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On Generalizing Cabbages, Messages, Kings, and Several Other Things: The Virtues of Multiplicity

James J. Bradac

The spirit informing Jackson and Jacobs' "Generalizing About Messages" can be entitled "Willful Multiplicity." This vital principle rejects the possibility of attaining the logical positivistic goal of discovering a single best (most accurate) case (theory, method, measure, message, or whatever), arguing instead for diverse replications designed to refute canonical knowledge (Cook & Campbell, 1979; Popper, 1963). From this perspective, knowledge claims that survive a multifarious onslaught are viewed as less erroneous than those that perish with ostensibly irrelevant variations in conception or procedure. I embrace willful multiplicity and I am therefore responsive to Jackson and Jacobs' arguments and suggestions regarding the use of multiple messages. In particular I also worry about the problem of

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generalizing from linguistic materials, the problem that motivates the "language-as-fixed-effect" argument (Clark, 1973). In a recent study I and my co-researchers produced two sets of sentences that were identical from the standpoint of linguistic theory (Bradac, Martin, Elliott, & Tardy, 1980). The sets of sentences were randomly assigned to respondents. Yet a Q analysis revealed that sentence set was a primary source of variation in respondents' judgments. Apparently, these sets of sentences represented different populations despite our best efforts to equate them.

Nonetheless, I must note that the transcendent influence of willful multiplicity offers choices that are in some cases at odds with those offered by Jackson and Jacobs. These theorists do not take their own excellent logic—the logic of willful multiplicity—as far as it will go. In some cases it will actually be desirable to use single messages in experiments. This claim, which I will argue for below, is an alternative to Jackson and Jacobs' suggestions that multiple messages should be included in studies of message effects and that these messages' idiosyncracies should be examined with a conservative random-effects statistical model (Clark, 1973). From my standpoint these suggestions are more controversial than their suggestion that we should examine the independent effects of multiple messages when multiple messages are, in fact, used in a single design. Before I develop the argument regarding the potential desirability of single-message designs, I want to discuss two mistakes I see in the Jackson-Jacobs paper. These seem to me to be more than niggling objections.[1]

THE LANGUAGE (?)-AS-FIXED-EFFECT FALLACY

Following Clark (1973), Jackson and Jacobs worry about the problem of generalizing from linguistic materials. But, as they imply in a note, the problem may be misconceived—it may be a much more general sort of problem. Any social stimulus used in any experiment exhibits theoretically irrelevant attributes. For example, in a set of bearded (versus nonbearded) faces used in a study of impression formation, nose length is confounded with beardedness (Berger & Bradac, forthcoming). This may be an obstacle if we want to generalize beyond the particular set of faces, which we often do. The answer is replication of faces not only within but also across studies. The problem is even more general than this—any experimental stimulus, whether social or not, exhibits theoretical irrelevancies. In studies of the effects of lighting on chicken embryos (Hardy, Harvie, & Koestler, 1975), each instance of the light's being on (or off) is confounded with an infinite number of factors, some of which may matter (bulb temperature) from the standpoint of a particular criterion (hatching). However, it seems in practice that researchers do not worry about the generalizability of some classes of particulars; for example, one puff of air blown into an eyeball in a conditioning experiment is thought to be the same as another puff and both puffing instances are thought to be reasonable members of the class "noxious stimulus." Common sense may force us to attend to certain classes of particulars when we are worrying about generalizability—there are obviously a lot of different words—when really the problem is a universal one of discovering interesting sources of variation vis-à-vis criteria of interest. Generalization of message effects is not a unique sort of problem.

THE REPLICABILITY OF PREVIOUS LANGUAGE EFFECTS STUDIES

Jackson and Jacobs are bothered by inconsistent findings of message-effects research. Such contradictions are evidence of the need to worry about the idiosyncratic effects of particular messages. I think the evidence for the claim of inconsistency is rather fragile. The variable given the most attention in the discussion of inconsistency is language intensity. And, indeed, with regard to the criterion of opinion change, this variable appears rather capricious in its production of effects. A related variable Jackson and Jacobs discuss is power of speech style, and they point to the inconsistent results of two studies of the effects of power that Hemphill, Tardy, and this author conducted (1981). It is important to note that the two studies differed in communication context—in one case a

single message ostensibly produced by a plaintiff or a defendant was read by subjects, whereas in the other case subjects read two messages ostensibly produced by the plaintiff and the defendant. Power of speech style may interact with the situational variable "prior message context." The different results of the two studies may reflect this interaction rather than indicating idiosyncratic effects of the two messages representing the construct "power." Also, this inconsistency, whatever its source, appeared for particular outcomes, for example, judgments of communicator internality and responsibility. With regard to other dependent variables, experiments on the effects of power of speech style have yielded 100% agreement (to my knowledge). Specifically, positive relationships between power and perceived communicator attractiveness and between power and perceived communicator competence have been obtained in all cases (Bradac, Hemphill, & Tardy, 1981; Erickson, Lind, Johnson, & O'Barr, 1978).

Other language variables not discussed by Jackson and Jacobs have yielded highly consistent outcomes. Six studies have shown that lexical diversity is directly related to judgments of communicator competence. This consistency has been obtained in replications crossing particular speakers, speaker genders, topics, presentational media, message types (public versus interpersonal), and geographical regions (Bradac, Bowers, & Courtright, 1979; Bradac, Courtright, & Bowers, 1980). At least five studies have shown that linguistic immediacy is directly related to subjects' perceptions of the extent to which communicators like the things they are talking about (Bradac, Bowers, & Courtright, 1979). (This result may not be very interesting, but it certainly seems stable). Four studies of the "gender-linked language effect" have shown that discourse produced by males tends to be rated high in dynamism, whereas discourse produced by females tends to be rated high in aesthetic quality (Mulac & Lundell, 1980a; Mulac & Lundell, 1980b; Mulac & Lundell, 1981; Mulac & Rudd, 1977). This outcome has been obtained in replications crossing encoding conditions (writing versus speaking; elicited versus extemporaneous production), geographical region and age of speakers, and age of raters. Many studies have shown that communicators using nonstandard dialects tend to be rated low in prestige and intellectual abilities, and, contrarily, they tend to be rated high in agreeableness, friendliness, sociability, and related attributes (Giles, Conway, & Wilson, 1981; Giles & Powesland, 1975; Ryan, 1979). This pattern has been obtained in several countries with male and female speakers of various languages.

Thus, some language variables have, in fact, affected some measures very consistently across studies.

THE LOGIC OF WILLFUL MULTIPLICITY SCRUTINIZED AND EXTENDED

Regardless of the uniqueness of message effects and the stability of previous results, one could still entertain the idea of including multiple messages in the designs of future research. Perhaps, after all, the generalizability of language materials is not uniquely problematic, and perhaps results have not been all that inconsistent across studies; still, we can increase the validity of our knowlege of message effects by incorporating multiple messages in our experimental designs, can we not? I think the answer is: not necessarily. I now want to consider circumstances in which the researcher might be advised to use a single message in a particular study. These circumstances and their implications for message selection grow out of some assumptions about the research process that I now make explicit:

1. Researchers have limited resources (most notably time and access to respondents).
2. This necessitates trade-offs: "Since some trade-offs are inevitable, we think it unrealistic to expect that a single piece of research will effectively answer all of the validity questions surrounding even the simplest causal relationship" (Cook & Campbell, 1979, p. 83). The question is: What do we trade off and when?—not: Do we trade off or not?
3. Trade-offs involve us in dilemmas. We must choose to emphasize one part of the problem at the expense of another. The part we emphasize will have (at best) explanatory virtues that are

different from the virtues of the part we ignore. As McGrath puts it, "You can reduce noise, by cutting scope; so you can learn more about less. Or, you can leave scope broad, by accepting noise along with signal; in which case you can learn less about more. At the limit if you constrain scope by manipulating and controlling more and more variables... you will... be able to learn everything about nothing.... At the other limit, you can constrain less and less... to maximize scope.... Here... you can learn... little or nothing about everything" (1981, p. 199).

4. In a given case, we must creatively and rationally choose the horn of the dilemma upon which we will impale ourselves. This choice will probably reflect our perception of the state of the art.

Thus, the researcher must decide whether to emphasize the construct validity advantages that may come with the inclusion of multiple messages or to: (1) emphasize the construct validity advantages that may come with including multiple measures representing a single dependent variable, (2) emphasize the "statistical conclusion validity" advantages and internal validity advantages that may come with restricting both messages and measures to a single instance (Cook & Campbell, 1979), (3) emphasize the "ecological validity" advantages that may come from exposing different kinds of persons to a single message and a single measure, perhaps in diverse settings (Bracht & Glass, 1968), or (4) emphasize the theoretical advantages that may come from manipulating other independent variables, for example, situational differences, in conjunction with a single message. Recognizing the reality of trade-offs and dilemmas, willful multiplicity insists that we emphasize different advantages from study to study, in some cases those deriving from multiple messages, in some cases those deriving from other sorts of inclusions. For any given study the researcher can ask: What advantages have been slighted in the past? Of course, other questions may prod a decision about what to include (and, alas, exclude) in a particular case.

It is possible to get a bit more specific about circumstances that may suggest to researchers that multiple messages not be included in a single study:

Stability of effects. Where several studies of a given message variable have yielded a consistent effect, other sorts of inclusions may take precedence over multiple messages. For example, I think this is now the case for lexical diversity, where different situational contexts and measures are called for (cf. Giles, Wilson, & Conway, 1981). This is probably not the case for language intensity.

Irrelevance of the particular case. Where a particular message, with all of its idiosyncracies, is merely a successful vehicle for producing an effect (*A*) which is subsequently related to another outcome (*B*), and where the *A-B* relationship is the only item of theoretical interest, the inclusion of multiple messages may be unnecessary, perhaps even harmful. This will probably be a rare case. An analogue is the concern with the connection between cognitive dissonance and attitude change, where features of the particular source of dissonance arousal are unimportant, as long as they do indeed arouse the state of dissonance.

Importance of the particular case. On the other hand, where a particular message is important in and of itself, there is little need for message multiplicity. Some particular messages are extremely pervasive and may be worth studying for this reason, for example, "Hello," "Goodbye," or "F--- you." (In this case, these would not be studied as representatives of larger classes, i.e., interaction rituals or curses.) Both Clark (1973) and Jackson and Jacobs mention this exception.

Extensiveness of the particular case. Certain general classes of messages have few members. In the extreme case, one or two particular messages may exhaust the category. For example, inspection of a large number of compliments collected by Wolfson and Manes (1980) revealed surprisingly few syntactic and semantic variants. Just two verbs, "like" and "love," occurred in almost 90% of the compliments that contained a semantically positive verb. Multiple versions of a compliment may be less important to include in a par-

ticular effects study than multiple types of complimenter relationships, for example (Bradac, Schneider, Hemphill, & Tardy, 1980; Jones, 1964).

Importance of variable levels. Sometimes an effect will appear at one level of a message only. For example, it appears that the relationship between lexical diversity and competence ratings assumes a quadratic form in such a way that for 25-word segments mean segmental type/token ratios of .82 and .92 differ from the ratio .72 although they do not differ from each other (Bradac, Desmond, & Murdock, 1977). Where there is reason to expect a nonlinear effect, it may be best to sacrifice message multiplicity for multiple levels of a single message.

Long messages with predicted effects. Some of the kinds of messages we are interested in are rather lengthy, for example, political speeches or lectures. A long message includes many words, many topics, many arguments, many metaphors, and many other things, most of which are theoretically irrelevant. The longer the message, the greater the number of irrelevancies and (potentially) the greater the number of representatives of the theoretically interesting construct distributed uniformly throughout the message. If a hypothesized effect is obtained in a study of long messages, the hypothesis has at that point transcended a wide range of irrelevancies even prior to replication by another investigator. The long message can be viewed as many within-study replications comprising shorter messages (although the independent effects of these shorter messages will not be assessed). It would seem implausible to argue that an obtained predicted effect is idiosyncratically bound to a particular theoretically irrelevant part of the long message. This argument becomes more tenable to the extent that memory for any given idiosyncracy decays as a function of message length. The same logic indicates that irrelevent confoundings may become increasingly serious as message length decreases, the shortest messages being individual words (Clark's primary concern, 1973) or individual sentences (Bradac, Martin, Elliott, & Tardy, 1980). The inclusion of multiple messages may be less important when message length is great. Obviously, time and energy constraints also work against including in a single design multiple messages of great length. On the other hand, where no reasonable a priori hypothesis is possible, the inclusion of mulitiple long messages may be important. Or, where theoretically anomalous effects are obtained in a study of long messages, the next study might well include multiple messages representing the construct of interest.

Importance of the presentational medium. Cook and Campbell (1979) note that one source of variation often ignored in discussions of construct confounding is "delivery mode," or the way in which the experimental stimulus is conveyed. Rather clearly, medium of presentation may be very important to communication researchers especially; that is, "delivery mode" may easily move from a confounded variable to one of theoretical interest (Bradac & Bell, 1975). Where presentational medium seems to be either a serious source of confounding or a theoretically rich variable, inclusion of multiple "delivery modes" of a single message may be preferable to unimodal presentations of multiple messages.

Apart from these specifics, willful multiplicity exhorts us to conduct not only within-study replications (in some cases using multiple messages, in some cases exploiting other forms of multiplicity) but also between-study replications. Between-study replications have unique advantages compared to their within-study counterparts (and, of course, vice versa). Importantly, in the case of experiments using constructed stimuli, typically a single entity (often the experimenter or the experimental team) produces the experimental versions of the stimulus message. This entity has unknown idiosyncratic tendencies which may warp the message or messages constructed to represent a construct. Generalizability is restricted to the warped message or message set. A between-study relication using a different entity's differently warped message or message set yields increased generalizability if results are consistent with those obtained in the study replicated. Along this line, Cook and Campbell suggest that "a strong case can be made that external validity is enhanced

more by many heterogeneous small experiments than by one or two large experiments, for one runs the risk of having a weak and heterogeneous treatment... and measures that do not reflect the unique nature of the treatment at different sites. Many small-scale experiments with local control... is in many ways preferable to giant national experiments with a promised standardization that is neither feasible nor even desirable from the standpoint of making irrelevancies heterogeneous" (1979, p. 80). These authors also argue that consecutive replications by independent researchers are preferable to simultaneous replications because consistent results from the former type of replication give evidence that the relationship of interest has transcended epochal differences and investigator quirks. But willful multiplicity insists that between-study replications are not preferable generally but only in particular cases. In other cases, transcending epochs and investigators may be less important than transcending other irrelevancies.

CONCLUSION

I have argued that the within-study replication using multiple messages is one among several important replicative forms. In particular cases, given limited resources, a single-message design may be preferable to a design using multiple messages. In other cases, we should use multiple messages, examining their individual and combined effects as Clark (1973) and Jackson and Jacobs suggest. There is no design or procedure that will lead to better knowledge necessarily. However, there is an abstract rule that will eventually yield decreasingly erroneous knowledge when employed by creative and competent persons: Vary irrelevancies—including those residing uniquely and necessarily in any particular design or type of analysis.

NOTE

1. For purposes of comparison, here is an example of a niggling objection: Jackson and Jacobs suggest that it will be quite possible to include multiple messages in single studies of message effects, pointing to a study by Jackson, Jacobs, Philpott, and Dunning (1981) where six 2-turn dialogues were individually constructed for each of 80 subjects. Having in one case spent over 50 hours constructing six experimental versions of a single message, I am skeptical about the typical practicality of every subject's getting his/her own language sample. Jackson and Jacobs seem to anticipate my skepticism when they say, "This may seem impossible to put into practice, but...."

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Confessions of a Methodological Puritan: A Response to Jackson and Jacobs

Dean E. Hewes

It is with some trepidation that I don again the black and white garb of the methodological puritan. In part my fear stems from the social stigma attached to the role of methodologist. Methodologists are a cold and unsmiling lot, enforcing the moral precepts of Donald Campbell, Karl Jöreskog, John Tukey, and B.J. Winer. Methodologists demand that otherwise decent researchers absolve themselves of the sins of Varimax by performing a LISREL IV. Methodologists are not fun people.

Methodological puritans also run the risk of betraying the purity of their faith by slipping into methodological relativism. And nowhere is that risk greater than in confrontations with other methodologists. The Jackson and Jacobs article may well be my undoing in that regard. They have adopted the methodologically correct stance toward the "language-as-fixed-effects fallacy." I have been as guilty of that fallacy as others in our field (cf. Hewes, Brazil, & Evans, 1977; Hewes & Evans, 1978); we were all wrong, and, what's worse, we should have known better. Nevertheless, I can't help wondering if the methodologically correct stance isn't too rigid to encompass all the exigencies of the research process.

Jackson, Jacobs, and this author are in general agreement on the central point of their article: Studies of message effects should contain multiple instantiations of every message variable being manipulated. To do otherwise is to confound a particular message, with all its sources of uncontrolled variability, with the independent variable that message is supposed to operationalize. Jackson, Jacobs, and I differ only on the statistical purity of our treatment of these multiple instantiations.

In the discussion to follow I identify what I think to be a major source of ambiguity in Jackson and Jacobs' use of the term *generalization*. Many of their more controversial criticisms and recommendations flow from one interpretation of this term while the other interpretation seems more in accord with the statistical issues raised in the original discussions of the language-as-fixed-effects fallacy.

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