at best an assessment of a potential mediating state and hence should be analyzed and reported accordingly. For example, the relationship between the mediating state and the persuasion outcome variable should be reported.⁶ This conclusion arguably overlooks an important complexity in the conceptualization of some message variables, namely, that some message variations are defined in terms of their effects—which would seem to necessitate having (the right sort of) message manipulation checks. The next section discusses such effect-based characterizations of message variables.

Effect-Based Message Variable Definitions

A great many message variables can be described as based on an intrinsic property of the message, such as the presence or absence of some message feature (an introductory metaphor, an explicit conclusion, acknowledgment of counterarguments, and so on). Some message variables, though, have been defined not in terms of intrinsic message properties but rather in terms of some effect of the message variation. Here I argue that such effect-based message variable definitions impede progress in understanding persuasion and should be avoided. The argument examines three examples of effect-based variables: vividness, fear appeals, and argument quality.

Message Vividness

A number of studies have addressed the question of whether, or under what circumstances, vivid messages are more persuasive than nonvivid messages (e.g., Collins, Taylor, Wood, & Thompson, 1988; Frey & Eagly, 1993; Taylor & Thompson, 1982). This research area actually includes a number of different conceptual treatments of vividness—more than can be sorted out here. For present purposes, it will be sufficient to notice that some definitions of message vividness have at least implicitly involved certain affiliated effects, specifically effects on perceived vividness. When vividness is conceptualized along these lines, in order to actually be vivid a message must be perceived as being vivid. From this standpoint, it is definitionally part of the concept of a vivid message that the message has certain (perceptual) effects, namely, that it is perceived as vivid.

Naturally, this way of conceiving of message vividness variations requires a check of the message manipulation. Any study comparing the

persuasiveness of vivid and nonvivid messages, thusly defined, will necessarily have to include an assessment to confirm that the messages vary appropriately in perceived vividness. If by definition a vivid message is one perceived as relatively more vivid than a nonvivid message, then to see whether one has successfully created vivid and nonvivid messages one needs to see whether the required differences in perceived vividness obtain. (For examples of this way of conceptualizing message vividness variations, with attendant message manipulation checks, see Keller & Block, 1997; Kelley, 1989; McGill & Anand, 1989; Twible & Hensel, 1991.)

An effect-based definition of message vividness variations, however, is in the end undesirable, precisely because it avoids analysis of intrinsic message features. To illustrate, suppose that greater perceived vividness was found generally to be associated with greater persuasion. In such a circumstance, message designers would want to know how to produce a message that is perceived as vivid (a “high vividness” message), but with an effect-based definition of vividness, message designers get no guidance from vividness research. Because there is no effect-independent conception of the properties of a vivid message, no good advice can be given about how to design such a message. Vividness researchers would be reduced to telling message designers, “You should create a message that is perceived as vivid—but we don’t know what sort of message that is.”

To express this point more generally: When message variables are defined in terms of their effects, the kinds of research claims that can be studied are claims of the general form “messages that produce effect A are likely to also produce effect B.” For example, where message vividness variations are defined in terms of perceived vividness, research on the persuasive effects of message vividness variation amounts to studies of the hypothesis, “messages that produce the effect of being perceived as vivid are likely also to produce the effect of being persuasive.” Such research evades the task of describing how to bring about such effects, that is, the task of identifying the message characteristics responsible for the effects.

In fact, the use of effect-based definitions of message variations has the consequence of transforming such research into research addressing the first class of claims distinguished earlier, claims concerning the relationship of a psychological state to a persuasive outcome. (In research using effect-based definitions of vividness variations, the psychological state is perceived vividness; its relationship to persuasive outcomes is examined by comparing persuasive effects of messages perceived as relatively vivid or nonvivid.) This transformation is disguised because such research is commonly described as though it were an examination of the effects of a message variable (message vividness) on persuasive outcomes

(the second class of research claim distinguished above). But when the message variation is defined in terms of a psychological state, as when the vividness variation is defined in terms of its effects on perceived vividness as opposed to being defined in terms of intrinsic message characteristics, the research design in practice amounts to an examination of the relationship between a psychological state (the state used in the effect-based definition of the message variation) and the persuasive outcome.⁷

That is to say, the practice of defining a message variation such as vividness in terms of effects is tantamount to abandoning claims about the relationship between message features (intrinsic, effect-independent, message features) and persuasive outcomes in preference to focusing on the relationship between psychological states and persuasive outcomes. As suggested earlier, such research sheds little light on communication processes and effects.

Fear Appeals

Some definitions of fear appeal variations, and in particular the broad contrast between “strong” (“high”) and “weak” (“low”) fear appeals, have relied on effect-based definitions of that message variation. When approached in this way, a strong fear appeal message is defined as one that evokes relatively greater fear than does a weak fear appeal message.

Obviously, any study of the persuasiveness of strong versus weak fear appeals, thusly defined, will necessarily have to involve a check of the fear appeal manipulation, that is, an assessment that confirms that the various experimental messages vary in the degree of fear aroused. If by definition a strong fear appeal message is one that arouses greater fear than does a weak fear appeal message, then to see whether one has successfully manipulated fear appeal levels, one must see whether the various experimental messages produced the expected differences in aroused fear. (For examples of this way of conceptualizing fear appeal message variations, with attendant message manipulation checks, see Beck & Davis, 1978; Chebat, Laroche, Badura, & Filiatrault, 1995; Hewgill & Miller, 1965; Hill & Gardner, 1980; Keller & Block, 1996; Krisher, Darley, & Darley, 1973; Stout & Segó, 1994.)

Parallel to the case of message vividness, however, an effect-based definition of fear appeal variations is, in the end, undesirable, precisely because it avoids analysis of intrinsic message features and so makes fear appeal research shed little light on communication processes. To illustrate this problem, suppose that greater aroused fear were found to generally be associated with greater persuasion. In such a circumstance, message designers would want to know how to produce a message that arouses fear (a strong fear appeal message). With an effect-based definition of fear appeal messages, however, message designers get no guidance

from fear appeal research. Because there is no effect-independent conception of the properties of a strong fear appeal message, no good advice can be given about how to design such a message. Fear appeal researchers would be reduced to telling message designers, “You should create a message that arouses a lot of fear, but we don’t know what sort of message does that.”

Thus, when fear appeal message variations are defined in terms of aroused fear, research on the persuasive effects of fear appeal variations amounts to studies of the hypothesis “messages that produce the effect of arousing fear are likely also to produce the effect of being persuasive,” evading the task of identifying the message characteristics responsible for the effects. Indeed, just as with vividness, when the fear appeal message variation is defined in terms of its effects on aroused fear, as opposed to being defined in terms of intrinsic message characteristics, the research design in practice amounts to an examination of the relationship between a psychological state—fear, the state used in the effect-based definition of the message variation—and the persuasive outcome. That is, the practice of defining fear appeal message variations in terms of aroused fear is tantamount to abandoning claims about the relationship between message features (intrinsic, effect-independent, message features) and persuasive outcomes.

It will be noticed that these effect-based definitions of fear appeal variations involve something of a complexity (compared to the case of vividness), because the psychological state used in the definition of the message variable happens (also) to be a state that might plausibly be supposed to mediate the effects of messages on the eventual outcome of interest. That is to say, differences in aroused fear might be supposed to play some fairly direct causal role in influencing persuasive outcomes. Indeed, part of the rationale for fear appeal research is precisely the possibility that fear arousal is causally related to persuasive success. By contrast, defining message vividness variations by variations in perceived vividness invokes a psychological state that is not quite so readily seen in such a role. (The point here is not that it would be impossible to construct an account in which perceived vividness played some causal role in influencing persuasive outcomes, only that it is easier to envision such a role for aroused fear.) But whether the effect-based definition involves effects on a mediating state or on some other psychological state, the upshot is the same: Employing such effect-based definitions of message variables produces research that sheds little light on the role of message features in persuasion.

Argument Quality

One final example of a message variable whose definition is effect-based—argument quality—is also worthy of attention, especially because of its

prominence in research on the elaboration likelihood model (ELM) of persuasion (Petty & Cacioppo, 1986; Petty & Wegener, 1999).

The ELM offers a broad distinction between two different general routes to persuasion. Which process is activated is seen to depend on the degree of issue-relevant thinking (“elaboration”) engaged in by receivers. When receivers do not undertake much elaboration, peripheral routes to persuasion are engaged. Peripheral-route persuasion commonly involves the use of simplifying heuristic decision principles based on, for instance, the communicator’s apparent expertise. By contrast, when receivers engage in relatively extensive elaboration, central-route persuasion processes are activated in which receivers more closely scrutinize the message’s arguments and the outcome of persuasive efforts is seen to depend on the positivity of the receiver’s thoughts about the advocated view. Hence, argument quality variations have a significant place in the ELM because as elaboration increases, persuasive outcomes are seen to depend increasingly on the strength of the message’s arguments—stronger arguments will, when scrutinized, produce more positive thoughts.

In ELM research, however, argument quality variations are defined empirically, on the basis of observed effects under conditions of high elaboration. A strong-argument message is defined as “one containing arguments such that when subjects are instructed to think about the message, the thoughts that they generate are predominantly favorable.” A weak-argument message is defined as one in which the arguments “are such that when subjects are instructed to think about them, the thoughts that they generate are predominantly unfavorable” (Petty & Cacioppo, 1986, p. 32). There is also a corresponding ELM-recommended procedure for identifying strong and weak arguments. A pool of possible arguments is initially screened by being rated for persuasiveness by a group of pretest participants. Then messages composed of high- and low-rated arguments are presented to a second group of pretest participants who report their thoughts while receiving the messages. Messages that evoke predominantly favorable reactions are deemed to contain high-quality arguments; messages evoking predominantly unfavorable reactions are taken to contain low-quality arguments (see Petty & Cacioppo, p. 32).⁸

Obviously this effect-based definition of argument quality variations leaves unexplored the question of the specific message properties that give rise to the observed effects. Because strong-argument and weak-argument messages are defined on the basis of effects, not intrinsic message characteristics, one does not know what message features might be responsible for receivers’ reactions. Thus, parallel to the cases of message vividness and fear appeals, this effect-based definition of argument

quality variations has the undesirable consequence that argument quality research sheds little light on communication processes precisely because that research avoids the analysis of intrinsic message features. Consider the plight of a message designer who wants to know how to construct a message that will be persuasive under conditions of high elaboration (a strong-argument message). Because there is no effect-independent conception of the properties of a strong-argument message, no good advice can be given about how to design such a message. One can tell message designers only, “You should create a message containing arguments that evoke positive reactions from receivers—but we don’t know what sorts of arguments do that.”⁹

Summary

These three examples of effect-based message variable definitions plainly illustrate the problems attendant to such definitions insofar as progress in understanding persuasive communication is concerned. When message variables are defined in terms of effects rather than intrinsic properties, researchers forfeit the ability to speak to questions of the relationship between message properties and persuasive outcomes. Such effect-based definitions may enable one to learn a great deal about the relationship between various psychological states (such as aroused fear or perceived vividness) and persuasive outcomes, but such definitions thwart progress in understanding the role of intrinsic (effect-independent) message properties in persuasion. Obviously, only effect-independent characterizations of message properties permit exploration of the relationship between such message features and direct or mediated persuasive outcomes. Hence, the argument here is that effect-based message variable definitions should be avoided precisely because they impede rather than facilitate an understanding of persuasion processes.

Notice that if researchers avoided effect-based message variable definitions, they would never have need to report anything called a message manipulation check. From my vantage point, a researcher who reports a message manipulation check has probably made one of two mistakes—either the mistake of defining a message variable in terms of effects rather than in terms of intrinsic properties or the mistake of confusing the assessment of a potential mediating state with the description of message properties.

Conclusion

This article’s central argument actually concerns not message manipulation checks per se, but rather the conceptualization and definition of message variables in persuasion effects research. Expressed briefly, the central claims advanced here are (a) Effect-based message variable defi-

nitions impede progress in understanding persuasion processes and effects and hence should be avoided in favor of definitions expressed in terms of intrinsic features; and (b) when message variations are defined in terms of intrinsic features, message manipulation checks, under that description, are unnecessary, but similar measures may usefully be understood and analyzed as assessments of potential mediating states.

If persuasion research and data-analytic practices were informed by these beliefs, those practices would be different in two broad ways. First, researchers would avoid the use of effect-based definitions of message variables. Instead, message variations would be characterized on the basis of effect-independent message features. Second, researchers would no longer treat assessments of psychological states as checks on message manipulations. Instead, such assessments would, when appropriate, be analyzed as potential mediators of persuasive effects.¹⁰ So, for example, fear appeal research would address the question of what message variations produce differences in aroused fear (that is, would speak to the question of what intrinsic message properties are related to fear arousal), and reports of fear appeal studies would routinely provide information about the relationship between aroused fear and persuasive outcomes rather than discarding assessments of fear arousal after they had been used as manipulation-check data.

Thus, my argument is that both conceptual and empirical reformation is needed in persuasion message effects research. The use of effect-based definitions of message variables (the conceptual aspect) and the failure to understand the role of mediating-state assessments (the empirical aspect) have thwarted progress in understanding the effects of message variations. One, however, may see the problems identified here as reflecting a deeper inattention to the intrinsic features of message variations. In a sense, this article aims to point to the curiously undertheorized character of messages in persuasion research.

One way of putting this larger argument is this: Progress in understanding persuasive message effects requires an understanding of persuasive message properties. Assessments of psychological states, reported as message manipulation checks, are no substitute for a careful description of message properties, and effect-defined message variations obviously evade the task of describing message properties. Yet, without effect-independent characterizations of message variations, researchers will be unable to address questions of the relationship between such message properties and persuasive effects, message designers will have little guidance about the construction of effective messages, and theoretical understandings of persuasion will inevitably be stunted.

Because these issues arise most commonly and clearly in the context of persuasion effects research, that research domain has provided a natural

source of examples for this discussion, but similar issues arise whenever the relationships among message properties, mediating states, and outcomes are of interest. Consider, for example, research on social support communication. Social support has been conceptualized in diverse ways, with corresponding diversity in the assessment of social support (see, e.g., Albrecht, Burleson, & Goldsmith, 1994, p. 435), but progress in unpacking social-support communication processes will plainly require clear distinctions among (at least) intrinsic message features (e.g., whether the message explicitly acknowledges the feelings of the distressed other), potential mediating states (e.g., anxiety), and distal outcomes (e.g., on health indices). Or consider experimental research on the effects of variation in exposure to particular mass media contents (thin-ideal body image content, for instance). An effect-independent representation of such content variation will be essential to elucidating the effects of exposure to such media material.

Thus, my hope is that this treatment may prove helpful in the broader enterprise of message-centered research. Studying communication processes and effects can raise subtle methodological issues, and developing understandings of the distinctive aspects of message-centered research is a correspondingly important undertaking (see, e.g., Jackson, 1992). There can be no more central element in message-centered research than attention to the description of messages themselves.

However, as an indication of the complexities that lie ahead, it must also be acknowledged that the present argument invokes a thoroughly unsubtle conceptualization of messages and message properties—unsubtle because the separation of intrinsic message features from recipient responses is not an unproblematic undertaking. This is a more complex matter than can be sorted out here, but one need only consider such properties as “meaning” to sense the tangled questions that are avoided in the present treatment. Indeed, the very idea of a message (much less a message feature) is something of an abstraction—useful, perhaps, but not to be overinterpreted or reified.

Although I am not unmindful of or insensitive to these complexities, at the same time it seems plain that substantial improvements can be made in persuasion research data collection and analysis—improvements that do not depend on a grasp of such subtleties. Leading researchers to a still more sophisticated understanding of the nature of messages is a very desirable goal. The argument here cannot be more than an initial step toward that end, however, because any easy distinction between message features and recipient responses can be no more than—to invoke Wittgenstein’s (1921/1961, 6.54) image—a ladder to be climbed and thrown away.

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¹ "Message manipulation check" will be used here as shorthand for the use of assessments of participant psychological states as indices of the adequacy of experimental message manipulations.

² "Psychological state" is intended broadly, as encompassing emotions, beliefs, and so forth. Exactly what sort of psychological state is relevant depends on the substantive research question at hand.

³ One way of putting this point is to say: There’s no sense using a proxy independent variable (the message manipulation conditions) when the real independent variable of interest (the psychological state) is available.

⁴ As just one example of the commonality of the expectation that research designs should include message manipulation checks, notice that Smith and Shaffer (2000, p. 769) take it for granted that the absence of a message manipulation check is a methodological defect.

⁵ Sigall and Mills (1998) have offered a different argument aimed at showing that manipulation checks may be unnecessary in social-psychological research, but their reasoning appears to be limited to what are here called Class I research claims. The only independent variables of interest to Sigall and Mills are psychological states. In their view, an "independent variable" is always the consequence of some "perceived [experimental] treatment," which in turn is the consequence of the experimental treatment itself (p. 221). So for Sigall and Mills, the purpose of experimental treatments (differing experimental conditions) is the creation of variance in psychological states such as "the perception of whether one is alone or another person is present" or "a communicator’s perceived expertise" (p. 220), and these psychological states are the independent variables of interest. With such research questions, as Sigall and Mills correctly point out, manipulation checks (that is, direct assessment of the psychological states) may not be necessary if no plausible alternative explanations exist for the observed effects of the experimental treatment (no plausible alternative other than the hypothesized psychological state), and if such alternative explanations do exist, those alternatives are not undermined by manipulation-check data. Notably, Sigall and Mills’s analysis does not contemplate a researcher’s interest in the effects of independent variables other than psychological states. In particular, the analysis does not address an interest in understanding the effects of (what Sigall and Mills call) experimental treatments themselves (e.g., message-property variations), as represented by Class II and Class III research claims in Figure 1. It may be that social psychologists have little interest in causal factors other than psychological states, but communication researchers are likely to have a wider ambit.

⁶ It should be emphasized that not everything that might be called a "check" (in a research design) is discussed here. The present argument concerns specifically the use of assessments of psychological states as indices of whether some feature of the message was experimentally manipulated in the desired way (e.g., whether the strength of the fear appeal was different in the strong and weak fear messages). Other kinds of message checks are unobjectionable. For example, in a field study of the effects of a health communication campaign, it will surely be valuable to assess the extent to which the target audience was exposed to the campaign. Assessment of campaign exposure is not a message manipulation check in the sense under discussion here, because such assessments do not speak to the question, "Did the various experimental versions of the message differ in the way intended?" After all, in this hypothetical field study, there might not be any experimental message variation.

To put things a bit differently: Assessments of certain audience states (assessments of the degree to which the audience was exposed to the message, of the degree to which the audience understood the message, and so forth) may provide important evidence bearing on claims about message effects. For example, if the target audience for a health campaign exhibited relevant behavioral change but there was little evidence of campaign exposure, then one might attribute the behavioral change not to the campaign but to other forces. However, assessments of variables such as exposure and comprehension are valuable, not because the assessments provide reassurance that various experimental versions of the message differed in the intended ways, but because they provide information that bears on claims about potential causal processes underlying more distal effects (e.g., information about states that might be seen as preconditions for distal effects).

⁷ Notice that there is actually a subtle difference between this way of proceeding and more straightforward tests of the relationship between a psychological state and an outcome. When researchers

Notes

see clearly that their research question concerns the relationship between a psychological state and an outcome, the researchers can use experimental message variations as a means of creating variance in the psychological state and then disregard the message variation in the statistical analysis, as in the case of the earlier-discussed empathy arousal research by Skumanich & Kintsfather (1996). Indeed, as mentioned earlier, when one's research interest is in the relationship of a psychological state and persuasive outcomes, it would not matter if, in such a design, the message manipulation check "failed" (e.g., if a high-empathy message generated significantly less empathy than a low-empathy message) so long as the message variations accomplished the task of creating variance in the psychological state of interest. But with an effect-based message variable definition in which the message variation is defined in terms of its effects on the psychological state, it is crucial that the message variations create the "correct" levels of the psychological state (that is, it is crucial that the manipulation check be "successful").

⁸ Not all "argument quality" research follows these careful ELM procedures. Some operationalizations of argument quality variations have relied on participant ratings of argument quality (e.g., Andrews & Shimp, 1990; Axsom, Yates, & Chaiken, 1987; DeBono, 1992; Helweg-Larsen & Howell, 2000; Munch & Swasy, 1988). With the use of such ratings, argument quality research takes on an appearance very like that of message vividness research. That is, the investigator creates a message variation—argument quality or vividness—and, postexposure, collects both perceptions of the message property (perceptions of argument quality or message vividness, reported as a manipulation check) and persuasion outcome assessments. The specific message properties that give rise to the affiliated perceptions—perceived argument quality or perceived vividness—are left unexplored because the message variation has, in effect, been defined in terms of effects on perceptual states rather than in terms of intrinsic characteristics.

⁹ It should be acknowledged that ELM theorists have sometimes described the purpose of argument quality manipulations as simply a device "to gauge the extent of message processing by the size of the argument quality effects on attitude (Petty & Wegener, 1999, p. 53)—because only relatively elevated thinking produces differential persuasiveness of messages varying in argument quality—and so have disclaimed an interest in unpacking argument quality manipulations. Such disclaimers do not undermine the force of the present point, namely, that effect-based definitions of argument quality variations impede understanding of persuasion processes.

¹⁰ Importantly, it is not just that researchers would not call such an assessment a message manipulation check. It is that they would not think of it as a message manipulation check. They would think of the assessment as providing information about a potential mediating state and so naturally would analyze their data differently (e.g., by directly examining the relationship between that state and persuasive outcomes). Moreover, and correlatively, they would recognize the necessity for an independent feature-based characterization of the message variation. The necessity for effect-independent characterizations of message variations can be obscured by the presence of something called a "message manipulation check" because of the natural supposition that a successful manipulation check somehow ensures the appropriateness or validity of the experimental procedure. That is, reliance on message manipulation checks disguises the undertheorized character of messages.

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