DOCUMENT RESUME

ED 105 538 CS 501 053

AUTHOR	Mahood, Sharon M.
TITLE	An Argument fcr Phenomenologically-Based Theory.
PUB DATE	Dec 74
NOTE	20p.; Paper presented at the Annual Meeting of the Speech Communication Association (Chicago, December, 1974)
EDRS PRICE	MF-$0.76 HC-$1.58 PLUS POSTAGE
DESCRIPTORS	*Communication (Thought Transfer); Credibility; Educational Research; *Groups; Higher Education; *Interpersonal Relationship; *Theoretical Criticism; *Theories
IDENTIFIERS	*Small Group Communication

ABSTRACT

This document makes theoretical and methodological assessments of several debut papers in order to discover what they reveal about the nature of interpersonal and small group phenomena and how any particular investigation distorts the phenomena studied. The papers examined discuss such topics as the nature of the attitude change process, small group consensus formation, and credibility and subjects' perceptions. This document concludes that the importance of phenomenologically based the.ry must be further investigated and researched. (TS)

AN ARGUMENT FOR PHENOMENOLOGICALLY-BASED THEORY

Sharon M. Mahood

University of Illinois

A Critique of Interpersonal and Small Group

Debut Papers, Speech Communication Association Convention, 1974

Many of my comments regarding these papers might be best understood in the context of Robert B. MacLeod's recommendation to social psychologists. Concerned with social psychologists' tendencies to borrow potentially inappropriate models, concepts and methods to guide their investigations, MacLeod (1947) emphasized a phenomenological approach to social psychology. In so doing he suggested that the researcher must:

> look first at the world of things-as-they-are in its entirety before deciding which aspects of this world are to be considered important for theory, and at every stage in his investigation to keep checking back to phenomena to make sure that they have not been distorted by the very process of investigation. In essence the phenomenologist asks the question "What?" before he asks the questions "Why?," "Whence?" or "Wherefore?," and his answers to the latter questions are guided by his answer to the first. . . .
>
> If we are to follow the pattern of perceptual phenomenology, our descriptive analysis must be preceded by an attempt to uncover and temporarily to suspend the implicit assumptions, or biases, which govern our . . . thinking (p. 38).

In such a spirit, then, my theoretical and methodological assessments of these papers are made to discover what they reveal about the nature of the phenomena under investigation and how any particular investigation distorts the phenomena studied.

In choosing to test the appropriateness of a Markov chain model for investigating attitude change, Mr. Hewes and Ms. Evans-Hewes have directly focused on the nature of the attitude change process. I would first compliment them for the systematic fashion in which they have addressed the assumptions of the Markov model and for the realistic manner in which they have discussed the potential limitations of the model. In attempting to "upgrade the quality of theories of attitude change," however, there are more considerations of consequence than the three mentioned by these authors.

If MacLeod is at all correct, a most crucial requirement of a theory is that it be compatible with the nature of the phenomena under consideration. I suggest that Markov chain models are not compatible with the most significant aspects of the attitude change process.

Though Markov models are dynamic, in that they allow for the prediction of change, the bases of Markovian predictions are not sufficient for predicting attitude change in a complex social system. Consider the first Markovian assumption addressed in this paper--that of a stationary transition matrix. Notice that even in the most controlled conditions, the authors did not justify this assumption. For this assumption to be met, we would have to assume that ones susceptibility to persuasion remains constant throughout an entire persuasive campaign. That is, he is no more likely to be persuaded by a second persuasive attempt than he is by a third, fourth, or fifth attempt, and so on. Such an assumption implies a rather passive persuadee bombarded by similar persuasive stimuli, responding in some constant manner over a period of time. However, people are not passive creatures affected the same way by all persuasive attempts. People actively attach meaning to messages and other persuasive cues, so that it is very difficult to be sure how any one message might be interpreted by an individual. A message having relatively little impact on an individual initially may take on a special significance later due to contextual or personal factors differing over time. Hence, discovery of the persuasive situation in which a stationary transition matrix would be appropriate is unlikely. An alternative, of course, would be the discovery of systematic variation in susceptibility to persuasion so that alternative transition matricies might be derived for predicting attitude change. Such a venture would divert the researcher's attention far from the Markov model.

Rejection of the second null hypothesis in such controlled conditions also threatens the appropriateness of this model. If a few Likert-type scales can discriminate two subgroups among college subjects who respond differentially to persuasive messages, it seems very likely that multitudes of subgroups possessing varieties of response tendencies might exist in a natural setting. One could surely argue that initial attitude is not the only relevant dimension along which to identify subgroups in a natural persuasive setting. Differences in intelligence, verbal ability, cognitive complexity, or any other personal dimension could result in differences among people's responses to messages; there could be all sorts of subgroups responding differentially to persuasive attempts. In even the most mundane variable research in attitude change the interactions among personality variables, message variables, and credibility variables, alone suggest the existence of various subgroups which might respond differently to different sorts of messages.

Finally, it hardly seems sensible that we would only need to know a person's state at any one time in the past to predict his attitude change over time. This assumption seems especially inappropriate in view of the possible changes in ones propensity for attitude change as time passes.

It is significant, then, that the three assumptions of the Markov model received such questionable support in these controlled conditions. The authors, acknowledging the difficulties in confirming the assumptions still claim that the model is worth studying--the worst predictions within 28%

of the true values. However, we must remember the experimental conditions out of which such findings stemmed. College students were exposed to three persuasive messages over a period of approximately six days and repeatedly tested for attitude change. If the relatively obvious demands of this situation and the simplicity of the persuasive appeals did not yield similar linear patterns of attitude change for subjects, it is extremely unlikely such patterns would evolve in ordinary circumstances. Even here, the attitude change process does not appear truly Markovian. Even if it were, the "external forces which disrupt that process in field work" could hardly be identified and taken into account. So, asking the phenomenological question, "'What' is the attitude change process?" would not likely lead to a Markovian description.

Let's turn now to Mr. Cooper's paper on small group consensus formation. The paper was easy to follow, the hypotheses were clearly stated, and the details of the method were fairly completely reported (inter-rater reliability of measures, etc.). My comments are directed to a broader issue, however, and are probably relevant to most small group communication research currently conducted in our discipline. The research in atheoretical, an indictment commonly made, but rarely considered seriously. Return to MacLeod's warning that our concepts and theories should evolve from our answer to the question of "what" we're studying. The researcher interested in developing some explanation of consensus (an uninteresting variable to me, but that's personal preference) would need to choose factors most likely to influence consensus formation. I would not likely seek out those factors among the arbitrarily categorized communication fragments of concern in this study.

Essentially, there is no theoretical justification for the hypotheses of this study. After reviewing literature relating consensus to group size, conflict conditions, talkativeness, consensus statements and orientation, the author notes that we can look at the occurrence and linkage of specific types of statements in communication and decides to discover whether high and low consensus groups differ along these dimensions. Why? The review of literature does not suggest these as significant features influencing consensus and there is no empirically based argument justifying this direction for consensus research. There is no theoretically sound reason for the category system chosen or for the hypotheses advanced in this study.

The consequences of this a-theoretical approach are clearer when we examine the design and alternative interpretations of the results of this study. The experimenter tries to construct a "real-life" group problem-solving situation in which group members have responsibility for the success of group outcomes. It is difficult to imagine that such an objective was accomplished when the anonymity of the group members was maintained. However, the artificiality of this situation is only one source of concern. The major difficulty with the study rests with the category system employed. The author writes that "Persons record human behavior from different perceptual vantage points with varying degrees of observational purity. The use of a category system provided a common frame of reference for observation and permitted the standardization of statement coding." This justification for a category system is interesting because it admits that people--including group members themselves--view communication behaviors differently. If it would ever be hoped to sensibly explain a relationship between consensus and communication structure, feedback, and statement linkage we would surely have to establish that group members perceive the communication process as do the coders. If, for example, coders identified certain statements as information giving which were perceived as disagreement statements by group members, the coders might explain lack of consensus as a function of information giving statements while group members themselves might feel disagreement caused low-consensus in the group. At least, then, the category system should be verified as consistent with group members' perceptions.

Further, however, taken at face value, the categories of the system are not mutually exclusive. One could surely initiate a solution and through the very initiation advance additional problems to be solved. Again, the only way to really know how a statement functions in the group is to discover group members' perceptions of the communication.

One aspect of this study which may have seemed very insignificant to the experimenter is the 30 minute time limitation imposed on the discussions. Rather than concluding that the two low consensus groups failed to reach consensus, one could easily argue, in view of the reported results, that they merely ran out of time. Even the Matthews and Bendig (1955) index of agreement would not guard against the confounding of time, for various aspects tapped by this measure are potentially functions of time.

The potential confounding influence of the imposed time limit legitimizes alternative interpretations of the results. Consider, for example, the finding that solution statements in the consensus groups were most often followed by agreeing statements, whereas the low-consensus groups most often introduced information at this stage. Such findings do not necessarily suggest, as the author claims, that low consensus members really disagree but dilute their disagreement with information or opinion statements. The findings may well mean that the groups which did not reach consensus ran out of time before they could efficiently deal with all of the information they introduced. Perhaps "low-consensus" group members knew more about the topics discussed, were more interested in the discussion, and had more to talk about than consensus group members. Hence, they did not reach an appropriate agreement point as did their high consensus counter parts.

It is possible, then, that low consensus group members did not reach consensus in 30 minutes because they were more systematically and more thoroughly discussing the problems before them and less hastily jumping to superficial solutions. This is a plausible alternative explanation. and seems more likely to me than the suggestions that low consensus group members avoid agreeing or disagreeing discussion statements, or that their reluctance to accept a decision will not be reflected by disagreeing statements. There is no evidence that low consensus groups ever _were_ reluctant or disagreeable. These interpretations are inferences made by the authors with no theoretical justification.

This study illustrates the importance of phenomenological theory in social science. It is relatively easy to collect data but more difficult to explain significant results. In this case the lack of theory leads to the obvious conclusion that consensus groups agree more than do low consensus groups, if by low consensus we mean groups which do not complete the task in the amount of time allotted. If anything, we may have to conclude from this study that "low consensus" groups, operating from more informed bases, would arrive at higher quality solutions in time than high consensus groups.

In dealing with Mr. Hawkins' paper let's recall MacLeod's claim that researchers should first describe the phenomena of consideration and out of the unbiased description develop his models, concepts, methods and research questions. I would have to compliment Mr. Hawkins for choosing a natural setting for study, but in this case of research into organizational morale, MacLeod's approach was not taken. Figure I, the summary of possible analogues for affection, dominance and "other factors" most vividly demonstrates my point. The author relies on apparent semantic synonyms in establishing the argument that prior concern with "climate" can best be understood through administration of a modified version of the FIRO-B and Brim's "desire for certainty" index. In reading and rereading this paper I had to stop to retrace the lines of argument leading me to the author's operational definitions of interpersonal climate; I found it very difficult to legitimize his concern with Schutz's categories and with desire for certainty. The major reason for the focus of this study appears to rest on the existence a FIRO-B scale and a "desire for certainty" index which sound similar to other notions which have not been operationalized but must be important in organizational settings. The concepts, results and theory of this paper are not phenomenologically based.

The difficulty in sorting out the implications of this research project are even more difficult when we consider the report of the method. From the written version of this research, on the basis of which I must critique the study, it is extremely difficult to determine what the researcher eventually measured.

Consider the criterion measures consisting of "frequency and duration of communication topics, as perceived by the subordinate." What did the experimenter demand of the subjects to get these measures? Were subjects asked how frequently and for how long they talked about certain topics, then given the opportunity to respond in open ended fashion? Did the topics tapped exhaust the possible range of topics which might be discussed in the dyadic situations considered? At this point the reader knows nothing about the nature of the communication behavior ultimately predicted by the author's regression equations except that whatever the criterion measure was factor analyzed into a three factor solution. Even the magic properties of factor analysis, with which we are so enthralled these days, do little to resolve my confusion. I still don't know what was factor analyzed into a three dimensional structure so am left with some arbitrary labels chosen by the author to describe communication behavior. From this study I know that quite a lot of variance associated with the frequency and duration of some sorts of communication behavior can be accounted for by several predictor variables.

Unfortunately, the nature of the predictor variables is as confusing as that of the criterion variables. Who knows what a modified version of a FIRO-B really measures? However, I'll forego the standard arguments regarding the validity of the FIRO-B. I don't know what Brim's "desire for certainty" index measures; the author did not describe it in any detail and no bibliographic citation accompanied it in the paper so I was unable to track it down myself.

My major questions regarding the predictor variables, however, center around the potential confounding nature of perceptual processes supposedly tapped by the measures. There were six predictor variables:[1] need satisfaction indices for affection, dominance and certainty and expected satisfaction indices for affection, dominance and certainty. Need satisfaction indices were discrepancies between subordinates' wants (as revealed on FIRO-B and Brim Index) and their perceptions that these wants were being expressed in their relationships with supervisors. Expected satisfaction indices were discrepancies between the subordinates' perceptions of how the supervisor saw the subordinate's needs (meta-metaperceptions) and the subordinate's perceptions that his wants were being expressed in their relationship. These predictor variables are not independent. How would a person judge what his supervisor felt about his needs if not by partially inferring from what he perceived to be expressed by the supervisor in the relationship? What does one measure when he finds a discrepancy between a metametaperception and a metaperception? Even if these six predictor variables account for variance of the criterion variable, it's unclear what they are or how they interrelate; we'd have to understand the entire processes of perception before we could appreciate the significance of these predictor variables.

Thus confounding becomes very crucial when we consider the author's claim that:

---

[1]Of course this overlooks the additional four demographic predictors, apparently responsible for elevating the effectiveness of prediction considerably. It would be interesting to see actual reports of the variance contributed with each addition of predictor variables. Length of association between supervisor and subordinate might be expected to account for much more of the frequency and duration of interaction than is emphasized in the discussion of the paper.

> The implications from this research may be very important to the manager who wants to change a particular behavior or set of behaviors in his organization. If the beta weights in such regression equations can be shown to be stable, definite prescriptions could be made to alter specific behaviors. For example, if subsequent research suggests that there exists (an equation) that accounts for 82% of the variance of a particular behavior, . . . then the researcher can examine the entire equation and potentially predict the resultant behaviors by changing what is given or perceived in the relationship.

The regression equations in this research, regardless of the amount of variance for which they account, do not provide the basis for predicting behavior change. To predict behavior a manager would have to know how perceptions are formed, and how metaperceptions relate to meta-metaperceptions, etc. Generally the regression equations are a-theoretical and provide little understanding of communication behavior. At the most they reveal that people interact more frequently and for longer periods of time when it is rewarding to them. They do not reveal what rewards a person or why he sees his needs as being met in certain circumstances. Affection, dominance and certainty are themselves matters of perception and to know how to manipulate them for any one person a manager would have to know what a subordinate saw as affectionate, dominant and certain. The model provides no basis for making such a determination.

This is the first point in the critique where I mention statistical analyses, and I comment here only because a bias has been struck in reading the reported analysis in Mr. Hawkins' paper. He spends a great deal of time discussing statistical procedures; to some readers such statistical thoroughness conveys the impression that a great deal was discovered in the study. I would compliment Mr. Hawkins on his analyses, but offer a word of caution. Essentially, any statistical analysis involves adding, subtracting, dividing, squaring, or otherwise operating on numbers in order to analyze or interpret variance. Computers perform numerical operations very easily and rather rapidly. Statistics books assist us in deciding which packaged statistical program we need if our numbers match certain assumptions. But no matter what we do to our numbers, we're stuck with a mess unless our numbers are realistic approximations of some phenomenon about which we're making claims. So, I'll not bicker about regression procedures or about how factor matrices might have been rotated in this study. Instead, I'll summarize my critique of this paper by asking what we now know about the nature of climates in organizational settings.

Initially, I was pleased to see that Virginia Richmond focused on subjects' perceptions in her study. The conclusions of her paper on opinion leadership are interesting, but partially unwarranted. She suggests that her results "indicate the dimensions of perceived credibility other than competence also play a role in opinion leader selection . . . these results show promise for expanding research in this area which will . . . hopefully result in greater precision in the prediction of opinion leader selection and the understanding of the process of diffusion in general." The major difficulty with this claim stems from its dependence on the logical fallacy of affirming the consequent. In this case, as well as in most opinion leadership research, the claim is made that opinion leaders will be seen as more similar along some dimensions and as credible. Having confirmed such assertions, the researcher then reasons backwards from the perceived similarity and credibility to claim he has found the bases upon which opinion leaders are selected. The argument takes the form: If one is an opinion leader he will be perceived as similar and credible; this person is seen as similar and credible; therefore, this person is an opinion leader.

Consider the consequences of such reasoning in this study. Supposedly, asking subjects to identify people from whom they would borrow notes identifies opinion leaders for those subjects. Whether borrowing notes resembles adopting innovations is questionable. However, assume for a moment a note-lender _is_ an opinion leader (rather than a person who has good attendance and neat handwriting). Research in accuracy of person perception has consistently revealed the potential confounding influences of assumed similarity in person perception. That is, people generally assume similarity among themselves and people about whom they feel favorably (Dymond, 1953). A potential note-lender is likely to be positively evaluated, hence is likely seen as similar to the potential note borrower and as credible. It would be silly, after all, to borrow notes from an incompetent, less favorable incredible person. And, it is only reasonable that the chosen note-lender would be perceived as more similar and more credible than others in the class; otherwise, the _others_ might be the ones from whom one would borrow notes. Further, to ask a subject to identify a person from whom he would _not_ borrow notes would sensibly tap a person less credible and more dissimilar than the subject if we can assume that the subject sees himself as competent and credible.

So, the results of the study and the support rallied for the hypotheses are not surprising. However, it is unlikely that _opinion leadership_ has been studied, since subjects hardly open themselves to influence by borrowing a good set of notes. Further, however, the results do not prove that the perceived similarity between leaders and followers combined with the perceived credibility of leaders caused subjects to choose the particular note-lenders identified.

It is also interesting to note that this study has strayed somewhat from initial assumptions regarding the characteristics of opinion leaders. Credibility, rather than "optimal heterophily" was measured in this study. That is, subjects who were themselves followers were not asked to compare their own level of credibility to that of their "leaders." The reasoning behind opinion leadership choice suggests that subjects would see an opinion leader as more credible and more competent than themselves. Since there is no basis for subjects to compare themselves to leaders on credibility dimensions in this study, optimal heterophily has not really been measured. To say that I see my opinion leader as more credible than I see others does not say that I see my opinion leader as having more of an attribute than I have.

As a final comment on the tapping of opinion leaders in this study, consider a possible situation. Suppose I am a subject asked to identify a person from whom I would borrow notes. I identify person _A_ and you then ask me to identify someone in the class who would also borrow notes from person _A_. I tell you person _B_ would also borrow notes from person _A_ so you assume that person _A_ is my opinion leader and person _B_ is a fellow follower. After the experiment you ask me why I would borrow notes from person _A_ and I explain that upon being absent a few days ago I asked person _B_ for his notes but he said that his notes were incomplete and that he was planning to borrow person _A_'s notes. So, I guessed that if _B_ would borrow _A_'s notes I should too. Now, who's the leader and who's the follower?

Opinion leadership or interpersonal influence cannot be fully explained until the natural processes of interpersonal perception are better understood. Choice of an "opinion leader" is a process of interpersonal impression formation, and could be most fruitfully examined within the framework provided by attribution theories. Redefining the phenomenon this way would significantly redirect the methods of research in this area.

Mr. Weinberg's three-stage model of group panic follows sensibly enough from the group panic literature he cites. Having developed such a descriptive model from observations of panic situations, however, he does not provide a basis for predicting potential methods of controlling the panic situation. While the second stage of the panic cycle is characterized by development of communication, the model does not dictate that <u>any</u> communication would signal the second phase in the cycle--the beginning of reestablishment of communication. More than description of prior panic situations as they occur naturally is needed to discover the impact of communication attempts in <u>altering</u> the panic cycle.

It is very easy to claim that a model provides a basis for future research. However, future research requiring manipulation would be very difficult in studying the extremely emotional aspects of panic. The suggestion that computer simulations provide a basis for research sounds much simpler than it would likely be. In order to simulate a situation one must have relational rules governing the interactions of the elements being investigated. Where do such rules come from? Like most communication models, this model is not really predictive. That is to say that the model enumerates stages of a panic cycle, but the basis for predicting relations among the stages falls outside the model. So, the model is a-theoretical as are most social science models. The *why* of panic situations is not addressed in the model *per se*. Until theoretical bases for providing the relational rules governing the elements of the model are provided, the model cannot generate research questions beyond those descriptive sorts of questions already addressed by the model.

Throughout this critique I've focused on the broader aspects of theory construction and methodology in keeping with MacLeod's concern that our research be sensibly related to the phenomena investigated. It may be disappointing that I've not chastised any of the authors for statistical flaws in data analysis. Unless reported results appear so glaringly unrealistic that I suspect them or analyses appear so ill-chosen that they're unreasonable, I assume the results to be accurate. After checking the matrix multiplication in the Hewes--Evans-Hewes paper and examining the reported eigenvalues, factor scores, etc., I was satisfied that their analyses were legitimate. Admittedly, I played with Mr. Cooper's frequency formula for some time before my skepticism regarding its validity was satisfied. In the process I found two additional errors in one table--which were inconsequential I might comment. Mr. Hawkins did some sort of factor analysis (likely a packaged program in the possession of the Purdue Computer Center library) and told me nothing about it. Apparently, however, he knew enough to very thoroughly analyze potential sources of variance and even went so far as to cross validate his regression equations: I'll assume he knew what he was doing. Virginia Richmond's reported results are very abbreviated; her analyses of variance results look sensible, however, and the appropriateness of her analysis raises no questions for me. The report of performed discriminant analyses is very sketchy, but since they are not the thrust of the paper, the analyses are of little concern to me. Generally, then, too little is known about the particular analyses performed to claim there were errors. I belabor this point here to put this critique in perspective.

Having spent a considerable portion of my educational career in math classes I respect the magic of numbers! I recognize that there are more and less powerful statistical techniques. However, I'm more concerned that researchers in this discipline reflect a bit on <u>what</u> they're analyzing, than that they've chosen the most powerful multiple comparison tests available. The statistical justification to reject a null hypothesis does not make our choice of the alternative hypothesis legitimate. Such a choice is rather arbitrary and can only be assessed by determining if the answer to the "Why?" provided by that alternative hypothesis has any realistic association with "what" we're investigating.

So, in conclusion I'd thank the authors of these papers for providing research examples around which I could build an argument potentially important to all researchers in this discipline. If I have been overly harsh in an effort to emphasize the importance of phenomenologically based theory I am confident they will acknowledge it.

REFERENCES

MacLeod, R. B., The phenomenological approach to social psychology. In Taguiri, R., and L. Petrullo (Eds.), Person perception and interpersonal behavior, Stanford, Calif.: Stanford University Press, 1958.

Matthews, J. and A. W. Bendig, The index of agreement. Speech Monographs, XXII (March, 1955), 39-42.

Dymond, R. The relation of accuracy of perception of the spouse and marital happiness. American psychologist, 1953, 8, 344 (Abstract).