Message Description

Daniel J. O'Keefe

Department of Speech Communication
244 Lincoln Hall
University of Illinois
702 S. Wright Street
Urbana, IL 61801

Paper presented at the Speech Communication Association annual convention
November 1987
Boston, Massachusetts

Message Description

Abstract

Messages (utterances, texts, etc.) are indefinitely describable; that is, the number of true descriptions of any message is indefinitely large. Thus an inevitable and recurring task for investigators of communication is the selection of message descriptions (or, more broadly, descriptive vocabularies for messages). This task, however, has too often been unappreciated or misapprehended, precisely because of a failure to grasp the indefinite describability of messages.

Message Description

One recurring task faced in communication research is the task of describing messages (where "messages" can be utterances, texts, communicative behaviors, interactions, etc.). Whether one is interested in the sorts of effects had by a given message or message type, or in the sorts of processes underlying the production of certain messages or message types, or in the sequencing of messages in an interaction, or in the role certain message types play within large organizations or families or dyadic relationships, or in virtually any facet of communication processes and effects, one is inevitably in the position of somehow having to offer descriptions of the messages involved. By "description," of course, I mean not only extended linguistic characterizations (which is a sense given "description" in everyday use), but also any linguistic representation, any label or categorization (as in, e.g., the classification of a communicative act within a particular category in an interaction coding system).

Despite its importance, the task of message description has not always been appreciated for the difficult undertaking it is, and current thinking about how to evaluate message descriptions sometimes reflects misapprehensions of the task. This paper seeks to clarify the task of message description, by underscoring a little-noticed yet consequential point about how messages can be described.

The Indefinite Describability of Messages

There is a particularly important point about messages that is crucial to understanding the task of message description, and yet this idea has gone largely unnoticed and unremarked. The point in question concerns what might be called "the indefinite describability of messages." In this section I explain, and elaborate some consequences of, the indefinite describability of messages.

Perhaps the most direct way to express the indefinite describability of messages is this: The number of true descriptions of any given message is indefinitely large. That is, for any given message (utterance, communicative behavior, text, interaction, etc.), the number of descriptions that are true of that message is indefinitely large.

Consider, for instance, an utterance such as "Can you pass the salt?" This utterance might in a given circumstance be correctly described as a request, an interrogative, in the active voice, a first pair part, an utterance with five words, a turn at talk, a one-up relational move, an interruption, an indirect speech act, and so on and so on. Even if one's inventiveness wanes, and one suspects that all the possible true descriptions have been given, one cannot rule out the possibility that some future investigator will devise some new true description. (Notice, for instance, that there was a time when descriptions such as "first pair part" and "one-up relational move" were not yet dreamed of.)

There is another way of displaying the indefinite describability of messages, and that is by saying that the number of <u>features</u> of messages (utterances, etc.) is indefinitely large. Every description of a message makes reference (in some fashion) to some aspect or feature of that message, and the indefinitely large number of possible message descriptions straightforwardly suggests the indefinitely large number of possible message features. We can never be sure that we have detected <u>all</u> the possible features of a message (which is why we can't rule out the possibility of some future investigator devising some new true description of a given message).

Given what has been said, it should be easy to recognize that no single description can be _the_ intrinsically correct description of a message. Since the number of true descriptions of a message is indefinitely large, it does not make sense to say that "X is _the_ correct description" of a given message. All of the descriptions given above of "Can you pass the salt?" (it's an interrogative, a first pair part, etc.) can simultaneously be correct descriptions; none is _the_ right description. One can correctly describe any given message in an indefinitely large number of ways. Of course, none of this means that a description cannot be incorrect. It would be incorrect to describe "Can you pass the salt?" as a French sentence, for instance. But there is not just _one_ correct description of that (or any) utterance.

Even stringing together all the particular individual true descriptions of a message into one very long description will not produce _the_ single correct description. The reason, again, is that one cannot rule out the possibility of some future researcher devising some _new_ true description that hadn't been included earlier.

Since the number of true descriptions of a message (or the number of message features) is indefinitely large, the number of possible _systems_ for distinguishing and classifying messages is also indefinitely large. That is, the number of possible "descriptive vocabularies" for communicative conduct (ways of describing messages, message category systems, message coding or classification schemes, etc.) is indefinitely large. Since any feature of a message can serve as the basis of a descriptive vocabulary, the fact that the number of message features is indefinitely large means that the number of possible descriptive vocabularies is similarly indefinitely large.

It follows quite naturally that (just as there is no single intrinsically correct description for a given message) there is no single intrinsically correct _system_ for describing messages--that is, there is no one intrinsically correct descriptive vocabulary for messages (utterances, texts, etc.), no one correct message category system, no one correct way of distinguishing and classifying messages. There are a great many descriptive vocabularies for communicative conduct, and no one of them is _the_ correct descriptive vocabulary.

This doesn't mean that all descriptive vocabularies are equally good or useful, or that no vocabulary is ever inappropriate or unsuitable. Just as the fact that there is no single intrinsically correct description of a given message doesn't mean that there are no defective descriptions of messages, so the fact that there is no single intrinsically correct system of description for messages doesn't mean that there are no defective systems for describing messages. One might have better and worse descriptive vocabularies for communicative conduct, but this does not mean that there is some one correct descriptive vocabulary.

As a result of this indefinite describability of messages, there is an inevitable, recurring, and general task faced by investigators of communication: the task of devising or selecting a descriptive vocabulary. One has to describe messages somehow, and (as just shown) one can neither locate the one correct description nor give an exhaustive description--so researchers face an inevitable choice among alternative descriptive vocabularies and must therefore devise procedures for selecting and using some descriptions rather than others.

So, for example, this task is faced in interaction-analytic work as the task of creating or choosing a coding system. In studies of influences on compliance-gaining message production, the task surfaces as the task of devising or selecting a typology for compliance-gaining. The task appears in persuasion "effects" research, because investigators studying the effects of variations in message features need somehow to describe the messages employed (e.g., as a "high fear appeal" message, or a "two-sided" message, or a "low language intensity" message). And so on, across the span of communication research. Because of the indefinite describability of messages, communication research inevitably involves choosing, evaluating, and justifying descriptive vocabularies for messages.

Message Description: An Unappreciated and Misapprehended Task

It might seem rather obvious, hardly worth noticing, even trivial, that there is no one intrinsically correct vocabulary for the description of communicative conduct, and that consequently researchers inevitably face the task of selecting some descriptive vocabulary. Unfortunately, this task has too often been unappreciated or (more commonly) misapprehended, with these problems stemming precisely from a failure to grasp the indefinite describability of messages.

<u>Insufficient Appreciation of the Task</u>

Sometimes the task of selecting a descriptive vocabulary for messages is unappreciated. That is, it occasionally happens that the task simply isn't recognized by researchers as a task that needs careful attention. There are at least two manifestations of this in the research literature.

<u>Malformed research questions</u>. One manifestation of inattention to the matter of message description is the appearance of certain sorts of malformed research questions or claims. Consider, for instance, the research question of Bragg, Ostrowski, and Finley (1973): "Do first-borns and last-borns use different techniques when they attempt to persuade another person?" (p. 351). A moment's reflection should make it clear that--despite seeming to be a straightforward yes-no question--this research question can't meaningfully be answered. Using one scheme for classifying persuasive techniques, the persuasive attempts of first-borns and last-borns might not differ at all; but using some other message classification system, first-borns and last-borns might be seen as using very different techniques. Since there is no single intrinsically correct way to describe persuasive techniques, there can be no meaningful pursuit of a research question such as "do first-borns and last-borns use different persuasive techniques?" To ask such a research question is to fail to acknowledge that the number of possible descriptions of persuasive efforts is indefinitely large.

Similarly malformed research questions are not difficult to locate:

> "The present study investigated whether women and men differ in their social influence behaviors with subordinates in a simulated organizational setting." (Instone, Major, & Bunker, 1983, p. 323)

> "The pivotal research question becomes: Does an influencer's liking (or disliking) for the target determine the form of influence he uses against the target? In other words, _does liking determine preference among influence tactics_?" (Michener & Schwertfeger, 1972, p. 191, emphasis in original)

> "The aim of this paper is to investigate whether masculine, feminine, and androgynous husbands and wives will tend to use different patterns of means of influence." (Rim, 1980, p. 117)

Taken at face value, each of these research questions makes sense only with an accompanying assumption that there is some single intrinsically correct way to describe messages (because such an assumption makes message description an unproblematic undertaking, there being only one correct way to describe any given message). But that assumption is defective, and thus each of these questions needs a reformulation that reflects a recognition of the indefinite describability of messages. To be meaningful, questions about the determinants of communicative conduct must specify what _feature_ of messages is under examination (e.g., by clarifying that the research question was whether liking for the target would influence, say, the degree of face protection accorded the target in the persuasive message).[1]

The examples discussed thus far have all involved a mistake embedded in questions of the form "Does X (birth order, sex-role type, etc.) influence the compliance-gaining messages persons employ?" But similarly malformed questions can be found in other lines of research as well.

Consider, for instance, research on the structural organization of group interaction. One research aim pursued by some investigators in this area has been the identification of the relative predictability of group interaction (or, more precisely, of the sequence of acts in an interaction). The basic research paradigm involves recording (e.g., videotaping) group interaction, coding the interaction using some interaction category coding system, and then assessing the relative predictability ("structure") of the coded act sequence.

At least some investigators have cast their central question this way: "How much structure is present in the interaction of a specific social system [e.g., a group]? How complex is this interaction?" (Fisher, Glover, & Ellis, 1977, pp. 231-232). But this is not a meaningful question. How much predictability (structure, complexity) there is in the interaction of a given group will depend centrally on the choice of a descriptive vocabulary, that is, on the choice of a coding system for classifying acts in the interaction. With one coding system, the act sequences may be very predictable; with another system, the very same interaction may appear haphazard and unstructured (see Stech, 1977). To ask the question "how much structure is present in this group interaction?" is to fail to recognize that the number of possible descriptive vocabularies for communicative conduct is indefinitely large. There cannot be some single "correct" estimate of the structure (predictability) of a interaction, because there cannot be some single intrinsically correct choice of coding system. It makes no sense to ask "how predictable is this interaction?" because the same interaction can be both highly predictable and utterly unpredictable, depending upon the descriptive vocabulary with which the interaction is approached.

The mistake here--the failure to recognize that the predictability of interaction cannot be given some single correct estimate--is connected to an implicit belief that there is some one correct general vocabulary for the description of interaction. If there were some one correct general category system for interaction analysis, then it would make sense to ask "how much structure is present in this group interaction?" since the correct "amount of structure" would be whatever amount was given through application of the one correct category system. But if there are many different ways of categorizing acts in interaction, with no one classification system being the one correct system, then there can be no meaningful answer to questions such as "how predictable is this group interaction?"

For similar reasons, some ways of thinking about the "group phase hypothesis" turn out to be unsatisfactory. If one defines group phases as as a matter of groups undergoing "regular and predictable changes over the period of time during which the members constitute themselves as a group," where "these changes may be looked upon as relatively distinct and discrete <u>phases</u> or stages in group behavior" (Cissna, 1984, p. 6), then it does not make sense to ask whether groups do or don't exhibit phases. Since any set of relatively distinct stages will count as the occurrence of "phases," one could find that a given group both <u>did</u> exhibit phases in development (using one way of describing the group's interaction) and did <u>not</u> exhibit phases in development (using some other way of classifying the acts in group interaction).

And, for obvious related reasons, it cannot be quite satisfactory to suggest that there are "two mutually exclusive models of small group decision-making processes. The unitary sequence model--the classical model of decision development--assumes that all groups follow the same sequence of phases. The alternative, the multiple sequence model, assumes that different groups follow different sequences" (Poole, 1981, p. 1). It is entirely possible both that all groups follow the same sequence of phases and that different groups follow different sequences (thus making the two models not mutually exclusive): using one descriptive vocabulary (one coding system) different groups may be seen to follow different sequences in decision-making, but with another vocabulary (a different coding system) all groups may exhibit the same sequence of phases. To ask whether all groups follow the same sequence of phases is a question that is malformed in the same way as is the question of how predictable a given group interaction is. In each case the malformation stems from having overlooked the fact that a message may legitimately be described in different ways, depending on one's choice among the indefinitely many possible descriptive vocabularies, and so from failing to recognize that the "predictability" or "phase structure" of a group interaction will vary depending on the descriptive vocabulary employed.

Single-message instantiations of categories. A second consequence of insufficient appreciation for the indefinite describability of messages (and, correspondingly, of the importance of the task of message description) is the use of single-message instantiations of message categories in studies of message effects.

Consider a hypothetical experiment concerning the effectiveness of persuasive messages. Two messages are constructed for the investigation: message A is a message with a great deal of concrete imagery, message B is a message with very little concrete imagery. The experimenter finds that message A produces significantly greater attitude change in receivers than does message B, and so concludes that variation in the degree of concrete imagery is an important influence on attitude change through persuasive communication.

This sort of reasoning has been critically examined by Jackson and Jacobs (1983). As they note, the inference that the variation in concrete imagery is the cause of the variation in effectiveness may be unsound. The main reason is that when the experimenter induces variation in concrete imagery in the experimental messages, there is inevitably other variation that is also induced in those messages. Message A and message B will inevitably differ not only in concrete imagery, but also in other message features as well (perhaps in the comprehensibility of the vocabulary used, perhaps in the emotional tone, etc.)--and hence it may be unwise to conclude unqualifiedly that the variation in concrete imagery is what's crucial in determining the outcome.

The connection between this matter and the indefinite describability of messages may perhaps be made clear by rephrasing the point that there is not some single correct description of a given message, using the distinction between types (abstract categories) and tokens (concrete instantiations of those categories): any message token (any actual message) exemplifies an indefinitely large number of message types. The token "Can you pass the salt?" instantiates the "request" message type, the "first pair part" message type, and so forth. Given some message token, there is not just one message type that it exemplifies, but rather an indefinitely large number of message types.

Thus the problem of generalizing about message types from message tokens derives from the indefinite describability of messages. If one compares two particular messages (two particular message tokens), those two messages can differ in an indefinitely large number of ways (not just in the one way that originally interested the experimenter). Hence, as Jackson and

Jacobs suggest, one can begin to feel confident about attributions for message effects only when one has a great many different instantiations of the abstract message type in question.

The general point to be noticed is that a sensitivity to the indefinite describability of messages is critical for appreciating the nature of the task of message description. Difficulties such as poorly formulated research questions or weak evidence for generalizations about message effects are not isolated troubles or problems unique to particular research areas; instead, these stem from a common failure to grasp the indefinite describability of messages.

Misapprehension of the Task

As just indicated, the task of devising or choosing a descriptive vocabulary for messages has sometimes gone unappreciated or unacknowledged; more commonly, however, the task is noticed but in one way or another is misapprehended. There are various forms of misapprehension, but these typically share a failure to appreciate the indefinite describability of messages.

Seeking a general interaction coding system. One research area in which the task of choosing a descriptive vocabulary for messages has become apparent is the study of group interaction. There are already quite a number of different interaction-analytic coding systems. This has sometimes been approached as a matter of "coding system proliferation," and so as a problem to somehow be solved. Correspondingly one will occasionally see it suggested that researchers agree on (e.g.) some single message coding system as a way to avoid the complexities induced by the proliferation of descriptive vocabularies. Fisher (1975, p. 203), for instance, called quite explicitly for the establishment of "a general category system for interaction analysis." But this is a suggestion that makes sense only with some accompanying belief that there is some one correct general vocabulary for the description of interaction. After all, if there is some one correct way of describing interaction, then obviously the way to cope with coding system proliferation is to locate the one correct descriptive system.

But there is no one correct way of describing interaction. And one cannot hope for some "general" all-purpose category system for interactional conduct. The reason, obviously enough, is that no category system can embody all of the possible dimensions of difference among interactional behaviors (because no category system can simultaneously orient to all of the possible message features). Just which dimensions of difference are important will vary from investigation to investigation, depending on the research questions pursued--and hence there will need to be corresponding variation in the category systems employed.

The proliferation of interaction-analytic coding systems, then, is not a "problem" to be "solved," much less solved by adopting some single "general" coding system. Rather, the proliferation of these coding systems is the perfectly natural outcome of variation in the sorts of message features attended to by different investigators.

Seeking the correct compliance-gaining taxonomy. Message taxonomy "proliferation" can also be seen in the extensive research on compliance-gaining behavior, and--perhaps understandably--one can see the same sorts of reactions to such proliferation as one finds in the interaction-analytic literature, and the same sorts of manifestions of a belief in the existence of a single correct descriptive vocabulary.

A particularly clear illustration is provided by Wiseman and Schenck-Hamlin (1981). They distinguished "two approaches toward the development of taxonomies of compliance-gaining strategies" (p. 251)--a "deductive" approach (said to be exemplified by Marwell & Schmitt, 1967, and by Miller, Boster, Roloff, & Seibold, 1977) and an "inductive" approach (said to be exemplified by Clark, 1979, and by Schenck-Hamlin, Wiseman, & Georgacarakos, 1982)--with these two approaches yielding different classification schemes. Wiseman and Schenck-Hamlin argued that "because there are differences between the resultant category schemes of the two approaches, we need to determine which approach yields a better operationalization of compliance-gaining strategies" (p. 252).

But this argument is unsound, as can be brought out by imagining a simplified parallel case involving category schemes for classifying sentences. One system has three categories: interrogative, declarative, and exclamatory. The other system has two: active voice and passive voice. Surely one would not reason that "because there are differences between these category schemes, we need to determine which is the better one," and yet this is just the sort of reasoning Wiseman and Schenck-Hamlin offered with respect to influence category schemes.[2]

The implicit premise in such reasoning is that since the category schemes are different, there must be some <u>evaluative</u> difference between them--one must somehow be better (e.g., "a better operationalization") than the other. Now this implicit premise is plausible on the assumption that there is <u>one</u> correct category scheme for representing compliance-gaining strategies; if there <u>is</u> some single correct scheme, then the existence of two different schemes must indicate that at least one of them is inadequate. On the other hand, if there isn't any single correct scheme for classifying persuasive messages, then the mere existence of different classification systems can say nothing about the relative worth of the schemes.

The general point to notice is that Wiseman and Schenck-Hamlin's reasoning rests on an implicit belief that there is just one correct classification scheme for influence strategies, just one correct descriptive vocabulary to be used here--but because that implicit belief is questionable, the reasoning that rests upon it is suspect as well. One should not assume that the existence of two different message classification schemes means that at least one of them must be defective or inferior, nor should one assume that the existence of different classification schemes represents a "problem" to be overcome by establishing consensus on some single general category scheme. Assumptions such as these reflect a failure to grasp that the number of true descriptions of any message is indefinitely large, and correspondingly such assumptions represent bad grounds on which to assess possible message classification schemes.

A related instance is provided by Wheeless, Barraclough, and Stewart (1983). They recognize that the proliferation of different compliance-gaining taxonomies makes for an awfully confused research area:

> The different ways of categorizing the various possible compliance-gaining behaviors seem to defy comparison because they seem to be genetically different. . . . Upon close and thoughtful inspection, the utility of [a listing of various alternative taxonomies of compliance-gaining techniques] diminishes beyond that found in comparing apples and oranges; both are, after all, foods. It approaches the level of

> utility in comparing apples and, say, pickup trucks. . . . Because there is no standard of comparison, research cannot be compared adequately from one study to another. (pp. 117, 118)

But Wheeless et al. still think that there is some one correct compliance-gaining taxonomy to be found, one that will encompass all the varied particular extant lists--and Wheeless et al. think they've found the key that unlocks the correct compliance-gaining taxonomy:

> How then are the different lists of compliance-gaining techniques to be analyzed collectively? The answer lies further back in time, within the power literature from which compliance-gaining research emerged. (p. 118)
>
> The above analysis of bases of power provides a structure for understanding the compliance-gaining process and how it works. . . . Our derived category schema provides a basis for reconceptualizing compliance-gaining mechanisms. . . . The three general categories of power bases give us a broad, preliminary taxonomy into which specific tactics can be classified. Previously discussed problems associated with commonly used techniques . . . indicates that we probably need to start over in developing lists and typologies. However, it is initially possible to sort most previously used techniques into this preliminary taxonomy. (p. 128)

But of course there is no reason to think that Wheeless et al. have <u>the</u> correct taxonomy for compliance-gaining, or even a "preliminary" version or approximation of the correct taxonomy. This does not mean that the proffered classification scheme is without value, only that it cannot be the one correct (or best, or most nearly correct) compliance-gaining taxonomy; it cannot be such a thing because there is no such thing.

The taxonomic efforts by Wheeless et al. and by Wiseman and Schenck-Hamlin thus share a common assumption--an assumption that there is some correct taxonomy of compliance-gaining efforts that awaits identification. That assumption motivates their efforts to identify the correct taxonomy for compliance-gaining. But the assumption is unsound: the number of true descriptions of compliance-gaining attempts is indefinitely large, and no single descriptive vocabulary is _the_ correct vocabulary. And because the assumption is unsound, the search for the correct compliance-gaining taxonomy is a search that cannot be successful. When one evaluates a given message classification scheme, there is no "correct" system that it should approximate, no "correct" system against which it can be compared.

The argument thus far has been cast in terms of "messages," but it applies equally well to "strategies" (and, parenthetically, also to "tactics" or "means of influence" or suchlike). That is, the number of legitimate ways in which strategies (e.g., compliance-gaining strategies) can be described is indefinitely large, with no one of these being _the_ intrinsically correct way.

The indefinite describability of message strategies has not been appreciated in the literature. In underwriting their construction of a taxonomy of compliance-gaining strategies, for example, Miller et al. (1977) suggested that "the ability to specify unambiguously the various compliance-gaining strategies that may be invoked by communicators could aid researchers interested in studying a wide variety of persuasive problems" (pp. 38-39). But the difficulty is that there is an indefinitely large number of _different_ ways to "specify unambiguously" alternative strategies--and there is no reason to think that there is only _one_ way that is truly correct. Imagine a parallel claim: "the ability to specify unambiguously the various kinds of utterances that may be employed by communicators could aid researchers interested in studying a wide variety of communication problems." Surely no one would think that this claim could underwrite a search for some single correct classification system for utterances, and no one should think that similar reasoning can underwrite a search for some "correct" taxonomy of compliance-gaining strategies.

After all, what is the "correct" basis for creating a taxonomy of compliance-gaining strategies? Any taxonomy or classification rests on similarities and differences in the items to be classified, but which dimension is the correct one here? Is it similarities and differences in the power base employed? In the degree of listener-adaptedness exhibited? In respondents' ratings of the likelihood of use? In the influencer's attending to face wants? In the obviousness of the influencer's intent? What is _the_ correct way? Obviously, none is _the_ correct way for creating a strategy taxonomy. And just as obviously, no combination of these is _the_ correct basis for a strategy taxonomy. Compliance-gaining strategies can be described (and so cast into a taxonomy) in an indefinitely large number of ways, and no one of these is _the_ intrinsically correct way.

Hence it is not consequential whether Wiseman and Schenck-Hamlin's reasoning is designed to underwrite a taxonomy of compliance-gaining strategies as opposed to messages; it is not consequential whether Wheeless et al. and Miller et al. seek the correct taxonomy of compliance-gaining strategies as opposed to messages. There is no one correct way to taxonomize either strategies or messages, and thus efforts to locate the correct strategy taxonomy are as misguided as efforts at locating the correct message taxonomy.

But suppose that somehow one became convinced that some particular taxonomy of (say) compliance-gaining strategies was indeed _the_ correct strategy list. (As should be clear, I don't know what it would mean to be able to show that one had devised _the_ correct list of compliance-gaining strategies, but suppose that one became convinced anyhow.) This would still not represent the correct vocabulary for describing compliance-gaining _messages_. It might represent the vocabulary to be used when one wished to describe compliance-gaining messages according to the sort of strategy employed, but it would not represent the vocabulary of choice when one wished to describe compliance-gaining messages according to (say) the intensity of the language used, or the number of goals pursued, or the comprehensibility of the vocabulary employed.

This point can be put more generally: There can be nothing sufficiently special about _any_ given way of describing messages (either messages generally, or messages of a particular sort) that somehow one is compelled to use it no matter what. For instance, there is no special way of describing compliance-gaining messages such that any investigation of compliance-gaining messages is somehow bound to employ it. There is, thus, nothing special about Marwell and Schmitt's (1967) category system; there is nothing special about Clark and Delia's (1976) category system; there is nothing special about Wheeless et al.'s (1983) category system; and so on. And, to choose examples from other research domains, there is nothing special about Applegate's (1980) and Burleson's (1983) category system for comforting communication, or about Rogers and Farace's (1975) relational communication message classification scheme. A researcher interested in relational communication is not somehow compelled to use Rogers and Farace's vocabulary; a researcher investigating comforting communication is not automatically bound to using Applegate and Burleson's message coding system; and so on.

And because there can be nothing sufficiently special about a given way of describing messages that researchers are somehow compelled to employ it, it is futile to try to concoct justifications that are aimed at achieving such compulsions, justifications aimed at showing that this is the vocabulary to be used. The invocation of "strategy," as an effort after such a justification, as an effort to underwrite the special compelling nature of one vocabulary over another, is an effort that cannot succeed precisely because the number of true descriptions of any message is indefinitely large.

The motivation to search for a single descriptive vocabulary is readily understandable. Fisher's call for a "general category system" (for interaction analysis) and the efforts of Wheeless et al. and Wiseman and Schenck-Hamlin (in compliance-gaining research) are reactions to a common circumstance: the circumstance of proliferation of different descriptive vocabularies (coding systems, strategy taxonomies, etc.). In the face of a welter of (apparently) competing vocabularies, it can be tempting to suppose that what's really needed is agreement on some single scheme, where that agreement can be compelled by virtue of that scheme's correspondence with the world: "if only we can identify the right compliance-gaining taxonomy," one may think, "then clarity will come to this research area by virtue of everyone's employing that taxonomy."

But this line of thinking becomes unattractive once one gives up the belief that there is some one intrinsically correct descriptive vocabulary for messages. If there is no single correct vocabulary to be identified (or approximated), then there is no point to hoping that someday the correct vocabulary will be found. Whether in interaction-analytic work, compliance-gaining studies, or any other area of communication research, the indefinite describability of messages guarantees the existence of alternative descriptive vocabularies, with no one of these being the correct vocabulary.

Seeking coding system validity. Once one comes to grasp the indefinite describability of messages, some common ways of thinking about the evaluation of message coding systems are revealed to be unsatisfactorily formulated or misleading. One of these is the belief that one needs to establish the validity of one's coding scheme (one's taxonomy of messages, one's descriptive vocabulary).

There are, of course, various types of validity that one can see discussed in the literature (see Folger, Hewes, & Poole, 1984). But the discussion is very nearly always lodged at the level of the coding system (the classification system): the coding system is said to possess (or not possess, or possess to some degree) validity. The presumption is that having a valid coding system is a prerequisite for undertaking research; establishing the validity of one's coding system is something to be taken care of early in the game.

But this way of talking (and thinking) can lead to bad reasoning and to unnecessary confusion--and the origin of these difficulties, I think, is that it's misleading to speak of coding systems as being "valid" or "invalid." The language habits of the social sciences predispose us to speak of establishing that procedures (instruments, coding systems, questionnaires, etc.) have or lack validity, when we might more lucidly speak of gathering evidence concerning our interpretation of a procedure. "Validity," after all, describes a relationship between a procedure and an interpretation given to it. For instance, though we may be inclined to speak colloquially of questionnaire X as "having (or lacking) validity," in fact what is at issue is whether it is justifiable to _interpret_ questionnaire X _as a measure of property P_. That is, validity is a concept that is meaningful only in the context of some interpretation of some procedure. (Thus the very same instrument can be simultaneously valid _and_ invalid: valid when interpreted as a measure of one characteristic, and invalid when interpreted as a measure of some other characteristic.)

But it is crucial to distinguish the procedure (the instrument, the coding system, etc.) and the interpretation given to that procedure. A procedure and the data it produces exist independently of any particular interpretation of the procedure or the data; thus both procedures and data are open to reinterpretation. Indeed, this is particularly clearly seen in communication research that relies on message classification, where examples of coding system reinterpretation are common. For example, Folger and Poole (1982) have argued that the control codes in several popular "relational" coding schemes are better interpreted as assessing affiliation-versus-hostility; and O'Keefe and Delia (1982) have argued that many constructivist coding systems that had been interpreted as assessing "listener-adaptedness" in fact are better understood as reflecting message multifunctionality.

Thus to speak of "establishing the validity of a coding system" is misleading on two counts: First, a coding system itself can't be valid or invalid (though one can have better and worse interpretations of a coding system). Second, "validity" isn't the sort of thing that can be "established," in the sense of somehow being fixed in place. All one can do is offer (and try to support) an interpretation of a coding scheme; but the best interpretation of a coding scheme may change with the acquisition of new evidence or the appearance of new arguments.

And precisely because speaking of "establishing coding system validity" is misleading, it is also dangerous, for it can lead one to over-emphasize questions of the "validity" of a message classification scheme, at the expense of thinking about defensible interpretations of that scheme. To bring this out, imagine the following circumstance. A researcher has a message classification system that is used with high intercoder agreement. Significant and substantial correlations (say, above .70) are found between the messages people produce (as coded using the classification scheme) and a whole host of communicator characteristics: age, sex, socioeconomic status, self-monitoring, communication apprehension, etc. (the reader is invited to imagine correlations with the reader's favored communicator characteristics).

Now: does the researcher have a "valid" coding system? The question seems utterly misplaced. Whatever else is true, the investigator certainly seems to have hold of <u>something</u> important with the coding system. The trick, of course, will be describing just what it is that the coding system is detecting. That is, the problem is one of identifying the best interpretation to be given to the coding system. Some interpretations may be utterly implausible (and so discarded quickly), others may seem more likely and so be retained for closer inspection. And any interpretation offered of the coding system is a criticizable interpretation, mutable, open to the acquisition of later confirming or disconfirming evidence. But in no case does the concept of "validity" need to be invoked to understand the ongoing enterprise.

There is another way to express this point about "validity," and that is through Jackson's (1986) distinction between two views of research method.

> One view of method takes specific design and analysis procedures as rules to be followed in doing research. If the rules are followed, the results will be considered scientifically acceptable; otherwise not. . . . The second [and preferable] view takes methodology to be a way of generating arguments for empirical claims. Specific design and analysis procedures are seen not as guarantors of correct conclusions, but as routinized solutions to argumentative problems. . . . Where bodies of methodological rules exist, they can be seen to spring from deeper principles as standard solutions to anticipated counterarguments. (p. 131)

With this "method-as-argument" view in mind, the generalized requirement that a researcher establish the "validity" of a coding system can be seen as a methodological principle that derives from underlying argumentative considerations. That is, a researcher who offers evidence of the "validity" of a coding scheme in fact might most lucidly be described as offering evidence for the proffered <u>interpretation</u> of that scheme, as a means of shutting off anticipated possible alternative interpretations. Once we understand "validity" considerations in this way, it becomes obvious that as circumstances vary, so will the appropriate amount and type of "evidence for validity" that is required. In the absence of plausible alternative interpretations, an investigator who offers a <u>prima</u> <u>facie</u> plausible interpretation of a message coding scheme is not appropriately asked for additional "validity" evidence; to make such a request is to reify the "validity" requirement, and to detach the requirement from its argumentative wellsprings. As a way of underscoring the relevant argumentative point, then, the recommendation here is that evaluation of message classification systems focus not on the "validity" of the system but on the evidence concerning alternative interpretations of that system.

The importance of a focus on one's interpretation of one's classification scheme (as opposed to a focus on the scheme's validity) can also be displayed, I think, by considering another common idea about the assessment of message classification schemes: the idea that one's message coding systems must be shown to possess "representational validity."

Requiring representational validity. One criterion that has been offered for assessing message coding systems is that a satisfactory coding system must be shown to possess "representational validity." The idea of representational validity (as developed by Poole and Folger, 1981) is that (at least in most studies of human communication) one's coding system should accurately represent the shared interpretations of naive social actors; that is, the structure and content of the analyst's coding scheme should reproduce the structure and content of the subjects' interpretive scheme. By this standard, if one's message coding system doesn't have representational validity, then the coding system is defective, not to be used.

The call for "representational validity" in message coding systems is often accompanied by an invocation of (broadly put) cluster-analytic statistical procedures: factor-analysis, clustering, multidimensional scaling, and the like. It's easy to see how the idea of representational validity pretty quickly can get hooked up to the use of such techniques: it becomes attractive to use multidimensional scaling (etc.) to discover the sorts of dimensions (and thus message categories) underlying the perceptions of naive actors, and thereby automatically guarantee the "representational validity" of one's coding scheme.

But the requirement of representational validity is not a sound one. Consider an investigation in which I have respondents rate various sentences on some scale (or do paired-comparisons for the sentences), and then I analyze the data to extract the underlying dimensions of judgment. And suppose that (as seems plausible) there is no extracted dimension that corresponds to the difference between active- and passive-voice sentences--even though the sample of sentences contained both active and passive sentences. What should we conclude from this? Obviously, we shouldn't conclude that there really isn't any difference between active and passive sentences, or that the distinction between active and passive sentences is worthless. And correspondingly we ought not conclude that a coding scheme that classifies sentences as "active" or "passive" is not a good coding scheme. And we should not conclude that active and passive sentences will always have identical _effects_ on hearers, nor should we conclude that there are no regularities to be discovered concerning the _production_ of active as opposed to passive sentences.

What _should_ we conclude about a coding system from the results of such cluster-analytic work? Not much, it seems. At the very best, such work might give information relevant to answering the question of whether the sorts of discriminations made by the respondents are somehow related to the discriminations made by the coding system, but even this concedes too much. After all, there are likely to be discriminations that naive actors make that are not picked up by the particular cluster-analytic procedures employed.

What this suggests is that researchers should not worry about whether their message classification schemes have "representational validity." Whether a message classification scheme has "representational validity" says nothing about that scheme's usefulness, appropriateness, or importance. To be sure, information from clustering techniques applied here may well bear on possible interpretations that one gives to one's message classification schemes, but even here the path is fraught with difficulty. As is well known, different dimensional structures can be obtained with variations in instructions, contexts, sets of objects to be rated, and the sorts of scales used to obtain ratings (e.g., a researcher may obtain one grouping of persuasive techniques if respondents rate techniques for likelihood-of-use, but a different grouping if the techniques are rated on some different quality). The general point is that information about the extent to which a message classification scheme reflects discriminations seemingly made by naive social actors may be information that sheds some light on some questions concerning the classification scheme, but such information is not decisive in determining the worth or soundness of the scheme.

One reason for the appeal of a criterion such as "representational validity" is a subtle, tacitly-held version of the belief in the existence of a single correct descriptive vocabulary. The tacit belief in question is that a person's mind (or brain) secretly contains the correct description (or all of the correct descriptions) of each act the person performs, and hence an investigator must somehow seduce the person into revealing the vocabulary of those descriptions (because only then will the investigator obtain the "correct" descriptive vocabulary). Consider, for instance, how someone who followed this sort of reasoning might proceed in identifying the "correct" compliance-gaining taxonomy: one would elicit the taxonomies of naive social actors (perhaps directly, perhaps through some indirect means--such as paired-comparison similarity ratings--that might give evidence about naive taxonomies), collate these somehow, and then declare that the resulting category system is the preferred way of classifying compliance-gaining attempts. And the defect with this all-too-recognizable way of proceeding is, again, a failure to recognize that the number of true descriptions of any message is indefinitely large, and that no single descriptive vocabulary can be thought of as the "best," the "correct," the "most nearly correct," or the "most useful" vocabulary no matter the purpose.

There is another avenue to seeing the difficulties of "representational validity" as a criterion for the assessment of descriptive vocabularies, and that is by noticing that any descriptive vocabulary that permits the location of behavioral regularities might *prima facie* be said to possess "representational validity." If one believes that regularities in conduct stem from the underlying mechanisms of behavioral production and that the underlying mechanisms of behavioral production are to be located in the actor's cognitive machinery, then *any* identifiable behavioral regularity must somehow be connected to the cognitive machinery that putatively gave rise to it.[3] Hence any message coding system that locates behavioral regularities must in some sense be reflecting corresponding regularities in the actor's cognitive machinery, and thus (on its face, at least) the coding system presumably qualifies as "representationally valid."

In fact, reasoning such as this is not uncommon. Some investigator devises concepts that appear to capture important behavioral regularities; these regularities are then presumed to be the issue of the underlying cognitive mechanisms of behavioral production; and hence efforts are made at showing how the initial descriptive concepts can be fitted into a picture of the cognitive system. For example, concepts such as "first pair part" and "transition-relevance place," though not invoked by naive social actors to describe their own communicative conduct, arguably do permit analysts to detect behavioral regularities. The very detection of such regularities leads investigators to presume that somehow something like these concepts must be embedded in the cognitive machinery of actors (otherwise how would the regularities arise?). Or, as another example, analysts of conversation find it useful to invoke concepts connected with topic and topical coherence as ways of capturing certain regularities in conversation; but the existence of such behavioral regularities suggests something about the mechanisms by which conversational conduct is produced, and so (e.g.) Planalp and Tracy (1980) can confidently pursue the question of a cognitive approach to the management of conversation (after all, the relevant discriminations must be cognitively represented somehow, otherwise where would the observed conversational regularities come from?). That is to say, the very ability of a descriptive vocabulary to identify behavioral regularities is commonly taken as evidence that something like the elements of that vocabulary are already represented in the cognitive systems of social actors.

All told, then, a general requirement of "representational validity" for message classification systems is an unsatisfactory criterion for the assessment of such systems. A coding system that does not correspond to (say) the dimensions extracted from a clustering of respondent-perception data is not automatically a system that cannot detect significant behavioral regularities; and even a coding scheme that is composed of the dimensions that appear to underlie naive perceptions can give no guarantee that one can detect significant behavioral regularities with that scheme, nor can it guarantee that there are no other coding schemes capable of detecting significant regularities. Thus what's usually taken as demonstrating satisfaction of the criterion of representational validity in fact does not bear on the question of the utility or desirability of a descriptive vocabulary. And if a broader view is taken, any descriptive vocabulary that locates behavioral regularities might plausibly be said to somehow reflect underlying discriminations made by naive social actors, and hence could be said to possess representational validity; but this obviously makes "representational validity" a pretty empty requirement.

Seeking to compel certain distinctions. We have already seen that, in approaching the task of message description, some have thought that there exists a single correct descriptive system for messages, and hence have sought to identify that system and so compel its employment. But something similar is also visible on a smaller scale: an investigator notices that a given descriptive vocabulary fails to employ some particular descriptive dimension or facet, and so argues that the vocabulary is thereby defective. The implicit assumption seems to be that if a descriptive distinction *can* be recognized then one is *compelled* to include it in one's descriptive system (never mind whether the distinction is relevant to the research purposes at hand). In fact, of course, there is nothing so special about a given descriptive distinction (or about a given collection of descriptive distinctions, a descriptive vocabulary) that one is somehow compelled to employ it no matter what.

Consider, for example, this complaint about Wiseman and Schenck-Hamlin's (1981) compliance-gaining scheme: "Important strategic variations [in the discourse of requests], especially ones related to speech act felicity conditions and the satisfaction of 'face wants' distinct from the goal of compliance, are not captured very well within the compliance-gaining framework [of Wiseman and Schenck-Hamlin]" (Tracy, Craig, Smith, & Spisak, 1984, p. 513). It is important to see what such an argument (if accepted) does and doesn't show. Such an argument doesn't show that Wiseman and Schenck-Hamlin's compliance-gaining taxonomy is somehow intrinsically defective or that the taxonomy shouldn't be used in research. All it shows is that <u>certain</u> descriptive dimensions (concerning, e.g., face wants) aren't represented in the taxonomy. But this is not something wrong with the taxonomy. After all, no taxonomy can capture <u>every</u> message feature, so the absence of some particular feature is no strike against a taxonomy.

Notice, too, that what dimensions of difference in discourse are taken to be "important variations" and which "unimportant" will surely vary from research question to research question. For answering certain sorts of questions about the discourse of requests, it may be critical to attend to differences in the satisfaction of face wants; but for other sorts of questions even about that same body of discourse, that particular descriptive dimension may be utterly irrelevant. Hence one cannot justifiably say that, as a general matter, Wiseman and Schenck-Hamlin's taxonomy misses some "important" dimension of compliance-gaining; whatever descriptive dimensions of compliance-gaining are absent from any given taxonomy cannot be said to be "important" dimensions in general, but only important with respect to investigating certain sorts of questions.

Another example is provided by Cody, McLaughlin, and Jordan (1980), whose cluster-analytic and multidimensional-scaling research yielded categories for compliance-gaining efforts that were absent from earlier compliance-gaining taxonomies. They concluded that, for instance, "a category of manipulation strategies ought to be incorporated into research on compliance-gaining strategy selection" (p. 44), and more generally that "in future studies on compliance-gaining strategies, researchers ought to take care to include representative examples from the direct-rational, exchange, manipulation, threat, and/or expertise-claims categories" (p. 45). But the reasoning here is parallel to that of Tracy et al., and suffers from the same problem. The essential suggestion is that every compliance-gaining classification scheme should include certain descriptive distinctions, certain descriptive categories, or else it is somehow deficient or defective or incomplete. But just what distinctions a researcher wants to draw in classifying compliance-gaining efforts--and just what categories are thereby appropriate--will depend upon the questions the researcher is trying to answer; it is not possible to specify in advance and for all time just what features of compliance-gaining efforts can or should or must be studied.[4]

Summary

So: sometimes researchers haven't sufficiently appreciated the task of choosing a message description system; and even when they have noticed the task, too often they haven't thought about it clearly because they haven't firmly grasped the indefinite describability of messages. So what are we to do?

Emphasizing the Research Utility of Descriptive Vocabularies

As we have seen, there are a number of difficulties with current conceptions of how the task of assessing or selecting descriptive vocabularies for messages is to be approached. So what does all this mean for how we *should* proceed?

First, we should come to fully appreciate that a message can appropriately be described in an indefinitely large number of ways, with no one of these being *the* intrinsically correct way. This is a central fact about messages, and should be faced squarely. Refusing to face this fact squarely results in avoidable mistakes such as malformed research questions and research designs; being sensitive to this fact about messages would improve both the kinds of questions asked about communication and the means selected for answering questions.

Second, we should adopt a more pragmatic attitude toward the assessment of descriptive vocabularies. We should not fool ourselves into thinking that the utility (much less the correctness) of a given vocabulary will somehow be guaranteed by its derivation from a factor analysis, or into supposing that the proliferation of different message coding systems represents a problem to be solved by enforcing agreement on some single system, or into believing that just because it is possible to employ a given descriptive distinction that such employment is somehow mandated. And we should not assess message description systems as though there is some "correct" descriptive scheme that any proffered system should somehow approximate.

Rather, descriptive vocabularies should be assessed (first and foremost, although not necessarily exclusively) in terms of their research utility, and particularly in terms of their ability to identify interpretable regularities. This criterion is purposefully broad and general (in the sense that it does not specify just what sorts of regularities might be found or are worth finding), but it is nevertheless a pointed one (in saying that what matters is a scheme's utility in identifying empirical regularities).

The requirement that regularities be _interpretable_ follows directly from the pragmatic requirement that descriptive vocabularies prove themselves empirically. But one's chances for locating interpretable regularities are maximized (ceteris paribus) when one's descriptive vocabulary is arranged in a conceptually organized and coherent way at the outset. If a message classification scheme is a hodge-podge of categories with no underlying conceptual organization, then any "findings" derived from that scheme will be difficult to interpret meaningfully.[5]

Hence, even though the utility of a descriptive vocabulary can only be determined in light of its ability to illuminate communication processes, one can make some _a priori_ reasoned assessments of the likely usefulness of a given descriptive vocabulary. In particular, only a coherently constructed scheme is likely to permit one to offer a plausible interpretation of the sorts of dimensions of difference that animate a classification; and without some plausible interpretation of the classification scheme, it will be difficult to give a coherent description of any "regularities" that emerge from the scheme's use. Since interpretable regularities are the point of a descriptive vocabulary, and since the interpretability of findings depends on the coherence of the system used in describing messages, researchers should take pains to insure that their message classification systems offer clear and coherent contrasts and orderings of messages; such preparation of the descriptive vocabulary for interpretation (and not illusory evidence of "validity") is an essential step toward interpretable findings.

In short, then, what one wants from a descriptive vocabulary for messages is that it be useful in research. One can make some assessment of the possible utility of a vocabulary in advance (by inquiring about the bases of its construction, its conceptual coherence, and the like), but even this assessment will indirectly be focused on research utility, because it will concern the likelihood of locating interpretable regularities with the vocabulary.

Third, rather than discouraging the "proliferation" of alternative descriptive vocabularies (e.g., alternative interaction analysis coding systems, or alternative compliance-gaining taxonomies) we should be receptive to the development of new descriptive vocabularies that identify new message features in a coherently organized fashion. To be sure, the proliferation of vocabularies can create something of a difficulty, in that it can make reaching dependable conclusions more difficult (if too little research is done with each of a large number of unrelated message coding systems, for instance). On the other hand, a new descriptive vocabulary offers the possibility of uncovering new empirical regularities, phenomena not otherwise visible, and this surely is not to be disregarded. But the proliferation of vocabularies is not to be coped with by locating the one <u>correct</u> vocabulary that all researchers will share. We do not need to agree on a general category system for interaction analysis, we do not need to find some all-encompassing taxonomy of compliance-gaining efforts: we should not seek the one correct descriptive vocabulary as a way of dealing with the proliferation of message taxonomies.

Now it may come about that a given vocabulary turns out to be useful to a large number of investigators, turns out to solve many researchers' problems, and so turns out to be widely and commonly used. This is well and good, but one should not think that this state of affairs comes about because the one true vocabulary has finally been located. If one evaluates vocabularies by their utility in research, some vocabularies may emerge as more valuable than others, but this will be the natural outcome of trying out different vocabularies and seeing the results.

Finally, we should recognize that message description is as much a theoretical as a methodological task. Evaluating message description systems on the basis of their contribution to our general understanding of communication processes might seem like an obvious and natural way of proceeding. But as plausible as this simple evaluative standard may appear, many researchers (as we've seen) approach the assessment of message classification systems from a very different perspective. They operate on the premise that they need to locate the one correct vocabulary, or they want evidence for the "validity" (especially the "representational validity") of a coding scheme, or they hope that statistical procedures will somehow automatically create the right set of categories. A simple emphasis on research utility, however obvious, has not been at the forefront of researchers' thinking about how to assess descriptive vocabularies for communicative conduct.

Of course, the view offered here means there's no easy answer to the problem of locating a descriptive vocabulary. There's not any procedure, statistical or otherwise, that will magically solve the problem of selecting or creating or evaluating a message classification system. And no matter how one initially devises one's classification scheme, there's no basis for assuming that one's interpretation of that scheme can somehow be established for all time as the proper one. But this is only to say that the task of finding useful message descriptions is perhaps even more difficult than we might already have thought.

It's unfortunate that the task of message description seems to have been (implicitly, at least) thought of primarily as a "methodological" matter, as something bound up with validity questions and coding system reliabilities and factor-analytic procedures. This is unfortunate, because describing messages is not some small matter of methodology, but is a major theoretical project. Indeed, it may not go too far to say that message description and analysis is the central theoretical task to be addressed in communication research. This task cannot be appreciated or apprehended, however, without a fundamental recognition of the indefinite describability of messages.

Notes

1. Is a more charitable interpretation of these quoted research questions possible? Perhaps one might think that these excerpts have been "taken out of context," and that actually the investigators _do_ have some particular message feature in mind. But if these investigators did have some specific facet or aspect of persuasive efforts in mind, surely that facet would be clearly identified in the statement of the research question. But none of these research questions specifies the feature of persuasive efforts under investigation, which suggests that (as the text indicates) the authors do not see that the indefinite describability of messages requires such specification.
2. One should not think that there is something special about Wiseman and Schenck-Hamlin's discussion because it concerns taxonomies for influence _strategies_, not just taxonomies of influence types or categories generally. This matter is discussed below.
3. Notice that someone who didn't accept the two premises in the antecedent clause of this sentence would probably not find representational validity a plausible criterion anyway.
4. A reminder: It won't help here to say "what if the researcher is interested in studying message _strategies_? Won't _that_ underwrite mandating the inclusion of certain categories in some taxonomy of strategies?" As discussed above, there is no single intrinsically correct description of message strategies; just how one wants to classify message strategies (according to the power base employed,

according to the likelihood of use, etc., etc.) will vary depending on the question one is investigating, and hence invocation of the concept of "strategy" will not underwrite mandating inclusion of particular descriptive dimensions or categories.

5. One is put in mind of the invocation by Foucault--for a different purpose--of the passage in Borges that quotes "a certain Chinese encyclopedia" in which it is written that "animals are divided into: (a) belonging to the Emperor, (b) embalmed, (c) tame, (d) sucking pigs, (e) sirens, (f) fabulous, (g) stray dogs, (h) included in the present classification, (i) frenzied, (j) innumerable, (k) drawn with a very fine camelhair brush, (l) *et cetera*, (m) having just broken the water pitcher, (n) that from a long way off look like flies" (Foucault, 1970, p. xv).

References

Applegate, J. L. (1980). Adaptive communication in educational contexts: A study of teachers' communicative strategies. Communication Education, 29, 158-170.

Bragg, B. W. E., Ostrowski, M. V., & Finley, G. E. (1973). The effects of birth order and age of target on use of persuasive techniques. Child Development, 44, 351-354.

Burleson, B. R. (1983). Social cognition, empathic motivation, and adults' comforting strategies. Human Communication Research, 10, 295-304.

Cissna, K. N. (1984). Phases in group development: The negative evidence. Small Group Behavior, 15, 3-32.

Clark, R. A. (1979). The impact of self interest and desire for liking on the selection of communicative strategies. Communication Monographs, 46, 257-273.

Clark, R. A., & Delia, J. G. (1976). The development of functional persuasive skills in childhood and early adolescence. Child Development, 47, 1008-1014.

Cody, M. J., McLaughlin, M. L., & Jordan, W. L. (1980). A multidimensional scaling of three sets of compliance-gaining strategies. Communication Quarterly, 28(3), 34-46.

Fisher, B. A. (1975). Communication study in systems perspective. In B. D. Rubin & J. Y. Kim (Eds.), General systems theory and human communication (pp. 191-206). Rochelle Park, NJ: Hayden.

Fisher, B. A., Glover, T. W., & Ellis, D. G. (1977). The nature of complex communication systems. Communication Monographs, 44, 231-240.

Folger, J. P., Hewes, D. E., & Poole, M. S. (1984). Coding social interaction. In B. Dervin & M. J. Voigt (Eds.), Progress in communication sciences: Vol. 4 (pp. 115-161). Norwood, NJ: Ablex.

Folger, J. P., & Poole, M. S. (1982). Relational coding schemes: The question of validity. In M. Burgoon (Ed.), Communication yearbook 5 (pp. 235-247). New Brunswick, NJ: Transaction.

Foucault, M. (1970). The order of things: An archeology of the human sciences. New York: Pantheon Books.

Instone, D., Major, B., & Bunker, B. B. (1983). Gender, self confidence, and social influence strategies: An organizational simulation. Journal of Personality and Social Psychology, 44, 322-333.

Jackson, S. (1986). Building a case for claims about discourse structure. In D. G. Ellis & W. A. Donohue (Eds.), Contemporary issues in language and discourse processes (pp. 129-147). Hillsdale, NJ: Erlbaum.

Jackson, S., & Jacobs, S. (1983). Generalizing about messages: Suggestions for design and analysis of experiments. Human Communication Research, 9, 169-181.

Marwell, G., & Schmitt, D. R. (1967). Dimensions of compliance-gaining behavior: An empirical analysis. Sociometry, 30, 350-364.

Michener, H. A., & Schwertfeger, M. (1972). Liking as a determinant of power tactic preference. Sociometry, 35, 190-202.

Miller, G., Boster, F., Roloff, M., & Seibold, D. (1977). Compliance-gaining message strategies: A typology and some findings concerning effects of situational differences. Communication Monographs, 44, 37-51.

O'Keefe, B. J., & Delia, J. G. (1982). Impression formation and message production. In M. E. Roloff & C. E. Berger (Eds.), *Social cognition and communication* (pp. 33-72). Beverly Hills: Sage.

Planalp, S., & Tracy, K. (1980). Not to change the topic but . . . : A cognitive approach to the management of conversation. In D. Nimmo (Ed.), *Communication yearbook 4* (pp. 237-258). New Brunswick, NJ: Transaction.

Poole, M. S. (1981). Decision development in small groups I: A comparison of two models. *Communication Monographs*, *48*, 1-24.

Poole, M. S., & Folger, J. P. (1981). A method for establishing the representational validity of interaction coding systems: Do we see what they see? *Human Communication Research*, *8*, 26-42.

Rim, Y. (1980). Sex-typing and means of influence in marriage. *Social Behavior and Personality*, *8*, 117-119.

Rogers, L. E., & Farace, R. V. (1975). Analysis of relational communication in dyads: New measurement procedures. *Human Communication Research*, *1*, 222-239.

Schenck-Hamlin, W. J., Wiseman, R. L., & Georgacarakos, G. N. (1982). A model of properties of compliance-gaining strategies. *Communication Quarterly*, *30*(2), 92-100.

Stech, E. L. (1977). The effect of category system design on estimates of sequential and distributional structure. *Central States Speech Journal*, *28*, 64-69.

Tracy, K., Craig, R. T., Smith, M., & Spisak, F. (1984). The discourse of requests: Assessment of a compliance-gaining approach. *Human Communication Research*, *10*, 513-538.

Wheeless, L. R., Barraclough, R., & Stewart, R. (1983). Compliance-gaining and power in persuasion. In R. N. Bostrom (Ed.), Communication yearbook 7 (pp. 105-145). Beverly Hills: Sage.

Wiseman, R. L., & Schenck-Hamlin, W. (1981). A multidimensional scaling validation of an inductively-derived set of compliance-gaining strategies. Communication Monographs, 48, 251-270.