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ABSTRACT

In this review of the current state of humor research, the author traces the basic problems to two major deficiencies: (1) a lack of adequate theorizing concerning the effects of humor in communication, and (2) problems in experimental designs, particularly the study of humor in a laboratory setting. Too little research and insufficient attention to operationalization of independent variables, such as measurement of the humor of humorous material, coupled with a dearth of valid measurement procedures are seen as other contributing factors. The author makes several suggestions for the improvement of humor research, emphasizing the need for development of very clear theory about humor. Among the possible study directions he mentions are the use of humor in affecting ethos, humor's unifying effect upon audiences, and making use of certain psychological theories of humor to examine how humor may be produced. (LG)

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## WHAT'S WRONG WITH HUMOR RESEARCH?

by

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## WHAT'S WRONG WITH CURRENT HUMOR RESEARCH?

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"What's wrong with current humor research?" A great deal but probably not much more than is typical of many areas of interest to speech communication. There isn't much humor research. Almost all the research in retention and attitude shift has failed to achieve statistically significant findings. Many experiments failed to operationalize the independent variable of humor. Many others have neglected to test the measuring instruments or to weigh the influence of demand characteristics. Humor research is in a serious state.

I do not wish to settle for a few cheap shots in holding humor research up to ridicule. That is too easy a route and doesn't really move us forward in the important task of learning more about humor and its function in communication. One can do little in eight minutes to develop the variety of problems which beset humor research. Let me therefore concentrate on four indictments:

- 1) There is too little research.
- 2) The empirical research in humor is not grounded in adequate theoretical models and in turn has not contributed to theory building. (The next two items really grow out of this latter point.)
- 3) There has been too little attention to the demand characteristics of the experiments. These characteristics are such to almost rule out the possibility of significant results.
- 4) There has been insufficient attention to operationalization of the independent variable, i.e., assuring that humorous material is indeed humorous, and with the development of valid measurement procedures.

It is tempting to undertake a detailed analysis of several of the experimental designs which have been employed. Time does not permit nor do I think an oral presentation is the proper place for such an analysis. Such an analysis is most meaningful when there is time to examine the original research report in connection with the critique. Mr. Kennedy and I hope to provide such an analysis in the near future.

I should add that I am not convinced that humor research is in a worse state than the research in many areas of our discipline. But I have not been asked to criticize those areas. Do not be surprised, however, if the topics in this paper are similar to lines of criticism that may be used as a basis for evaluation of research on many variables in our discipline.

### 1. There is too little research on humor.

The bibliography for this paper lists 47 items. Treadwell lists 96 in what she assumes is an exhaustive summary of empirical studies of wit and humor from 1897 to 1966. (44) Other bibliographies list fewer and many of these are empirical only in the most general of terms. If we list studies which are experimental I think we would run under fifty independent citations. Many of the empirical studies simply correlate items related to humor with other personality characteristics. It may be comforting to know, for example, that height correlates better than weight with humor. Fat people don't have a better sense of humor but tall people do. (41)

But when you consider the central role which humor is supposed to play in human interaction and in the daily lives of individuals, 50 or 100 items is a surprising modest amount of research. Thus we should join with Berlyne in bemoaning the lack of research. (2)

Most of the research in speech communication has been on the effects of humor on retention and/or attitude change in the public speaking situation although some such as Gruner have not hesitated to include written material. (17) But when we consider the attention that humor is given in textbooks and rhetorical treatises we should be concerned about the limited range. Humor is seen as a key factor in interest and as an important attention device, an important stylistic feature, as an element which affects ethos, and as an important element in acceptance. Humor is seen as basic to entertainment value, as a means of establishing audience polarity, as a strong weapon in attack and defense--including reductio ad absurdum yet we see very little study of any of these. The limitation to source to audience situations and omission of small group and face-to-face communication situations is particularly unfortunate.

Considering the importance attributed to it from classical times to the present (14, 15, 22; also 2, 8, 28, 29) available quantity of empirical research is surprising. In part, this may arise from the fact that research in humor has had a negative reinforcement effect, that is most researchers have not succeeded in confirming their expectations. Further, the lack of clear theoretical guidelines and problems in operationalizing the independent and dependent variables may contribute to this lack of research. We will next examine these factors.

## 2. Lack of adequate theoretical models and failure to contribute to theory building.

This criticism needs some qualification. Certainly speech communication as a whole and those interested in the public address setting do not carry the sole burden for generating sound theory in the area of humor. Humor is too generally of interest to all of human behavior for communications research to carry this burden alone. Indeed, there are many diverse theories of humor and the laughable which are available in psychology. (2) Unfortunately the experimental researchers starting with Lull (30) do not turn to these models. Rather they have drawn upon one or more statements about what humor is presumed to do in communication and then tested to see if this is true. The theories in psychology are not comprehensive, and there are several, each of which does a somewhat adequate job of explaining some small portion of the humor spectrum, but we could be using them. The theories of incongruity, superiority, aggression, relief, etc. give much potential for theorizing about humor's role in communication.

Students of humor have often occupied their time in making endless classifications of humor--some of these sophisticated and some not. (4, 9, 24) Some few seem to have been concerned with operational reality of these classifications and others have not. Certainly it would be difficult to classify humor reliably in terms of the many schemes which we have available.

With the exception of Gruner (22) and those who have modeled their researches upon his or been guided by him, (33) few researchers in humor in speech communication have worried about classifying the type of humor used. In part, this may reflect the fact that most researchers conduct one study and do not continue any kind of programmatic research. Failure to develop programs of research necessarily limits the probability of grounding research in a well-conceived comprehensive theoretical position and similarly in devising research that will test key components of a theory.

Since most of the researchers in humor have not developed or explicated a theoretical model, we need to employ some negative evidence to argue that they lacked one. The insensitivity to design characteristics and the fact that the results of confirmation or failure to confirm the experimental hypotheses will really do little to advance understanding of the causal relationships involved leads me to argue that sound theoretical models have not been employed. One key function of a theory or a model is to guide research to the key points of the theory for testing and to determine when testing is reasonable. The status of a theory can help to determine when it is more effective to continue building and developing theory and when it needs to be tested. The failure of almost all our tests to show humor makes a difference in retention (excluding the Gibb study 11) suggests the theory development was not very far advanced. The weaknesses in sensitivity to demand characteristics and to experimental design problems further reinforces the view of an atheoretical approach to the research. In short, we have wondered whether girls with warts on their nose are funnier than girls without, without asking what significance the answer has to understanding communication and what meaning to give to the answer once we have it.

### 3. There has been little attention to the demand characteristics of the experiments.

Almost all the experiments have been done with students in classroom settings. The demand characteristics of these settings are such that it is not surprising that the introduction of humor as contrasted with similar material with the humor omitted should produce no significant differences. Surely we are now familiar enough with the demand characteristics of experiments in the classroom as to be sensitive to their effect on humor research. If we compare a good, interesting presentation with the same presentation with "presumably" relevant humor added and set the class for the fact we will want their reaction, we should not be surprised that retention is the same in both situations. (19) Our students are used to learning in spite of the difficulties we put in their way. The novelty effects (ala Kennedy presenting his stimuli with the first color closed-circuit TV the students have seen 25) or the endorsement of the instructor to cooperate in this important experiment, i.e., put on your scientizing role and help science, may explain many of the results.

The fact that when students are told items are funny they then laugh more and report them as funnier on measuring instruments suggests the operation of demand characteristics (25).

Should humor add so much to an already good speech given the conditions of these experiments that significantly different results should occur? I doubt if humor could prove that strong.

We urgently need to move humor out of the classroom into the real world where the normal social facilitation involved in humor, and the expectations and sets of normal receivers are such that humor can indeed have a chance of producing significant differences.

#### 4. Insufficient attention to operationalizing the independent variable and developing valid measurement procedures.

One of the concerns that Gruner has pointed to is that of being sure that material designed to be funny is indeed perceived as being funny by receivers. This is illustrated in Gruner's study on satire in which the majority of his receivers failed to perceive the material as satire. This despite the fact that the faculty members in the English department clearly perceived the material as satire. (20) (also see 1, 3)

Researchers have tended to use Smith's scales developed for measuring the seriousness of material in communication. I would like to see some additional factor analyses which replicate the isolation of this dimension. Also, two scales may not be as sensitive a measure as is needed. And these scales may not work equally well for the many different types of humor.

Many of the retention tests have been multiple-choice items which may measure recognition, not recall. Further, I have very real trouble getting any assessment of reliability and validity of the tests employed. Researchers seem to assume that something valid once in one setting for one purpose is valid for any setting and any purpose. I am sorry--it simply is not so. All we know about testing and measurement says it isn't so. Researchers must make the measurement of reliability and the arguments about validity particular to their use--and they have not done so in almost all instances. (e.g., 20, 30, 33)

#### A Final Statement

I suggest we make our humor research productive. And the way to do this is to devise some very clear theory about humor and then to begin to test that theory in productive ways.

There are some clear hints to follow up: use of humor can affect ethos (19, 6, 5, 12, 23) humor does have impact in small group settings (6, 38) and humor may well affect attention and interest, if we can get away from demand characteristics which mask this effect. Humor may serve as catharsis. It may produce identification. It may facilitate response. It may unify audiences. Certainly the various psychological theories of humor have value in suggesting how we may produce humor, using relief, aggression, superiority, incongruity, etc. and indicating what personality types may respond to what kind of humor in particular situations.

In short, designs which ask if humor makes a difference are not likely to be very productive. But if we put the question of humor to tests in situations where the presence or absence of humor can reasonably make a difference we may attract quite a few more researchers to humor. More importantly, we move away from prescription to a more descriptive level and yield a much more coherent theoretical basis for the advice which we do offer relative to humor in communication.

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