CHAUTAUQUA: WHY ARE THERE SO FEW COMMUNICATION THEORIES?

COMMUNICATION THEORIES AND OTHER CURIOS

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Over the past two decades, with undulating regularity, considerable numbers of communication researchers have focused their attention on metatheoretical issues germane to the study of human communication. In one of the early waves of this tide, Cushman (1977) and others (see Benson & Pearce, 1977) provoked lively interchanges among advocates for covering-laws, systems, and rule-governed approaches to the study of human communication. More recently, even larger scale efforts have been made to negotiate a paradigm, or a set of paradigms for the study of human communication processes (Dervin, Grossberg, O'Keefe, & Wartella, 1989a,b). A common factor motivating both of these potentially field-defining efforts is the unease that many share concerning the state of development of communication theories. Two concerns in particular surface in these discussions. First, the traditionally high level of fragmentation manifested by the field seems to be increasing as the field expands. Although specialization is almost an inevitable consequence of growth, the fact that there is no particular theoretical paradigm or touchstone theory around which communication researchers might organize their efforts is at least one source of concern. Many disciplines have such paradigms or touchstone theories, even though these disciplines show high levels of specialization, and even though these paradigms and theories may be in dispute.

In the case of communication, not only is there relatively little commerce among the various sub-areas of the field, that is, interpersonal, mass, organizational, political, health, instructional, and so on, there is apparently no common body of theory that unites research conducted in these ostensibly unique communication contexts. It has been observed that the process of organizing the field of communication by contexts itself countervails against the development of general theories of human communication (Berger & Chaffee, 1987). The implicit message of context as an organizing principle is that communication phenonema that occur in each context are so unique that context-specific theories are needed to explain them. This untested assumption is not as plausible as it may seem, and, as a matter of practicality, when one critically examines research reported on the convention programs of some of these contextually defined sub-areas, one wonders why that research could not just as plausibly have been presented on the programs of another sub-area. Many, and perhaps most, of the research reports presented under the rubric of health communication, for instance, could be accommodated within the domains of

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interpersonal and mass communication, and there are numerous examples of potential opportunities for the integration of research done in interpersonal and mass communication contexts themselves (Berger & Chaffee, 1988; Reardon & Rogers, 1988; Wiemann, Hawkins, & Pingree, 1988).

A second concern, one that has been echoed frequently in many quarters, is that not only is there no particular theoretical core to the field of communication, but there has been little evidence of theoretical activity by communication researchers within any of the particular contexts which define the field (Berger & Chaffee, 1987; Craig, 1988; Hart, 1986). Bibliometric studies of journal citations have revealed extensive Balkanization within the field, especially between those trained in the speech and journalism traditions. More importantly, these studies have produced compelling evidence that the field of communication has been suffering and continues to suffer from an intellectual trade deficit with respect to related disciplines; the field imports much more than it exports (Reeves & Borgman, 1983; Rice, Borgman, & Reeves, 1988; So, 1988). My intent is not to spend the remainder of this paper decrying and documenting the relative lack of original theory development in the communication field; rather, I will attempt to provide at least a partial explanation for this well-documented state of affairs. Under the assumption that it is easier to solve problems when their causes are known, I hope this theoretical explication itself will set in motion processes that will at least partially ameliorate the situation. I believe that the future growth and well-being of the field depend upon the ability of communication researchers to advance ideas and theories that are taken seriously by colleagues in related disciplines. Such developments not only ensure the crucial support of colleagues from other disciplines that is vital for continued development of the field, they also have favorable effects on the self-esteem levels of the inhabitants of our field, which at times tend to be unjustifiably low.

Before addressing potential explanations for the relative lack of theory development by researchers in the field, it should be noted that there are some examples of relatively unique theory development efforts by persons trained as communication researchers. For example, action assembly theory (Greene, 1984), constructivism (Applegate, 1990), coordinated management of meaning (Pearce & Cronen, 1980), discrepancy arousal theory (Cappella & Greene, 1982), interpersonal and intergroup communication theory (Gudykunst, 1988); nonverbal expectancy violations theory (Burgoon, 1978), speech accommodation theory (Giles, Mulac, Bradac, & Johnson, 1987), and uncertainty reduction theory (Berger & Calabrese, 1975; Berger, 1979, 1987) have had varying levels of impact within the field. Unfortunately, few of these theories have had much influence beyond the boundaries of the field. This lack of cross-disciplinary outreach notwithstanding, these and other efforts represent important first steps toward filling the current theoretical void.

My intent here is neither to argue that theory development should be made a sub-speciality of the communication field, as some seemed to suggest during early meta-theoretical forays, nor that an elite coterie of communication scholars should be charged with generating communication theories. To the contrary, the ideal situation would be one in which theory development is made an integral part of the training of all communication researchers so that the roles of theoretician and researcher might be played simultaneously by the same individual. Furthermore, I am not contending that more theory is necessarily better, as demonstrated by the fact that on the eve of the demise of attitude change and persuasion research, Ostrom (1968) counted some 34 different theories of attitude change. Although many of these theories were hardly full-blown theories of persuasion, this high level of conceptual activity failed to ensure the vitality of the attitude change research enterprise and may have contributed in some ways to its demise. Nonetheless, at this juncture in the development of the communication field we are hardly in danger of being buried under an avalanche of original communication theories. In the space that remains, I will identify some of the reasons for the lack of theory in the field and some steps that might be undertaken to improve this situation.

WHY HAS THIS HAPPENED?

Historical Legacies

Space does not allow a complete description of the development of communication research as a social scientific enterprise, although Delia (1987) has provided the most comprehensive and insightful account of this development to date. One important feature of his historical account is the fact that the origins of present day communication research can be traced to influences emanating from several other disciplines. For instance, Delia (1987) pointed to the significant role played by the Chicago school of sociology in the development of mass communication effects research, and the later influences of social psychology in the development of the field. These extra-field roots were also discussed by Schramm (1963) when he designated psychologists Carl Hovland and Kurt Lewin, political scientist Harold Lasswell, and sociologist Paul Lazarsfeld, as the "founding fathers" of communication research. Reinforcing the view that communication is "an academic crossroad where many have passed, but few have tarried" (Schramm, 1963, p. 2), is the fact that none of the 11 contributors to Schramm's edited volume titled *The Science of Human Communication* were persons trained as communication researchers; these contributors were political scientists, psychologists, and sociologists. The appropriation of communication research by speech and journalism departments over the last 30 years has produced considerably more permanent residents at the figurative crossroad of which Schramm spoke; however, these extra-field roots still exert considerable influence on present day communication researchers. What are these residual influences?

First, because of the melange of disciplines that were seen to have some relevance to the study of communication at the time the Schramm volume was published, graduate students being socialized into the field of communication were strongly encouraged to take course work in "relevant" cognate areas. During the 1960's these opportunities for interdisciplinary wanderings served as an attractive alternative to committing one's self to the relatively parochial range of possibilities offered by traditional social science disciplines. Moreover, there was a strong belief that communication is so central and vital to most social processes that, over time, communication might well become a kind of supradiscipline that would integrate the other social sciences. Schramm (1963) provided a glimpse of this vision when he wrote,

> . . .communication is a—perhaps *the*—fundamental social process. Without communication, human groups and societies would not exist. One can hardly make theory or design research in any field of human behavior without making some assumptions about human communication. (p. 1)

The reality that has unfolded since the promulgation of this vision has not quite met these heady expectations. It is still true that in their graduate education students of human communication may range far and wide across the disciplines in search of insights about communication processes. What has been sorely neglected since the early days, however, is recognition of the necessity for a synthesis of these interdisciplinary forays into home-grown theories of communication. Even worse is the still pervasive tendency for communication researchers to conclude these interdisciplinary wanderings by becoming fixated on one cognate area such as anthropology, political science, psychology, or sociology. Once such commitments have been made, communication researchers become mere hypothesis testers for theoreticians in the cognate areas. Moreover, such fixations may lead simultaneously to unrealistic veneration of "great theoretical minds" in cognate disciplines and to unwarranted skepticism about one's own ability, as well as the abilities of one's colleagues in the field. This is not to say that communication scientists should ignore relevant theoretical developments in cognate disciplines; however, it is one thing to integrate such developments into one's theory, but quite another simply to test deductions from theories developed by investigators working in cognate areas. Unfortunately, this latter course of action is the one followed by a considerable number of communication researchers.

A second consequence of this interdisciplinary legacy has been the development of the view by some that communication research is an applied social science. Certainly, since the early days, a great deal of mass communication research has had a strongly applied flavor, as has a considerable amount of research dealing with face-to-face interaction in organizational, instructional, and health communication contexts. Of course, if one views the communication research enterprise merely as the application of theories developed in other disciplines to "communication problems," then there is no necessity for communication researchers to develop their own theories of communication. In general, applied researchers are interested in finding solutions to practical problems. Although there is no fundamental reason for bifurcation of theoretical and applied research, it is true that some applied researchers eschew any reference to theory, at least formal theory, and some theoretically oriented researchers similarily condemn applied research as being "atheoretical." Setting such unproductive battles aside, by defining communication research solely as an applied social science, one lets one's self and one's field off of the theory development hook.

Finally, the appropriation of communication research by speech and journalism departments over the past 30 years (Delia, 1987) may partially explain the current lack of theoretical growth in the field. One of the strong traditions of both speech and journalism has been the teaching of various communication skills to undergraduate students. In speech, this mission has recently been expanded to the teaching of these skills to those employed by formal organizations. The legacy of the skills emphasis, and its continuation, structures the role expectations of colleagues outside of the field in such a way that these colleagues tend to view those affiliated with communication departments primarily as purveyors of communication skills rather than as researchers. Communication researchers seeking to redefine the role expectations of their colleagues may either unconsciously fall prey to the expectations of their students and colleagues and continue only to purvey skills, or be unable to muster the requisite energy to alter such expectations. Persons fighting these basic academic identity battles, and there are more than might be supposed, cannot be expected to be productive theoreticians or researchers.

While many actively attempt to alter these role definitions, others, unwittingly perhaps, reinforce them. Here I am thinking of those who define scholarship mainly in terms of the production of introductory textbooks that aid in the teaching of skills. There is nothing inherently wrong with producing such texts, as long as they are of respectable quality, and as long as it is recognized that within the context of a scholarly community, such contributions should not be valued as highly as original contributions to theory and research. Synthesizing the work of others in textbook form, especially in the form that a large number of introductory texts in the speech field manifest, simply is not as demanding a task as generating and testing original theory. Yet, some scholars in the field have defined their careers largely by the generation of such introductory texts and have been rewarded significantly for doing so. Again, these observations are not meant to demean the importance of introductory textbooks; however, they are meant to indicate that such contributions need to be evaluated within the proper value system, one that ranks the generation of new ideas over the recycling of old ones. Furthermore, it is doubtful that the field of communication has gained or will gain a great deal of respect from relevant cognate areas by producing a literature consisting mainly of introductory textbooks, no matter how well written these textbooks may be.

The Methodological Fixation

Even a cursory glance at current communication research suggests that, as a group, communication researchers are competent users of the latest techniques for collecting and analyzing data. In fact, in some ways those who study social interaction within the communication field are far ahead of their like-minded counterparts in social psychology; where, paradoxically, social interaction behavior itself is infrequently the object of study. In fact, over the past 30 years, communication researchers educated at major research institutions generally have received excellent training in the tools of social science research. Over this same period of time, the field has been visited by a series of statistical and non-statistical techniques, which, at the time of their introduction, have been heralded as panaceas for achieving new insights into the communication process. While ethnomethodology, factor analysis, lag sequential analysis, log linear analysis, meta-analysis, multidimensional scaling, structural equation modeling, and other floats in this parade of techniques have elicited squeals of adulation from their admirers, these techniques have not necessarily produced insights about communication that are commensurate with the levels of hyperbole demonstrated by their advocates.

Perhaps, this almost obsessive preoccupation with methodology in some quarters of the field can be explained, in part, by recourse to the historical trends discussed previously. Given the theoretical roots of the field in several cognate disciplines, all that is necessary to become a well educated communication researcher is to familiarize one's self with the appropriate body of theory in related disciplines and to learn to use methodological techniques well. Since one does not bear the responsibility for developing one's own theory, one can always look to theories in related disciplines for research ideas; thus, all that one has to do is become a good methodologist, producing the methods fixation as a consequence. Essentially, then, this pattern of socialization tends to produce yeoman hypothesis testers who have not been appraised of the fact that there is more to doing good research than simply evaluating someone else's hypotheses, even when these hypotheses are tested with great methodological panache. The generation of new ideas and new theories is an activity that is at least as, if not more important than, becoming well schooled in the use of various data collection and data-analytic techniques.

Another negative consequence that flows from the methodological fixation is the possibility that in their graduate educational experiences, some students are inundated with methods courses that crowd out courses that might be useful for learning principles of theory construction. It is fair to say that in most graduate programs, the number of courses devoted explicitly to theory construction is far outweighed by the number of courses devoted to the teaching of various techniques of data collection and data analysis. In many cases, there may be no single course devoted to the teaching of theory generation techniques. Such an imbalance produces methodologically sophisticated researchers who would not know a theory if they stumbled over one. This lack of familiarity with even the basic concepts associated with the notion of theory leads some researchers to label erroneously any idea or hypothesis a "theory." Such instances vividly demonstrate that some researchers simply have never learned what a theory is, let alone how to construct one.

The fixation on methods not only tends to displace consideration of theory building in the education of graduate students, it creates a "have methodology, will travel" mindset that propels those afflicted with it to use their methodological expertise to research any problem, as long as the research is funded. Contract research and a considerable amount of research done under the aegis of grants from various agencies is aimed at solving relatively narrow, applied problems, many of which have little theoretical relevance. In a great majority of instances, within the communication field, theory is not directly tested in most funded research efforts. In fact, few, if any, of the theory development programs of communication researchers listed earlier in this paper were spawned within the context of externally funded research. Although external funding might provide an environment for theory development and testing, the "methodological hired gun" mentality certainly countervails against this possibility.

The methodological fixation also manifests itself in discussions of the relative merits of qualitative and quantitative approaches to the study of communication. In the present view, such debates are ill-directed and only serve to obscure the vastly more important issue of the scientific value of empirical data of any kind gathered in the absence of theory. As Popper (1974) suggests,

> More especially, there is no way that starts from observation or experiment. In the development of science observations and experiments play only the role of critical arguments.

And they play this role alongside other, non-observable arguments. It is an important role; but the significance of observations and experiments depends *entirely* upon the question whether or not they may be used to *criticize theories*. (pp. 151–152)

Until more communication researchers, regardless of their methodological commitments, become preoccupied with important theoretical ideas rather than continuing their methodological nit-picking, we will continue to see the proliferation of methodologically elegant research that addresses pedestrian ideas. To clarify, when I say "important theoretical ideas" I am not referring to the meta-theoretical debates discussed earlier; rather, I mean substantive theories that are purported by their creators to explain communication phenomena.

The preceding discussion is not meant to discount the importance of methodological rigor; however, when such rigor substitutes for theoretical insightfulness, as it frequently does, efforts must be made to redress this imbalance. In the final analysis, the worth of a field of study is determined more by the cogency of the ideas it contributes to a body of knowledge than by the ability of its members to use trendy data collection and data-analytic techniques. After all, modes of data collection and data analysis used by communication researchers are used by investigators in several disciplines. The *unique contributions* of our field to the universe of discourse, of which we are all part, should be theories, ideas, and new insights about the workings of human communication systems.

Risk Aversion

The construction and dissemination of theory is a high risk venture. When one proposes a theory, others can test the implications of the theory; assuming, of course, that the implications of the theory are indeed testable. Inevitably, at least parts of, or perhaps the entire theoretical system will be shown to be implausible. The potential for presentation of evidence that undermines one's theoretical thinking, especially in the public domain of journals and books, may represent a threat to the theory creator's ego. By contrast, testing others' theories, with the latest techniques—to boot, is considerably less threatening to one's academic ego. If the results of the tests render the theory's hypotheses more plausible, the theory tester may somehow win points from the theory developer. On the other hand, if the results of the tests render the theory's hypotheses less plausible, it is not the theory tester who is "wrong," it is the theory's creator. Moreover, in the case of negative evidence, the theory tester may receive accolades from colleagues for undermining the theory. In general, then, it is less risky to base a research career on testing others' theories than it is to create, disseminate, and test one's own theory.

Although the analysis just presented has some degree of plausibility, after all, theoreticians have egos, too, it is important to understand that even when theories are shown to be partially implausible, they may continue to exert influence in an area of inquiry. Einstein once observed,

There could be no fairer destiny for any . . . theory than that it should point the way to a more comprehensive theory in which it lives on, as a limiting case. (Popper, 1974, p. 32)

Rarely are theories completely implausible; thus, they are likely to exert impact on inquiry even though parts of them are incorrect. Moreover, the subsumption of one theory by another theory is a favorable rather than an unfavorable outcome in the process of doing science, as such subsumption may be an indicator of scientific progress.

The lesson to be learned here is a simple one in principle but a difficult one to practice. Advancing one's own theory should be approached with the view that at least some parts of the theory being proposed are likely to be wrong and that these errors will be exposed by others in the public domain of journals and books. This is not a bad thing to have happen to one's theory, in fact, it may be a positive outcome. In short, one must be willing to be and expect to be wrong when one advances a theory. In practice, of course, it is difficult to continue to adopt such a stance when one's theory is being attacked, especially if such attacks are mounted with a particularly nasty tone; nevertheless, if one can assume the attitude that theories, including one's own, are heuristic devices that are very likely to be fallible, much of the "risk" to one's ego can be taken out of the theory development process.

Another factor related to risk aversiveness that is particularly important in the case of the communication field is its size relative to the sizes of cognate disciplines. When compared with many other fields, the number of persons who identify themselves as communication researchers is very small. In some ways, the field can be likened to a small, midwestern town where, for better or worse, most people know each other. The fact that careers may be made or broken by colleagues in the field may discourage researchers from taking the risks involved in building their own theories. Researchers may be reticent to risk being wrong; especially when their next door neighbor may be the person who detects the theoretical flaw and disseminates this information to others in the neighborhood. The potential for being the focus of rumor and for being ostracized by fellow town dwellers may discourage communication researchers from advancing new theoretical ideas. Under such conditions, it is understandable why researchers might take the more conservative route of testing someone else's theory.

Self-Selection

The risk aversiveness account for the relative lack of theory development in communication research suggests an even broader explanation of this phenomenon. To wit, persons who select themselves into graduate communication programs are generally those who, for a variety of reasons, are not motivated to develop communication theory. Given the previous discussion, for example, it is possible that persons seeking advanced degrees in communication are, for some unknown reasons, risk averse, and therefore not particularly motivated to develop theory. In view of the previous observations concerning the fragmentation of the field and its skills legacy, an even more plausible set of self-selection factors presents itself. Prospective graduate students who see their graduate education primarily as a pathway to such enterprises as teaching communication skills, engaging in organizational consulting activities, and becoming market researchers are not likely to be highly attracted to theory generation activities. Such persons may be quite interested in learning about extant theories to the extent that knowledge of these theories furthers their ends; however, their orientation is not likely to be one that values theory development.

The fragmentation of the field, its skills legacy, the propensity for some to define it solely as an applied enterprise, and the existence of those who see graduate education in communication as a training ground for researchers present potential graduate students with such an extensive and potentially bewildering array of alternative professional paths that most students can see themselves fitting into the typical communication program in some way, even when the particular program they are considering may not do precisely what they would like it to do. Nevertheless, the vague, Rorshach-like sense of purpose projected by many communication departments, in terms of their goals for graduate education, invites considerable confusion about the central mission of these graduate programs. As a consequence, in the same classroom, one is likely to find graduate students who are seriously interested in becoming theory builders and researchers sitting next to students who simply want to acquire skills, techniques, and the trappings of academic prestige in the form of degree certificates that will enable them to become successful communication practitioners. Understandably, the potential for the development of an inclination to generate theory may be seriously undermined in these circumstances.

POSSIBLE PALLIATIVES

Although it is axiomatic that the past cannot be altered and that the historical roots of the communication field still exert considerable influence in shaping it today, historical forces discouraging theory generation are not necessarily immutable. Of course, it is always possible to rewrite history in such a way that present problems are explained away or "solved"; however, rather than engaging in this potentially self-serving activity, I will leave that task to interested historians. Consequently, I will use the remainder of my allocated space to consider some steps that might be taken to alter the current, undesirable state of affairs.

Graduate Educational Experiences

One obvious place to begin to ameliorate the problem outlined above is by making theory development an integral part of the graduate experience. It is not necessary for students to have had extensive training in research methods for them to perform at a high level in the theory generation domain. In fact, experiences with theory construction ideally should occur before students begin to acquire specific research tools. Moreover, theory construction should not simply be a part of a course that focuses on various substantive theories relevant to communication inquiry; rather, the course should be devoted entirely to the explanation of key concepts involved with the notion of theory and alternative approaches to the explanation of communicative action. Students should be required to explicate theoretical constructs and to begin to build theories that explain communication phenomena of interest to them. In my experience, students generally struggle with construct explication and theory generation at first, many times because they do not know what it is they wish to explain; however, by the end of several weeks, most students have a good grasp of what a theory is and how one might be built. In addition, students have acquired a set of criteria for evaluating theories.

Theory development is a creative activity, and it is difficult, if not impossible, to teach creativity. Nonetheless, even if courses in theory building do not increase students' creativity levels, they serve both to make them more critical consumers of others' theories and to appreciate the complexity of the theory development process. Since these experiences take a considerable amount of the mystery out of the theory generation process, students are less afraid to risk engaging in the activity; consequently, such educational experiences serve to deal with the risk aversion problem. In addition, experience with theory development before extensive exposure to research methods helps ensure that students will put methods in their proper place, thus reducing the likelihood that students will develop severe cases of methodological fixation.

Altering Values

Thirty years ago it was enough for communication researchers to demonstrate that they could use the data collection and data analysis techniques of cognate areas as well as, if not better than, those whose intellectual home was in these areas. While it is still necessary for communication researchers to demonstrate their competence in the use of tools of inquiry, the ante has now been upped. Increasingly it is becoming necessary for communication researchers to make substantive contributions to communication theory beyond those made by researchers from other disciplines. Since reward systems both reflect underlying value systems and promote the development of values, it is time for various scholarly reward systems to reflect this shift. There are a number of reward systems implicated here.

First, the criteria used to judge the value of papers submitted to journals for potential publication should not only include methodological considerations, as they currently do; criteria should also take into account the degree to which papers in some way advance communication theories. While theoretical substance is explicitly used as a criterion for publication by some journals in the field, some papers published in these journals do not reflect a great deal of theoretical sophistication, suggesting that the theoretical substance criterion needs to be emphasized more in the refereeing process. In addition, the creation of the journal *Communication Theory*, to provide an outlet for theoretically oriented papers, does not obviate the necessity for data-based papers submitted to other journals to have strong theoretical grounding. Second, promotion and tenure systems should not only focus on the quantity of research produced and the methodological sophistication of that research but also on the quality of theoretical thinking underlying the research. Theoretically grounded programs of research should be valued over atheoretical research that is scattered over several unrelated areas. Such research programs should also be valued over tabloid scholarship; that is, research motivated by the latest newspaper headlines (Hart, 1986). Tabloid scholarship may gain high visibility for its practioners in the form of appearances on talk shows and other media venues; however, matriculation in the Phil Donahue/Geraldo Rivera/Oprah Winfrey School of Social Research does not certify the theoretical importance of one's inquiry. It is not enough for researchers to demonstrate that they can use certain methodological tools, even when they are used to study current, highly visible social issues. It is the capacity to sustain theoretically driven, programmatic research that produces significant insights about communication phenomena in the long run. It is this ability that should receive maximum rewards. A third reward system that may need revision is the one used by national associations to recognize the scholarly contributions of their members. Again, contributions that flow from well articulated theoretical frameworks should be valued over both textbook syntheses of others' work and research on highly visible topics that lacks conceptual integrity.

Big Questions

Perusal of research reported in other domains of inquiry suggests that in many cases the efforts of researchers are organized around a relatively small set of overarching questions. For instance, the quest for cures for various diseases frequently motivates basic research in the biological sciences. Achieving a reasonably plausible and close to complete explanation of the operation of the human immune system may prove to be highly instrumental in finding more effective ways to prevent and treat diseases like cancer and AIDS. Cadres of researchers, based in various research centers, work toward the goal of achieving a better understanding of the human immune system. While their theoretical approaches and methods may differ, their research programs are generally oriented toward answering the same question. The same phenomenon can be observed in the social sciences.

Given both the small size and the general level of fragmentation of the communication field, it is not surprising that much research reported in our journals is not oriented toward answering a small, overarching set of questions. Of course, most of us are not attempting to achieve the potentially dramatic goal of finding a cure for communication; although, some of my colleagues outside of the field wish that we would, especially for communication that takes place during faculty meetings. Nevertheless, it is difficult to find more than two communication researchers from different universities who are working on the same question. Groups of researchers may be working in the same general area, for example, studying the relationships between cognitive processes and communication or examining the development of relationships; however, within these broadly defined areas there is very little overlap among specific questions being pursued. As a consequence, with few exceptions, there is relatively little in the way of theoretical dialogue in our journals. Granted, critiques and disagreements concerning methodology occasionally appear in our journals; however, there is a dearth of scholarly interaction and debate around theoretical issues. Again, this black hole in our scholarly discourse is partially the result of a lack of overarching research questions around which to interact.

How best to go about generating a small set of questions with which to orient research efforts is a difficult problem. Conferences involving researchers working within a particular interest area might produce some progress in this direction. Certainly, such a task cannot be accomplished with a single, broad-scale effort, given the current fragmentation of the field. It is possible, of course, that it takes a critical mass of researchers working in a particular field before such overarching questions emerge. If this is so, then such organizational efforts at any level may prove to be relatively fruitless, until the size of the field reaches this critical mass. This possibility notwithstanding, there is a need to develop a relatively small set of overarching research questions with which to guide inquiry so that discourse at the level of theory will be facilitated.

CONCLUSION

A decade ago, in his yuletide assessment of the state of communication research, Miller (1981) noted that considerable progress had been made by communication researchers along a number of fronts since the early 1960s. He cited the greater legitimacy accorded social scientific approaches to the study of communication and increased sophistication of both theory and research as empirical indicators of progress during this period. In the same colloquium, Phillips (1981) offered a considerably more pessimistic assessment of the state of communication research. He argued that the term "science" has been used uncritically by those who do communication research to describe what it is that they do. He noted that doing science involves much uncertainty and that there is little evidence of this uncertain attitude, as reflected by the lack of published corrections and recantations, in the scholarship of communication researchers. He also chided communication researchers for tacitly assuming that ". . . proper methodology is a source of truth, without regard to the object of study" (p. 362).

Where are we a decade later? Certainly, the historical roots that gave rise to the communication field continue to exert influence in the ways described previously, and some of the problems noted by Phillips (1981) appear to be outgrowths of this legacy. It will take a few more decades and considerable conscious effort for communication researchers to alter these forces in the direction of encouraging the development of communication theories. Such efforts may be met by resistance from those who define the field's goals as being primarily concerned with the proffering of communication skills instruction, or from those who define the field solely as an applied enterprise. Even in the absence of active resistance, however, it will take a few more generations of non-risk averse communication researchers, socialized into a theory building culture that places methodology in its proper perspective, to negate significantly the impact of these early influences. Most likely, these changes will be evolutionary rather than revolutionary, as Miller's (1981) assessment implies. Those who eagerly watch and wait for an Einstein-like figure to appear on the scene, complete with The General Theory of Human Communication in hand, are very likely to be disappointed. For it is the responsibility of those of us who are here now to meet the challenges of posing important questions about human communication, building theories that try to answer these questions, and encouraging our students to do the same.

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