GALILEAN RHETORIC AND PRACTICAL THEORY[1]

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Bold claims have been made for the practical usefulness of Galileo Theory as well as its potential to become an explanatory theory of human communication with scope and precision comparable to the fundamental theories of physics. This essay concludes that the arguments that have been offered to support those claims are weak, not only because the empirical evidence on which they are based is weak, but because the philosophy that warrants the relevance of the evidence is incoherent. The theory is not, however, without value. It makes useful technical contributions to communication science, and it suggests a "practical" notion of theory which, properly understood, would have profound implications for a discipline of communication.

THE seventeenth century physicist Galileo, a founding father of modern science, was also something of a rhetorician, having developed lines of argument basic to scientific discourse.[2] Neither Galileo Galilei nor his own "Galilean rhetoric" are, however, explicit topics of this essay. The "Galilean Rhetoric" of our title refers instead to a theory of communication proposed recently by Woelfel and Fink, that bears the name of Galileo and sounds a distant cultural echo of his rhetoric.[3] Its authors have made bold claims for the practical usefulness of Galileo Theory as well as its potential to become an explanatory theory of human communication with scope and precision comparable to the fundamental theories of physics. These claims have not been subjected to much critical scrutiny though several empirical studies of the theory have been published.[4]

My intention is to pursue a critique of Galileo Theory and in doing so to plumb a deeper set of issues: By what criteria should a theory of human communication be judged? What is or should theory be *for* in a field like communication? What does or might it mean to "test" a theory in this field?

In the sections that follow I summarize Galileo Theory and examine in detail the arguments that have been offered to support it. I judge that those arguments are weak, not only because the empirical evidence on which they rest is weak but, more importantly, because

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[1]The author thanks Walter Fisher, Herbert Simons, and Arthur Bochner for their helpful comments.

[2]Maurice A. Finocchairo, *Galileo and the Art of Reasoning: Rhetorical Foundations of Logic and Scientific Method* (Dordrecht: R. Reidel, 1980).

[3]Joseph Woelfel and Edward L. Fink, *The Measurement of Communication Processes: Galileo Theory and Method* (New York: Academic Press, 1980).

[4]George A. Barnett, "A Multidimensional Analysis of the 1976 Presidential Campaign," *Communication Quarterly*, 29 (1981), 156–65; George A. Barnett, Kim B. Serota, and James A. Taylor, "Campaign Communication and Attitude Change: A Multidimensional Analysis," *Human Communication Research*, 2 (1976), 227–44; Michael J. Cody, "The Validity of Experimentally Induced Motions of Public Figures in Multidimensional Scaling Configurations," in *Communication Yearbook 4*, ed. Dan Nimmo (New Brunswick, NJ: International Communication Association—Transaction Books, 1980), pp. 143-63; Robert T. Craig, "Limiting the Scope of the Spatial Model of Communication Effects," *Human Communication Research*, 3 (1977), 309–25; James Gillham and Joseph Woelfel, "The Galileo System of Measurement: Preliminary Evidence for Precision, Stability and Equivalence to Traditional Measures," *Human Communication Research*, 3 (1977), 222–34; Kim B. Serota, Michael J. Cody, George A. Barnett, and James A. Taylor, "Precise Procedures for Optimizing Campaign Communication," in *Communication Yearbook 1*, ed. Brent D. Ruben (New Brunswick, NJ: International Communication Association—Transaction Books, 1977), pp. 475-91; Joseph Woelfel, Michael J. Cody, James Gillham, and R. Holmes, "Basic Premises of Multidimensional Attitude Change Theory—An Experimental Analysis," *Human Communication Research*, 6 (1980), 153-67.

the philosophy that warrants the relevance of the evidence is incoherent. Galileo Theory, I conclude, has little to recommend it as an explanatory theory of human communication; yet even so it is valuable, both for certain technical contributions that it offers and for the challenging answers to basic philosophical questions that are suggested by the "practical" notion of theory that underlies it.

SUMMARY OF THE THEORY

Basic to Galileo Theory is a technique of psychological measurement called *metric multidimensional scaling* (MMDS). MMDS is a method of representing "concepts" in terms of a mathematical coordinate system. In Galileo® measurement, people are asked to estimate the amount of "difference" between each possible pair of concepts in a set. The concepts usually number about fifteen and are selected to represent a "cognitive domain" such as names of nations, brand names of competing products, or issues and candidates of a political contest. In estimating the differences between concepts, subjects are not forced to use a bounded rating scale. Instead they are given an example to use as a standard of comparison and are asked to write down the differences between all pairs of concepts as compared to the example, expressing the estimates with any numbers they wish. These numerical estimates are said to measure the psychological "separations" among the concepts on a true "ratio scale" much like ordinary physical scales of distance or weight. The perceived differences between and among a concept and all other concepts are said to compose the "meaning" of the concept for the individual: the averages of the differences given by a group of people are said to compose the "cultural meaning" of the concept for the group. In practice, only group averages are used for further analyses because they are considered to be more precise and stable than individual estimates.

The between-concept difference estimates then are submitted to a computer program that calculates the coordinates of the concepts in a multidimensional space so that the distances among concepts in the space equal the differences among them as perceived by the average person in the sample. Up to three dimensions of this "cognitive space" can be portrayed graphically. These pictorial maps of the mind, though they are often very interesting to look at, may be quite misleading as representations of the originally reported differences among the concepts. Only three spatial dimensions can be portrayed graphically, but a multidimensional space of fifteen concepts may actually involve as many as fourteen dimensions and is likely to be mathematically complex, violating the assumptions of ordinary Euclidean geometry. This point is important because a key advantage that is claimed for Galileo® measurement is that it is more precise than ordinary psychological measurement. There is, as we will see, some good evidence to support that claim. But the precision of the system is effective in practice only when the difference estimates or the spatial coordinates derived from them are employed mathematically; verbal descriptions and three-dimensional pictures of the space lose much of the available precision.[5]

When several Galileo® measurements of a cognitive domain are taken over a period of time, the computer program can calculate the changes that have occurred during the intervals between

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[5]Woelfel and Fink, p. 171.

measurements. This makes possible the use of mathematical equations of motion to express the direction and rate at which each concept has "moved" in the space. The capacity to represent cognitive or cultural change as motion is said to be a chief advantage of MMDS over the more widely used "nonmetric" multidimensional scaling procedures, which do not require ratio-scale measurement. It is hoped that "laws of motion" can some day be shown to explain the observed changes in terms of "forces" operating on the cognitive space. Although Galileo Theory does not at present make definite claims as to the exact form of the cognitive laws of motion, the logic of the theory is such that those laws would resemble physical laws, and the literature of Galileo Theory contains much speculation in that vein.

A theory of persuasion must have something to say about the structure of persuasive messages and the ways in which messages bring about changes in the beliefs and attitudes of audiences. In Galileo Theory, "the smallest unit of meaning consists in the relation between two objects or elements.... The statement of the extent of similarity or difference between two cognitive elements, therefore, is the smallest meaningful utterance that can be made in this system."[6] "George is friendly" and "France and Germany are identical" are examples of such "simple messages." Most real messages are "compound" in that they make reference to more than two concepts (e.g., "Mary says Frank is friendly"). Messages are assumed to produce forces in cognitive space. Because of the complexity of language and meaning, "it is not possible at this state to estimate the magnitudes of these forces."[7] In some simple cases it is, however, possible to estimate the ratios between the forces produced by two messages based on the relative amounts of cognitive motion they have brought about.[8] It can, moreover, be shown that all of the usual mechanical concepts such as force, mass, and acceleration apply *exactly* to cognitive space *by definition* although in the present state of the art those concepts must usually be applied only loosely or approximately in practice.

Similarly the internal forces that operate in cognitive space can be described by equations of thermodynamics according to reasoning such as the following:

> We assume, therefore, that energy in the form of information is required to differentiate the space and that structuring of the cognitive space represents fixed energy which can be released again by breaking down the structure. Leakage of energy from the system would be represented by a loss of information, which in turn is represented by a loss of distinctions among concepts—a shrinkage of the space.[9]

This sort of thinking leads to speculative accounts of learning and forgetting, induction and deduction, and psychological consistency.[10] Again, however, the ingenious speculation runs considerably ahead of what can presently be done with Galileo Theory in practice.

Perhaps the best way to illustrate the present state of the Galileo Theory of persuasion is to show how it can be applied in a typical persuasive campaign.[11] A persuasive message is designed to "move" a concept or concepts in cognitive space. A political candidate may wish to move "closer" to certain issues; a marketer may wish to move a product "away" from certain undesirable attributes in the "direction" of more desirable ones. Evidence indicates that when the concept "me" (or some variation of it) is scaled in cognitive space, the

[6]Woelfel and Fink, pp. 145–46.
[7]Woelfel and Fink, p. 147.
[8]Woelfel and Fink, p. 148.
[9]Woelfel and Fink, p. 156.
[10]Woelfel and Fink, pp. 155–61.
[11]See Woelfel and Fink, pp. 170–79, 205–10.

concepts located closer to "me" tend to be ones toward which the average person in the sample has more favorable attitudes. When activities (e.g., "smoking cigarettes," "voting for Smith," "buying brand X") are scaled in cognitive space, those that are closer to "me" tend to be more often done. In addition, the closer two concepts are to each other in cognitive space, the more likely they are to "compete" with each other. Thus if one were to scale, for example, brands of coffee, one would probably find that the brands closest together in cognitive space would be those most likely to win purchasers from each other. Considerations like these dictate the goals of a typical persuasive campaign: to "move" certain concepts in "directions" that promise more votes, more sales, or higher levels of approval for the campaign's sponsors.

The basic principle of persuasion in Galileo Theory is that one can exert force on a concept in the direction of a desired location by sending messages that associate the concept with concepts that lie in that direction. But this crude verbal description does poor justice to the techniques made available by the system. A "simple mathematical procedure for designing messages," though admittedly based upon certain as yet unverified assumptions about the dynamics of cognitive space, can specify the *exact* set of messages to maximize the desired persuasive effect. As evidence of the usefulness of this exact technique, it is shown that in a certain instance the messages designed for a political campaign through "visual and intuitive" study of the graphs of a multidimensional space were "44 degrees off from the theoretically optimal strategy."[12]

Repeated MMDS measurements taken during and after persuasive campaigns are said to make it possible to gauge the effectiveness of the campaigns while testing the theoretical assumptions upon which they are based. In the long run, this process will continue to refine the principles of Galileo Theory until the exact laws of motion applicable to cognitive space are known with great certainty. Since the techniques have already been applied in practice to real advertising campaigns as well as laboratory persuasion experiments, we can assume that the progress toward laws of motion is underway though we are now only in the early, halting stages of the quest.

Critique of Arguments for the Theory

At a general level of analysis, the case for Galileo Theory may be said to rest upon four lines of argument. One is that the theory is supported by empirical evidence. A second is that the theory is heuristic; it implies many interesting hypotheses that can be tested empirically in the future. These are standard arguments of scientific utility. The other two lines of argument are more philosophical. One is an argument from epistemology: that Galileo Theory follows from basic principles of science and is therefore scientifically superior to other communication theories. The final argument is ethical; it claims that Galileo Theory embodies a relativistic ethic that promises to promote human progress toward the good.

Empirical Support

The Galileo® system is clearly on its strongest ground in the psychometric arguments for the precision of the direct ratio judgment procedure for measuring cognitive stance. Woelfel and Fink do a useful critique of psychological measurement, pointing out that "all measure, physical as well as social and psychological, is conventional."[13] Some measures are, of course, more useful

[12] Woelfel and Fink, p. 171.

[13] Woelfel and Fink, p. 48.

than others; but all measures are arbitrary human constructs, and no measure, whether in psychology or physics, can be known to be "valid" in the usual methods-textbook sense that it "measures what it is supposed to measure." Phenomena cannot be observed at all except in terms of some arbitrary framework, or measure; one cannot establish the "validity" of a measure because one cannot compare it to the "real thing," only to other measures. Psychometricians who are well aware of this fact often write as if they were not. "Any instrument or measurement system, then, is valid not when its results corresponds [*sic*] to 'true' values, but rather when the pattern of measured values yielded by the system are nontrivially related to important theoretical or practical problems as defined by some human interest."[14] Validity is not a property of measurement per se but rests upon the success of the whole theoretical structure and research process of which measurement is a component.

Nor is "reliability" in the usual sense an adequate basis for evaluating a measure. Evidence for reliability typically consists of tests of internal consistency among components of a measure or its stability over repeated applications. But these criteria may encourage the development of crude measures such as lists of categories and seven-point rating scales, which are incapable of expressing fine psychological distinctions. As a general rule, the cruder a scale in this sense the more reliable it is. MMDS procedures permit people to express their difference estimates using any numbers they wish. Such an unconstrained scale makes possible the expression of arbitrarily fine psychological distinctions but is not highly reliable by the usual standards.

A more appropriate criterion than reliability, however, is precision, which combines consistency of results with the fineness of the distinctions that a measure sure affords. Woelfel and Fink offer empirical evidence of several kinds that the direct ratio judgment procedure gives markedly more precise results than do the bounded rating scales conventionally used in the social sciences.[15] One of the real strengths of the Galileo® system, then, is that it may encourage social scientists to be more venturesome in designing measures.

As to validity in the broader sense discussed earlier, I have already alluded to some of the empirical results that tend to validate measures of psychological distance, especially results tending to show that patterns in people's behavior correlate with patterns of psychological distance among concepts. Woelfel and Fink describe this research, much of which is very persuasive.[16] Especially interesting is their argument for a redefinition of "attitude" in terms of the distance of concepts from the concept "me"—again, a technical point that has implications for a great deal of behavioral research.[17]

On the evidence, then, one must concede that MMDS measurement offers distinct advantages, including increased precision and the capacity to represent some interesting phenomena. But the ultimate test of validity of a measure, according to Galileo Theory, is the success of the whole scientific enterprise in which it serves. From this standpoint, the chief advantage of MMDS is that it provides a framework in which laws of cognitive and cultural change can be discovered. Unless this larger project is successful, the Galileo® system becomes only a technically useful psychometric invention with nothing of much *theoretical* interest to say about human communication. And because the idea of cognitive laws of motion is rather implausible on its face, the empirical evidence that is

[14]Woelfel and Fink, p. 86.

[15]Woelfel and Fink, pp. 92–107.
[16]Woelfel and Fink, pp. 162–79.
[17]Woelfel and Fink, p. 164.

said to support it should be examined with special care

How would one go about empirically supporting the claim that cognitive motion conforms to exact laws? The most obvious way would be just to state the applicable laws and show that they correspond exactly to empirical data. But this may be at once too much and too little to demand: too much, because at an early stage of research, it might be thought sufficient to show even that crude approximations of laws account for data fairly well; too little, because it may be possible to state "laws" that are trivial within the system and therefore unfalsifiable in principle, or which fail to provide a fair test of the basic theoretical assumptions.

The mere existence of cognitive "motion," for example, is entirely trivial within the Galileo® system. Motion is by definition just any change that occurs in the spatial locations of concepts. Equally trivial are calculations of velocity, acceleration, etc., for all of these are properties built into the system by definition.

Nor should one be overly impressed by indications that changes in location of individual concepts in graphic pictures of cognitive space seem intuitively reasonable in the light of this or that factor. This sort of evidence has several serious flaws. First, it capitalizes on chance because it permits the observer to choose which of the large number of changes that have occurred in the space will be taken as evidence of lawfulness. Second, it exploits the considerable human capacity for discerning patterns, sometimes at the cost of greatly distorting reality.[18] Third, it begs at least two important questions about the MMDS representation of cognitive structure. Because, as I have mentioned, graphic and verbal descriptions of the space forego much of the precision assumedly made available by MMDS, intuitive judgments of lawfulness based on the study of pictures beg the usefulness of the measurement system: less precise techniques might do just as well. Intuitive judgments also beg the psychological reality of cognitive space itself because they involve only isolated events in the picture, not the whole spatial structure. This last point is important because the multidimensional space, unless it has properties that make it more than just a sum of the individual inter-concept distances from which it was constructed, is a redundant and therefore theoretically useless entity.

What, then, are the requirements of a fair empirical test of lawfulness of cognitive motion at an early stage of research? I propose the following:

1. The test should be mathematical because only a mathematical representation retains the full precision of the measurements.
2. Motion should be predicted *a priori* rather than merely described *a posteriori*.
3. The test should render the theory falsifiable in the sense that it will show positive results only if certain specific changes among the multitude logically possible within the system actually occur
4. The test should involve the whole structure of the multidimensional space, rather than just selected inter-concept distances. At the very least, "indirect" as well as "direct" changes should be predicted correctly.[19] Indirect changes are important because they show that the structure of cognitive space itself has psy-

[18]R. Nisbett and L. Ross, *Human Inference: Strategies and Shortcomings of Social Judgment* (Englewood Cliffs, NJ: Prentice-Hall, 1980).

[19]A direct change is one that is explicitly advocated by a message. An indirect change is one that is not explicitly mentioned in the message but is theoretically required by the model of cognitive space. Imagine, for example, several objects resting on a dining room table, a situation that confirms quite well to the model of two-dimensional Euclidean space. If we move the salt shaker away from the pepper shaker along a specified line, geometric logic as well as experience requires not only that the distance between the salt and pepper will increase (direct change) but that the distances between the salt and everything else on the table will change in a precisely determined way (indirect changes). This obvious principle is not so obvious in the case of *cognitive* space. See Craig, pp. 311-12

chological properties that influence the observed changes.

5. The test should conform to a statistical model; otherwise one has no way of knowing how seriously to regard what may appear as a "promising" trend in the data.

With particular reference to the third requirement, an unfortunate quality of Galileo Theory in its present state is that *no* experimental test logically can falsify it. Because the theory assumes that laws of motion can be found but does not make any definite claims about the form of those laws, any negative result can be discounted on the grounds that it may have applied the wrong laws or failed to meet the conditions for a test of the right laws. Logically, a study can only support the theory or fail to do so.[20]

The first published attempt to test Galileo Theory in a way consistent with the requirements listed above, a laboratory experiment that failed to find evidence of lawful cognitive motion, is not mentioned by Woelfel and Fink.[21] This study was by no means a definitive test of the theory, not only because the theory is unfalsifiable but because the study itself is open to various interpretations and methodological objections.[22]

Woelfel and Fink do cite several studies that suggest the lawfulness of phenomena in cognitive space. Some of the studies are based upon qualitative *a posteriori* descriptions of visual pictures and thus fail to meet the requirements of a fair test.[23] Other studies indicate relationships between cognitive distance and various other phenomena but do not predict the motions of concepts in the cognitive space.[24] Only the two studies by Cody appear to meet the minimal requirements of a fair test.[25]

Cody scaled a cognitive domain consisting of trait words related to source credibility (experienced, inexperienced, just, unjust, etc., and the concept "ideal credible source") along with the names of nationally prominent politicians. Messages were designed to move one of the politicians either toward or away from "ideal credible source" by associating (positive message) or contrasting (negative message) the politician with concepts that lay in the direction of "ideal credible source." The critical hypothesis was that the direction of the politician's motion in each experimental condition would correspond to the "resultant vector" of the message. In Study 1 this correlation was statistically significant for the negative but not the positive message; in Study 2 it was significant for the positive but not the negative message. Cody's other statistical results lend the theory similarly mixed support.

Woelfel and Fink introduce their summary of Cody's research with the comment that it "seems to show that the equations [of motion] are not too far off the mark."[26] This interpretation of Cody's results is misleading in the light of these facts: (1) only half of the critical

[20]At a recent convention session on MMDS applications, Robert McPhee and Richard Thomas reported their attempt to test the consistency of concept motions by observing whether the frequency of violations of triangle inequalities (a geometric relationship) increased following an experimental message. They concluded that "the findings contradict the assumption that motion in the general MMDS space is spatial." In public discussion following the paper, advocates of the theory argued that it does not assume that the structure of cognitive space is stable in the way that this test decides. The present author emerged from the discussion with the clear impression that Galileo Theory places *no* definite constraints on cognitive motions and is therefore strictly unfalsifiable. Specific hypotheses can be tested, but the theory as a whole is never at risk in such tests. Robert D. McPhee and Richard Thomas, "Is Concept Motion in Multidimensional Space Spatial? A Revision and Extension of Craig's Test," annual convention of the International Communication Association, Minneapolis, May, 1981.

[21]Craig.

[22]Craig, pp. 320–23; Cody, pp. 144–45.

[23]The "CTP" study, in Woelfel and Fink, pp. 139–44, and the study by Barnett, et al., in Woelfel and Fink, p. 171.

[24]Woelfel and Fink, pp. 162–70, 178–79.

[25]Cody.

[26]Woelfel and Fink, p. 176. The equations can be found in Woelfel and Fink, pp. 172–76.

correlations were statistically significant; (2) the pattern of significant correlations was not consistent between the two studies; and (3) the studies tested only the direction of movement, not equations expressing its amount or rate, much less "instantaneous acceleration." Cody's research was carefully done and represents considerable technical advancement over previous efforts. One may choose to consider the results as either promising or not, but they are unlikely to induce a skeptic to believe in cognitive laws of motion.[27] In regard to the explanation of cognitive and cultural change, Galileo Theory has a long way to go to meet the standards of empirical support that prevail in physics or even in social science.

Heuristic Value

A second line of argument in support of Galileo Theory is that it has heuristic value. Although Woelfel and Fink do not make this argument explicitly, it is implicit in the speculative discussions that compose much of their book.

A theory is scientifically heuristic when it opens up new empirical approaches to old problems or suggests previously unsuspected relationships among phenomena. If one is willing to suspend disbelief in its basic premises, Galileo Theory indeed suggests many interesting questions; yet it may also tend to distract attention from other questions that are at least as relevant to human communication but are difficult to represent in terms of this theory. The overall judgment of heuristic value must then be one of heavily qualified assent.

The idea that the mind can be thought of as a kind of space in which concepts are located has been influential among some cognitive theorists, especially in social psychology.[28] Galileo Theory follows from the esthetically appealing notion that phenomena in this cognitive space may conform to laws much like the laws that apply in physical space. Concepts like force, mass, and acceleration partake of a rigorous theoretical logic that clearly dictates a program of research. The theories of physics offer intriguing speculative analogies: What are the thermodynamics of cognitive space? What about gravitational fields? And so forth. Nor must these theories be applied only as loose metaphors; the precise mathematical representation of cognitive phenomena inherent in the Galileo® system makes it possible to define exact analogs of physical phenomena, to which the physical laws can then literally be applied. Human communication is not loosely *like* a thermodynamic system; hypothetically it quite literally *is* a thermodynamic system.[29] This hypothesis has not, of course, been proven empirically, but it is possible in theory and therefore is worthy of investigation—if, that is, one is willing to invest in the basic premises of the theory.

One might be inclined to bet on the theory if the physical analogies were intuitively compelling. Accordingly, Woelfel and Fink do a great deal of imaginative extrapolation. Ordinary concepts like meaning, attitude, belief, self-concept, reasoning, learning and forgetting, and culture, are all defined within the theory. Interpersonal conversation, group processes, and cultural evolution are translated into terms of thermodynamics.[30] These sections of the book, which offer the reader some theoretical insights of real value, suggest the possibility that large areas of social science can be incorporated into Galileo Theory.

However inspiring that prospect may be, one should view it in the light of

[27] Cody, pp. 160–61, suggests possible interpretations of his negative results that would "save" the theory.

[28] For a brief review, see Craig, pp. 309–10.

[29] Woelfel and Fink, pp. 183–90.

[30] Woelfel and Fink, pp. 32-36, 122-70, 180–98.

certain realities. The theoretical discussions are highly speculative. Often they suggest technologies of operationalization that do not yet exist (can cognitive motion be measured during a conversation?), or they invoke cognitive-cultural laws that have not been established empirically. One should realize too that those who invest their resources in this risky venture may have to withdraw them from other enterprises that are known to be profitable. Prudence demands a careful study of the market.

Spatial models of the mind are not, in fact, as much in demand today as they were a few years ago. Woelfel and Fink largely ignore the current literature of cognitive theory, which is dominated by an information processing model.[31] Cognitive science generally agrees with Galileo Theory that a useful model of the mind must be capable of handling vast amounts of information; but for most cognitive theorists the important questions concern the ways in which this vast store of information is represented and organized in the mind and used in performing information processing functions, such as retrieving information from memory, integrating procedural with factual knowledge, understanding discourse, and solving problems.

Galileo Theory, in its reduction of all mental relationships to the highly abstracted elixir of "cognitive distance," and of all mental processes to "motion," obscures most of the questions about structure and process that are of most interest to cognitive science. There is no place in this theory for "procedures," and therefore no way to represent structures of information by which the mind *does* things, which theorists have attempted to capture in concepts such as scripts, grammars, and production systems.[32] Communication theorists who want to develop models of *competence,* of the skillful *doing* of communication, should find such concepts very interesting and will not find Galileo Theory heuristic from their point of view.

Language is a subject of great importance to communication theory that the Galileo® system handles only awkwardly. Cognitive "laws of motion" will not be of much use to persuasion theory unless means can be found to translate between the ordinary discourse of persuasive communication and the mathematical language of the theory. Efforts to do this have not to date yielded impressive results.[33] If Galileo Theory has nothing interesting to say about language, it has little more to offer by way of insights into rhetorical forms such as figures of speech, strategies of argument, and genres, matters which are difficult even to discuss in terms of the system. The abstract, mathematical definition of "message" in Galileo Theory may not sell quickly in a market that wants information about the rhetorical and linguistic details of constructing persuasive messages.

To adopt the Galileo® system as a basis for communication research is, then, to speculate on a radically new paradigm which would shift attention to a new set of questions. Since the empirical success of the theory has been spotty, the decision to invest in it must be taken largely on faith. Speculative fever may be induced by an inspiring vision of the future that includes specific direc-

[31]For reviews, see Robert T. Craig, "Information Systems Theory and Research: An Overview of Individual Information Processing," in *Communication Yearbook 3,* ed. Dan Nimmo (New Brunswick, NJ: International Communication Association—Transaction Books, 1979), pp. 99–121; and Sally Planalp and Dean Hewes, "A Cognitive Approach to Communication Theory: *Cogito Ergo Dico?,*" in *Communication Yearbook 5,* ed. Michael Burgoon (New Brunswick, NJ: International Communication Association—Transaction Books, 1982), pp. 49–77.

[32]Woelfel and Fink, pp. 221–22, define the concept "algorithm," which, however, plays no further role in the theory.

[33]Woelfel and Fink, pp. 145–55.

tions for action—the "heuristic value" of the theory. Other minds, however, will demand more probative force to justify their faith. Thus we turn to the philosophical arguments for the theory.

Epistemological Basis

The first philosophical argument, rooted in epistemology, claims that the Galileo® system is superior to previous communication theories because it is designed in accordance with the "first principles" of science.

Woelfel and Fink begin by pointing out that all inquiry takes place within arbitrary, conventional systems of observation. Physical phenomena are not lawful in themselves but only *as we observe them* within the special frameworks of observation devised by physicists for scientific purposes. Social science until now has failed to discover any laws. Often it is claimed that this failure is due to certain inherent qualities of human behavior such as complexity, subjectivity, and free will. But no phenomenon is lawful or unlawful *in itself*, so the anthropomorphic claims about human nature reflect just another arbitrary point of view, the widespread acceptance of which may indeed have brought about the failure of social science through a kind of self-fulfilling prophecy.[34] Because most social scientists are ignorant of higher mathematics and share the general cultural prejudice against "dehumanizing" mechanistic explanations of human behavior, they have not seriously attempted to bring human behavior within the scope of exact science but have been satisfied instead with imprecise concepts and methods that have precluded any possibility of discovering exact laws of behavior. We should not, then, be surprised to find that social science has failed to discover any laws. "But if we include human phenomena within the framework of science, this means we agree to adopt a different way of observing those phenomena."[35] An exact social science can be built only within a framework of observation that is sufficiently precise and expressive to describe whatever laws can be found, the language of mathematics. Social science, conclude Woelfel and Fink, can be successful only if its theories are consistent with the basic principles of science.

Woelfel and Fink list five "first principles" of science that are reflected in the basic theories of physics.[36] I will not quarrel about the list, though I doubt whether centuries of disputatious philosophizing about the character of scientific knowledge, hardly mentioned by Woelfel and Fink, can actually be boiled down to these five principles. Let us accept for the sake of argument that they are basic principles of physical science and that the Galileo® system is consistent with them. What conclusion may then be drawn?

Woelfel and Fink state their conclusion in this fashion:

The implication of what has been said up until now is that the failure of social science to develop as satisfactory a system of knowledge as has been developed in physical science cannot be attributed

[34]Woelfel and Fink cite no specific writers who claim that human behavior is inherently unlawful, and their generalized opponent may seem like something of a strawman when compared, for example, to MacIntyre, who makes several powerful arguments for human unpredictability which Woelfel and Fink do not confront. But a subtlety of Woelfel and Fink's reasoning should not be overlooked. They do not argue that human behavior is lawful when viewed from perspectives of everyday life. They argue, indeed, just the opposite: that human behavior is *not* lawful when viewed from ordinary perspectives; that what an exact social science requires is the construction of an artificial framework of observation like those that physicists have devised to render lawful the apparent chaos of physical phenomena. Cf. Alasdair MacIntyre, *After Virtue: A Study in Moral Theory* (Notre Dame, IN: University of Notre Dame Press, 1981), pp. 84–102.

[35]Woelfel and Fink, p. 3.

[36]The principles are: Relativization, Objectivization, Empirical Verification, Maximum Information, and Minimum Information. For the definitions, see Woelfel and Fink, pp. 22–25.

to inherent differences in the subject matter of the two branches of knowledge, but rather can be attributed to an inconsistent application of the basic principles of science by social scientists. If this is so, then decisive improvement in the quality of social scientific knowledge must follow from a more explicit compliance with the principles of physical science.[37]

On a strict interpretation this argument is plainly fallacious. Its error is to suppose that because we cannot directly know the inherent character of phenomena, that phenomena themselves have no bearing on the success of a scientific theory. If science is just a set of conventions derived from "first principles," then rigorous adherence to those principles must *guarantee* the validity of a theory so constructed. One can only wonder why it would be necessary on a view such as this to collect any empirical data at all. But "empirical verification" is itself listed as one of the "first principles" of science!

A charitable act of interpretation might permit Woelfel and Fink to escape from this trap. Perhaps they inadvertently overstated their point and meant only to say that decisive improvement in knowledge *might*, rather than *must*, follow from adherence to the first principles of science. Because I accept their premise that we cannot directly know the inherent qualities of phenomena, I, at least, would not object to this weaker inference. It amounts to no more than the claim that we cannot rule out *a priori* the possibility of an exact science of human behavior. But this would be a very weak argument for the theory; for there are many propositions that we cannot rule out *a priori*, but which we nevertheless prudently ignore because we have no good reason to believe that they are true.

On another interpretation the argument might be thought to mean only that a theory, if it adheres to the first principles of science, must be "scientific" by definition.[38] But then we must wonder what is meant by a "decisive improvement" in knowledge; for a decisive improvement in communication theories would surely require that they be more than just scientific by definition, but that they be empirically successful as well. And empirical success is not *just* a matter of definition; it depends upon the cooperation of phenomena.

But to capture the practical force of Woelfel and Fink's argument requires, I think, yet another interpretation of it: that a communication theory which follows from the first principles of science is *likely* to yield decisive improvement in knowledge. This version, which strikes a semantic compromise between the troublesome modalities of "might" and "must," is an argument that cannot be strictly proven or disproven logically. It is instead an instance of deliberative rhetoric, designed to persuade us to accept Galileo Theory because of its *probable* scientific utility. Though I agree that this is a proper sort of argument to make for a theory, I am not persuaded by the argument in this case.

There seems little reason to expect that a theory of human communication derived from physics would be successful, or more generally, that the "first principles" of physical science would generate an exact social science. I do not base this claim on any assumptions about the inherent nature of human beings, but look instead to the evident diversity of science itself. Theoretical physics is surely the most admirable of sciences; by the same token it is atypical. Every branch of science from agronomy to zoology has its own methods of inquiry, its own kinds of data, and its own forms of theoretical knowledge, however modest. Mathematical theories are valued and

[37]Woelfel and Fink, pp. 21–22.

[38]Woelfel and Fink, p. 237 et passim, make some comments that suggest this interpretation.

physical theories are applied wherever possible, but branches of science do not usually set out to establish themselves explicitly on the first principles of physics.[39] Sciences are established by empirical and rational study of their own subject matters, using whatever concepts and methods *work*.[40] If the special knowledge of paleontology or molecular biology does not much resemble theoretical physics, why should we expect theories of human communication to do so? Even in physics the success of mathematical theory has been described as "unreasonable."[41]

There is no *reason* to have expected that mathematical theory would work in physics; it just happens to do so extraordinarily well. Much less should we expect that the *same* theories that work in physics would also explain cognitive and cultural processes. But Galileo Theory is based upon just that assumption.

[39]This fact should also be considered in the context of numerous philosophical refutations of the very notion that knowledge does or can rest upon any ultimate rational foundation. For a diverse sample of arguments that converge on this point, see: Michael Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy* (New York: Harper and Row, 1964); Richard Rorty, *Philosophy and the Mirror of Nature* (Princeton, NJ: Princeton University Press, 1979); and Calvin O. Schrag, *Radical Reflection and the Origin of the Human Sciences* (West Lafayette, IN: Purdue University Press, 1980).

[40]"Perhaps one of the greatest handicaps to the growth of knowledge in the scientific community has been the uncritical transfer of methods which have been successful in one epistemological field into another where they are not really appropriate." Kenneth E. Boulding, "Science: Our Common Heritage," *Science*, 207 (1980), 833.

While it may be true, as Woelfel and Fink argue, that the subject matter of a field is epistemologically constituted by the essentially arbitrary framework of observation employed, it is not true that frameworks of observation are selected capriciously or without reference to pre-existing, informal conceptions of phenomena. What may be "essentially arbitrary" from the standpoint of epistemology is not necessarily so from that of the working scientist.

[41]Eugene Wigner, "The Unreasonable Effectiveness of Mathematics in the Natural Sciences," in *The Spirit and the Uses of the Mathematical Sciences*, ed. Thomas L. Saatz and F. Joachim Weyl (New York: McGraw-Hill, 1969), pp. 123–40.

In conclusion, one cannot rule out the possibility that a science of human communication which adhered rigorously to the basic principles of physics would be successful. This unfortunately is the best that can be made of the epistemological case for Galileo Theory. It is an interesting point but not at all a sufficient basis on which to make the *practical choice* to do communication research within the Galileo® system.

Ethical Basis

Galileo Theory is derived from principles of science but its *ultimate* basis, write Woelfel and Fink, is ethical:

> Followed to their logical conclusion, we believe these principles are sufficient to lead to the development of the general thermodynamic-information theoretic model elaborated in this book. . . . According to our understanding, the application of different starting principles would yield alternative, nonthermodynamic models. Thus the argument developed in this book has never been that these principles must be applied to human phenomena, but that they should be. At its root, therefore, this book is an ethical argument, which suggests that certain benefits will accrue from the adoption of its conception of science.[42]

The view that science consists of practices that we may agree to follow as a matter of convention is emphasized throughout the book:

> The procedures for measuring separations among concepts in the representational continuum we will present here are precisely conventions in this sense rather than discoveries. As such, they describe conventional practices among the group of researchers who have adopted the procedures described here. Every argument, and indeed every bit of experimental evidence presented in this book, should therefore not be considered to be descriptions or explanations of the nature of cognitive or cultural processes, but rather descriptions of the advantages to be gained by the scientist who decides to adopt these conventions.[43]

Woelfel and Fink argue that Galileo Theory is relativistically "good" in the

[42]Woelfel and Fink, p. 236.

[43]Woelfel and Fink, p. 38

sense that it enhances the efficiency with which we can use information in choosing ends and pursuing whatever ends we choose:

> We have defined science here ... as progress toward a precisely articulated thermodynamic model of collective human experience. Other models can be constructed, but models other than the thermodynamic model all are characterized by "magic" at some juncture. By magic we mean non-thermodynamic models; that is, models in which energy inputs and energy outputs for any process do not match. . . .
>
> Development of a thermodynamic model for processes makes rationality possible, since it makes possible the calculation of benefits and rewards. . . . Science, then, enhances our collective capacity to define the good on cultural and individual levels, and to estimate the deviation of our course of activity from convergence on the good.[44]

An attempt to interpret this argument leads, I fear, to the same ambiguity that we encountered in the epistemological argument: Is the rationality of science *just* a matter of definition, or is it contingent upon the actual consequences of its application to the world? Scientific culture has indeed promoted the very definition of "rationality" according to which the application of scientific theories is good. But science as such is not a closed system. No theory is scientifically valid *just* by definition; a theory is valid only insofar as it passes empirical tests that it logically could have failed.

The ethical argument for Galileo Theory floats upon an equivocation. Galileo Theory is "thermodynamic" in that it uses equations of thermodynamics to describe cognitive and cultural motion. The theory of thermodynamics is paradigmatic of science. Science in general is "thermodynamic" in yet another sense: that its applications have yielded thermodynamic advantages as a result of engineering efficiencies in the use of energy. Thus, Galileo Theory is "thermodynamic"; therefore it is by definition "scientific"; therefore it will yield "thermodynamic" advantages in practice.

What this outrageously confused reasoning boils down to is the empirical claim that "engineering" applications of MMDS survey procedures and equations of motion derived from Galileo Theory will increase the economic efficiency of mass persuasion campaigns.[45] Because "the good" can be included as one of the concepts in cognitive space, those who apply the theory may optionally use various procedures to measure cognitive and cultural change in relation to "the good" as defined by some cultural group. Thus society may converge to "the good" through a kind of enlightened self-interest on the part of powerful information sources who apply the theory.[46] Woelfel and Fink themselves point out that these increases in efficiency can be realized only if "the equations for controlling such movements within the manifold are correct."[47] But as I have indicated in some detail, the empirical evidence for this assumption is not impressive.

Summary

The claim that Galileo Theory is the way to an exact science of human communication should be viewed with great skepticism. The empirical evidence for the theory, especially in respect to "laws of motion," is weak. The theory has heuristic value as a source of hypotheses for research, but the hypotheses tend to be very speculative and rather at odds with research interests central to the field. The philosophical arguments rooted in epistemology and ethics are unclear or fallacious; both rest upon an unacceptable equivocation between science as a system derived from "first principles" and science as an open pro-

[44]Woelfel and Fink, p. 237.

[45]Woelfel and Fink, p. 204.
[46]Woelfel and Fink, pp. 213–17.
[47]Woelfel and Fink, p. 204.

cess of empirical inquiry. One must conclude, in the end, that the empirical basis of this research program is just too weak to support the elaborate speculative edifice that Woelfel and Fink have attempted to erect upon it.

Nevertheless, we can learn a great deal from Galileo Theory; I do not wish to imply that this prodigious intellectual effort has been entirely in vain. Metric multidimensional scaling and the ratio judgment procedure remain as useful empirical techniques. The thermodynamic model, though it is speculative and unpersuasive, is a creative effort that expands the conceptual resources of communication theory. The epistemological argument, if it is too narrow to support the elaborate framework of Galileo Theory, is sharp enough to undercut at least one opposing position: The claim that "human nature" is *inherently* immune to scientific explanation simply will not hold against the conventionalist account of science. Those of us who would rule out "Galilean" theories of communication must find other grounds on which to do so.

Ironically, perhaps, we may be led to those very grounds through another useful contribution of Galileo Theory: the attempt, albeit unsuccessful, that Woelfel and Fink have made to root their system in ethics. This argument, although even Woelfel and Fink seem not to understand it properly, suggests possibilities that deserve the most careful consideration of communication theorists—issues which, in the few remaining pages of this article, I shall explore in a preliminary way.

Implications of a "Practical" Conception of Science

It is of course commonplace for researchers to avow that they choose theories because they are "useful," not because they are "true"; but Woelfel and Fink are unusual for the extent to which they have attempted to follow the implications of this cliché beyond the narrow utilities of the research process to the larger field of human purposes. The muddle in which we ultimately find them they have reached by thinking clearly up to that point. Other communication theorists, if they are to avoid similar muddles, must think at least as clearly.

To say that theories are "useful" is to say that they are "precisely conventions . . . rather than discoveries," that they describe "conventional practices among the group of researchers who have adopted them," and that their basis must therefore become "an ethical argument, which suggests that certain benefits will accrue from the adoption" of the practices.[48] This argument for a theory has an ethical component because it rests upon assumptions about the values by which the usefulness of a theory is to be judged. The methodological criteria of scientific theory—traditionally formulated as "prediction, explanation, and control"—represent an *ethical* commitment of the scientific community to a certain kind of epistemological rigor. The social sciences are "soft" insofar as their practices have satisfied the epistemological criteria of science only minimally in even the best of cases. Social scientists, however, can legitimately claim to be scientists to the extent that they agree to judge their practices by those values.

This "ethical" basis of science is admittedly rather narrow in scope, concerning essentially what I referred to earlier as the utilities of the research process; for that reason, perhaps, most researchers would be reluctant to claim an "ethical" basis for their arguments. A

[48] Woelfel and Fink, pp. 38, 236, 23?

more obviously ethical issue concerns the value of science itself, the proper role of science as an institution of human culture. This issue can be approached in two ways, which correspond roughly to MacIntyre's distinction between the goods "internal" and those "external" to a practice.[49] Jacob Bronowski has written eloquently of universal human values that are intrinsic to the scientific enterprise, such as the impulse to explore, the liberation from tradition and authority, the habit of testing truth in experience.[50] These are the "internal" goods of science, the universally applicable excellences which it cultivates by its practice.

The "external" goods of science arise from its application to human problems. The more advanced natural sciences have been applied technologically in rather spectacular ways; the social sciences, in more modest ways. But the *principle* of application is the same in both cases: to *whatever* extent scientific knowledge increases our capacity to control events, it has the potential to increase the efficiency with which humans pursue their chosen "goods," whatever they may be. This is the external or utilitarian justification for science as an institution. Woelfel and Fink's "ethical" argument for Galileo Theory, as we can now see, amounts to nothing more than the commonplace utilitarian argument for science in general. Galileo Theory fails, of course, to satisfy the terms of its own argument for itself; but that is not our present concern. I would like instead to inquire into the potential of social science to realize this utilitarian vision—specifically, to consider what it means to "apply" a theory.

For Woelfel and Fink, to apply a theory apparently means to export into everyday contexts the practices that have been adopted by a scientific research community; the ethical justification for doing so being solely that those practices satisfy scientific standards. This is, on the surface, a reasonable view, or at least a conventionally acceptable one; even so, I suspect it of being muddled. The muddle, I should first say, does *not* have to do with the intrusion of extrinsic values into the practice of science. The scientist must, of course, be sensitive to ethical issues. One has a *personal* responsibility to advocate the use of knowledge for good purposes; and the conduct of science itself must adhere to "extrinsic" ethical standards insofar at it impinges directly upon human beings. But the epistemological commitments of science forbid the scientist—or, for that matter, any rational person—to judge the validity of knowledge claims according to extrinsic standards, for to do so would open science to the worst sorts of ideological corruption. Science best serves humanity by preserving its own integrity. This traditional case for scientific autonomy I find quite convincing, despite the arguments for the "value-ladenness" of science that can be made against it. The muddle does not, I believe, have to do with the practices of science but with the relation between those and other practices.

What I suspect is that the practices of science cannot simply be exported to other institutions although scientific knowledge can certainly be incorporated into other practices in a contributory role. When nuclear physics, for example, is applied to the commercial generation of electric energy, physical knowledge goes through several transformations as it passes from "pure" physics to the quite distinct discipline of nuclear engineering, and to the even more alien fields of administration and politics, which are the realms in which practical decisions of whether and how to apply physical

[49] MacIntyre, pp. 175ff.

[50] Jacob Bronowski, *Science and Human Values* (New York: Harper & Row, 1972).

knowledge to this end are made. It is not only naive but dangerous to the ethical status of science to suppose that the "practice" of physics is to any large degree preserved in these transformations. Engineering, administration, and politics have their own practices—and most clearly, their own *values*—which are not closely related to those of pure physics. Physicists do different things with physical knowledge than engineers, administrators, or politicians do; they have a different attitude toward it; they ask different questions about it. An important point to note is that genuinely scientific research is carried on in all of these fields, and that physical knowledge might well be the subject of that research: Engineers might investigate the feasibility and relative efficiency to some range of technologies employing a physical principle; administrators might ask about organizational or regulatory problems concerning those same technologies; and politicians might ask about public opinion in regard to relevant issues. Physical knowledge abstracted from the practice of physics may thus become a subsidiary element of technology within the practices of engineering and administration, and in politics, sometimes, a subsidiary element of myth as well.

Now it might be objected that, contrary to Galileo Theory, physics is quite different from communication theory. This premise I gladly accept but maintain nonetheless that any application of "pure" research in communication theory to practical situations faces the same basic problem. It is true that what communication researchers study, in contrast to what physicists study, *is* a class of human practices, namely the practice of communication. Yet the scientific study of those practices is usually conducted in terms and for purposes quite different from those of the practices themselves. Woelfel and Fink would have us adopt this pervasive condition of irrelevancy as a fundamental methodological principle: Science, they suggest, must be conducted within *arbitrary* frameworks of observation expressly invented to serve the purposes of science. Whether this is precisely what researchers are doing when they routinely operationalize variables, quantify, and test hypotheses is a question that I will not attempt to tackle at present. What I do, however, wish to suggest is that the practices of communication research cannot be simply exported into everyday communication situations without undergoing significant transformations. To transfer knowledge from the context of theoretical research to a context of application is to make an important *institutional* shift of focus: that which was previously a knowledge claim subject to testing by the standards of science becomes instead a *technique* subsidiary to other ends. The practices in which it is involved, the attitudes toward it, the questions—including empirical questions that could be approached scientifically—are all different in the new context. The questions I have in mind are not, by the way, just questions for "applied" research of a very narrow sort; they include, for example, questions such as the engineer or administrator might ask about a technology, questions about feasibility, efficiency, safety, ecological side effects—genuinely "theoretical" questions in their own right, albeit at a much lower level of theoretical generalization than physics.

In the light of this reasoning, Woelfel and Fink's apparent assumption that a system like Galileo Theory can be instituted as an arbitrary framework for conducting pure scientific research and then simply exported for application seems unsupportable. Practitioners in the everyday human world are not afforded the luxury of setting up arbitrary frameworks in which to operate. They must confront the world as they find it, in all of its complexity, its irreducible historical particularity. They are, at the best, artists, however much they may import scientific knowledge or even employ scientific methods in their practice.

Numerous bothersome questions, quite apart from those related to the scientific adequacy of the Galileo Theory, spring to mind. *Who* can use these techniques? For what *purposes* can they be used? What are the *full consequences* of using them, including especially "ecological" side effects? What, for example, would be the effect upon public discourse were Galileo Theory to become a standard, widely used technique of mass persuasion? What reactive effects would it have upon the culture? Would it generate a public discourse that was even odder and more paradoxical than our contemporary rhetoric about "images" and the "momentum" of campaigns; a rhetoric in which messages would be thought of as "forces" having no ethical or logical status as "reasons"; in which *might,* in the form of sheer numbers of messages created by the correct technical procedures, would make *right;* in which ordinary forms of discourse would be thought of as mere epiphenomena of the operation of underlying laws, alienating speakers entirely from the contents of their messages and audiences from their role as judge; in which discourse would be conceived as a linguistically and rhetorically primitive rendering of geometric relationships, displaying no artfulness nor having any reason to do so? I do not seriously ask these questions about Galileo Theory because I do not seriously suppose that it might play the sort of role in our public culture that they envisage. My point is that they are legitimate questions to ask and that they arise from institutional concerns quite different from those of a pure science concerned only with the adequacy of Galileo Theory as an explanatory hypothesis. They arise from the view of public discourse as an art of rhetoric, a practice with its own history, its own internal goods, and its own cultural role—to all of which Galileo Theory, even though it might be incorporated into the art as a technical resource, is utterly alien. And they are representative of questions that could be asked of *any* scientific hypothesis about communication as it is abstracted from research and exported as a technique to a different institutional context—when, for example, it is incorporated into "practical" textbooks to be used by teachers, trainers, and consultants.

Communication scientists, distracted by the logic of explanatory hypothesis testing, do not typically ask these kinds of questions about their theories, but *someone* ought to be doing so. And they should be asked as well about the numerous "techniques" that find their way into practical textbooks, lacking *any* basis of research, scientific or otherwise.

Imagine the institutional structure of a discipline that would systematically ask questions of this kind. They are empirical questions—or at least they have empirical implications; such a discipline, then, would have to be one that conducted scientific research to the end of answering them rigorously. But the questions, even though they would be investigated empirically, would not arise within the practices of pure science. They would emerge instead from concerns intrinsic to the art of rhetoric and other practical arts of communication, arts with their own histories, their own philosophical commitments which are distinct from, though not incompatible with, those of science. Such a discipline would not be a pure science—certainly not a "Galilean" science—but neither would it be just an "applied" field serving the utilitarian interests of specific individuals and organizations. It would be a discipline with rigorous methods, a body of theoretical knowledge, an institutional commitment to its own intrinsic values. It would be a *practical* discipline, whose purpose would resemble in broad outline the task of rhetoric as described by Bitzer: "to conceive the better alternatives [to contemporary rhetorical practices] and do the theoretical and empirical work that could make the alternatives operative."[51]

[51]Lloyd F. Bitzer, "More Reflections on the Wingspread Conference," in *The Prospect of Rhetoric*, ed. Lloyd F. Bitzer and Edwin Black (Englewood Cliffs, N.J: Prentice-Hall, 1971), p. 207