Introduction

It is time that students of rhetoric bend their skills to an analysis of scientific discourse and the creation of scientific knowledge. In the light of recent historical, philosophical, and sociological approaches to the production of scientific knowledge, and even in some essays directed to rhetoricians, one (at least a sociologist such as I) can find solid reasons for extending rhetorical analysis and criticism to the process of knowledge production in science.

After years of dalliance with the reconstructions of a variety of positivisms, hints of the "social" and "historical" are appearing in analyses of scientific practice. Of course, the whole tradition of the sociology of science assumes a communitarian practice as the context for the production of scientific knowledge. Yet of more direct pertinence here is the growing importance of the work of Thomas Kuhn, the debate between Kuhn and the critical rationalists, and the undervalued work of Michael Polanyi and John Ziman, which points to the centrality of "community" in understanding the creation of scientific knowledge.

Ironically, the latter days of "positivism's" struggle to eradicate metaphysics and establish an objective standard of truth once and for all led it perilously close to using a notion of community as a criterion. The commitment of that tradition to the practice of natural science as a measure of success and the logical failure of successive reformulations of its criterion of meaning have led to a collapse into what looks suspiciously like a consensual theory of truth. In both Feigl6 and Nagel's7 formulation of "testability" as a criterion of meaningfulness, it is a collective agreement of scientists which establishes that a statement is testable or has been tested satisfactorily.

From a rhetorical point of view one might handle such a communitarian notion of science by the substitution of a conception of
"audience" for that of scientific community. As a result one could then understand the process of constructing scientific knowledge as a way of speaking about specific experiences before a limited and specially trained audience that is authorized to establish that discourse as knowledge.

Thus, I shall suggest that science as a knowledge-producing activity may be treated rhetorically because the construction of scientific knowledge involves argumentation before an audience. This presentation is sympathetic to Weimer's contention that science is a "rhetorical transaction" because its characteristic inference (the adjunctive conditional) is argumentative in form, and to Campbell's position that since scientific discourse is rhetorical "personae are indeed implied by and discoverable in every scientific discourse." However, I am persuaded that basing the rhetorical analysis of science on the audience-conditioned character of scientific discourse is likely to prove less controversial than is Weimer's argument. Even if further investigation should raise doubts about his reconstruction of science (as conceivably it might), it is unlikely that we shall soon retreat to the notion of self-uttering, self-evident discourse-as-knowledge which plagued the positivisms.

In this brief essay I shall offer, first, a display of the importance of a conception of scientific community in the work of Polanyi, Ziman, Kuhn, and the critical rationalists, and the consequent involvement of discourse and audience that it suggests. Second, considering rhetorical analysis and criticism to be concerned with all forms of audience-conditioned speech, I shall offer a simple rhetorical perspective on the construction of scientific knowledge. Finally, I shall outline a scheme for the analysis of scientific discourse and exemplify its application to one of the social sciences, namely, sociology.

Scientific Community

1. Polanyi and Ziman. The work of Michael Polanyi and John Ziman forms something of a complimentary pair. Polanyi mainly deals with the conduct of individual scientists; Ziman concerns himself more with the social context for that conduct.

Polanyi understands science as a form of activity undertaken by
individuals who are using skills acquired through a process of apprenticeship. The personal information generated by these inquiries is then referred to the community of scientists for judgment on its scientific status. Here I shall focus on his two major contributions, the notion of scientific conduct as a skillful activity that is acquired through practical tutelage under some "master"; and second, his grasp of the scientific community as a traditional collectivity organized to control the practice and products of scientific activity.

Polanyi's view of science takes it as a practice which involves skillful performances which are made possible by the kind of rules ("tacit knowledge") that we use when producing grammatical speech. Native speakers of a language acquire their proficiency in the production of speech through talking with audiences of skillful grammarians (other socialized native speakers) on the model of discourse they have heard, although they would be unable to reconstruct in detail the rules which allow for the grammaticity of their speaking. Continuing this analogy, the process of acquiring a grammar of scientific practice requires an engagement in research on the model of some skillful practitioner in whose person there is incarnated both the general culture of science and particular traditions within that culture. One can no more discover the culture of scientific research from its written results than one can construct a Stradivarius from measurements of an original.¹⁰

As an apprentice, the naive scientist becomes acquainted not only with the practice of inquiry, but also with its rationale — a passion for making discoveries that will be true for the individual and valued by other scientists. The neophyte learns the exhilaration of a personal quest for knowledge that has to be tempered by attachment to a validating collectivity. No matter how persuaded the individual might be of the beauty and truth of their vision of some aspect of the world, no matter that the individual's belief in the truth of their discovery is conceived as a belief that all scientists would hold given the same information, the scepticism of the scientific community about the discovery must lead to a personal scepticism. "Our vision must conquer or die."¹¹

Polanyi takes the collectivity of scientists as an institution of social control which legislates what is called "science" and who
shall be allowed to practice it. The consensus among scientists as to what is of value to them, what is reliable, of systemic and intrinsic interest, is made concrete in the many specialities that compose scientific practice by individuals who watch over their own and adjacent areas. Since no one can watch all areas, judgment as to what is admissible as scientific knowledge depends on networks of responsible and authoritative critics held together by trust in each other’s judgment.

Yet the traditional character of the scientific community does not make it immune to change and disagreements. Polanyi believes that controversies crucial to science may flare up around rival systems of explanation. Of the disputants in such situations, he says: “They think differently, speak a different language, live in a different world, and at least one of the two schools is excluded to this extent for the time being (whether rightly or wrongly) from the community of science.”¹² These controversies are resolved only by the proponents of some new understanding winning the intellectual sympathy of their opponents through plausible argument and thereby persuading them of the scientific value of that position.

But if Polanyi is concerned with the grammar of research in a tradition-based community, Ziman may be taken to provide a complimentary emphasis on the rhetoric of research and communication in search of consensus. Ziman begins his account of science with an effort to distinguish it from other kinds of inquiry. He does this partly along the lines employed by Polanyi. He focuses on scientific investigation as a “practical art. It is not learnt out of books, but by imitation and experience.”¹³ Thus science for him, as for Polanyi, is quintessentially the research practice of scientists. However, he insists that the thrust of that research is provided, not in a search for truth, but in a quest for agreement. “Science is unique in striving for, and insisting, on a consensus.”¹⁴ Scientific inquiry is characterized by a restriction to issues about which it is possible to secure universal agreement (within a community of scientists) through the application of a method that is likely to evoke unanimous acquiesence.

From his experience as a theoretical physicist Polanyi suggests three areas that are crucial to the production and preservation of this scientific consensus. The first of these is the highly persuasive
character of the scientific method; the second is a scientific education that inculcates understanding of this method and its purpose; and finally there are the patterns of communication among scientists.

Ziman agrees with Polanyi that the scientific method does not provide self-evident, ergo self-justifying knowledge, and then asks himself "How does this method lead to consensus?" His tentative answer leads him to formulate the scientific method as a potent and highly persuasive rhetoric. For example, when we examine the experiment as a persuasive argument, it may be seen as a "reproducible observation," which promises, in effect, "If you were there you would have seen the same result."

In presenting the results of scientific research using an experimental or some other technique, scientists try to act as critics of their own work. It is precisely here, in the reconstruction of experimental (and one may assume other kinds of research) into the form that it takes in journals, that Ziman focuses our attention. It is in stating a particular experiment in a form that would characterize the conduct of that research in general that there is provided the potency of experimentation as a rhetorical technique capable of generating universal consensus.15

Yet Ziman recognizes that this consensus is an agreement among scientists and realizes the importance of dealing with the processes through which individuals acquire the right both to support and to challenge that agreement. "To change the consensus you must . . . demonstrate that you understand and accept it as it is."16 Thus, the education of the young scientist seeks to inculcate the norms, traditions, and beliefs of those masters with whom the individual is apprenticed. In that relationship the neophyte learns how to think with the traditions that the master incarnates and ostentates. However, this docile acceptance is counterbalanced by the skeptical research attitude: "For when research itself begins, all this is changed. Everything, suddenly, must be doubted and rethought. Nothing, nor anybody, is to be trusted. There is no authority but one's own intellect."17 The young scientist must learn both when to accept the authority of the consensus and when to resist it.

The practice of scientific inquiry depends, therefore, on the acquiescence of individual scientists in the consensual agreement on
norms, standards, rhetoric, problems, solutions, judgments, and
the like, which provide for the continuing recreation of a scientific
consensus out of the scepticism of research. Since there is only
some limited control on the practice of experienced scientists, the
years of training have to develop a form of self-criticism which cor-
responds to collective attitudes of the scientific community.

This critical attitude is especially important, Ziman believes,
when it comes to reporting research results and dealing with subse-
quently citations and criticism. He argues that the individual's
capacity to prepare work for publication, or to evaluate other
material that has been published, all depend on this internalized
"policeman." This is especially important to him because he
suggests that while publication makes research "public," it does
not make it knowledge.

Ziman takes the position that a journal article, the fundamental
form of public communication among scientists, is no more than
data or information (despite the refereeing procedure) that might
become knowledge over time if it is neither ignored nor con-
tradicted. This continuing assay of the status of journal articles
depends on a community of individual scientists who are socialized
to the same standards, methods, authorities, and the like, so that,
despite the occasional error of judgment by an individual, it is
possible to construct a universal agreement over a period of time
on what is to count as knowledge.

Nonetheless, individual scientists are not completely on their
own after the formative years of apprenticeship. During that time
in the master's laboratory the neophyte gains access into an "in-
visible college," which Ziman understands as an informal set of
relations among persons studying the same problems. Those
relations are enacted in exchanges of letters, phone calls, pre-
prints, at meetings, colloquia, and symposia where these inter-
national groups communicate information about "their" problem.
It is membership in such a group which vitalizes the individual's
grasp of the universal consensus. Indeed, invisible colleges are the
communities within which scientific consensus is constructed. It is
precisely these groups of persons which represent the scientific
community to the individual. It is their attitude that is taken
toward work, it is their judgment that is sought, it is their
recognition which is exchanged for public contributions.
In this brief presentation of the complimentary work of Michael Polanyi and John Ziman I have broached most of the ideas (if not all the details) fundamental to a consideration of the part played by the scientific community in the production of scientific knowledge. Because of the relatively undervalued status of these two men I have chosen to approach the basics of this topic through their contribution, rather than more conventionally, through the work of Thomas Kuhn and the Popperians. We cannot, however, entirely neglect these latter.

2. Kuhn and the Critical Rationalists. Remember we are exploring here the importance of the concept of the “scientific community.” Thus, in looking at Thomas Kuhn, it is not to deal with his work in any density, rather it is to discuss his major contributions — the notions of “normal” and “revolutionary” science. The whole issue of the relation between these two forms of scientific practice is at the heart of the debate between the Kuhnian and critical rationalist positions.

Kuhn argues that the relevant scientific community for the production of knowledge is not the collectivity of all natural and social scientists. Rather, he believes that it is the small collectivity of about a hundred persons which he defines as: “the practitioners of a scientific specialty . . . they have undergone similar education and professional initiations . . . [and] have absorbed the same technical literature and drawn many of the same lessons from it.” It is this small community (similar to Ziman’s invisible college) that he takes to be the “producers and validators of scientific knowledge,” where normal science is the order of the day and revolutionary science an occasional event.

But what, after all, is normal science? Primarily it is research that takes place within the intellectual limits imposed by a “constellation of group commitments” (formerly termed a “paradigm” but renamed a “disciplinary matrix”). This matrix is a set of elements shared by the members of some invisible college and has as its most important element a set of shared exemplars (a notion parallel to Polanyi’s concept of tacit knowledge). In their training, all scientists encounter problem solutions that are the basis for scientific communities. These are the concrete results of the application of a set of general principles which allow members of the invisible college to “recognize” similarity among problems.
(and hence the most likely solutions) without there being an explicit rule for that recognition. Nonetheless, exemplars are only one element in the intellectual armatorium of a disciplinary matrix; individual scientists committed to particular matrices will also share common definitions of crucial terms, metaphysical assumptions that are required to ground the employment of analogy and metaphor in research, and a set of values.

Normal science, then, is research conducted by scientists committed to particular disciplinary matrices, and is understood as “puzzle solving”: research that more or less guarantees that “the truly clever practitioner will succeed.” The focus of normal science investigation is upon such things as precise measurement, establishment of constants, the accuracy of predictions, and the like. Perhaps the most unusual characteristic of normal science is that there is an absence of novelty in fact or theory; there is no discovery. The process of research is a kind of “mopping-up” in which persons try “to force nature into the preformed and relatively inflexible box that the paradigm [disciplinary matrix] supplies.”

However, there are always anomalies which cannot be accounted for within the disciplinary matrix. While members of invisible colleges are usually able to agree that these are merely anomalies, under particular circumstances they become the “crisis” which moves members of a matrix from normal to revolutionary scientific practice. Although there are a number of possible resolutions to such a crisis (it may be solved within the matrix, it may prove entirely obdurate to all efforts), the solution by the construction of a scientific achievement that forms the basis for a new disciplinary matrix is of most interest to Kuhn. This discovery grounds the revolution that constitutes the process of change in science.

Yet, like other revolutions, it does not bring universal joy. Two groups appear, one accepting the achievement as a resolution of crisis, the other resisting it. These groups then compete for acceptance by uncommitted members of the invisible college. While Kuhn uses a number of images to convey the difference between these two groups, it is reasonable to conclude that he thinks persons committed to one or the other to be involved in worlds so different as to make communication about the achievement dif-
ficult. In the competition and argument between the two positions there is displayed the character of normal science under the different rubrics these positions espouse. It is through this display that the uncommitted members of the invisible college are persuaded to accept the new achievement.

It is precisely here that we can introduce the Popperian notion of a "critical tradition." For Popper, science is one among many forms of human myth-making that is distinguished from all other traditions of myth production by a second order tradition, that of critically discussing the myths. The growth of scientific knowledge is not in the accumulation of observations, but in "the repeated overthrow of scientific theories and their replacement by better or more satisfactory ones."

For Popper, then, the institutional arrangements of science are (or should be) oriented to a program of methodological falsification. Scientists should seek to test, falsify, refute their theories in all their work; they do not begin anew each time they conduct research. Scientists start from problems that confront them in the practice of their peers: "This means that you pick up, and try to continue, a line of inquiry which has the whole background of the earlier development of science behind it; you fall in with the tradition of science." The practice of the scientific community displays what is important in its tradition and guides individuals to those matters that are of current interest.

The differences between Thomas Kuhn and the critical rationalists, insofar as they concern our purposes in this essay, surround this issue of criticism and the nature of normal science. Clearly, if science is a process of falsification then there is no normal science. Kuhn agrees that "the central episodes in scientific advances are revolutions"; yet he insists that (even as a logical counterpoint) normal science is required. If science is to advance by bursting through frameworks, then we need the framework (the disciplinary matrix of normal science) through which to burst.

However, there is dispute between the Kuhnian and critical rationalist positions even on the nature of the critical practice which characterizes the revolutionary science that both positions accept as the basis for scientific discovery. For critical rationalists, the very essence of scientific inquiry is that all positions held by
scientists are opened to critical investigation. Popper, himself, persists in stating: "[The] criteria of refutation have to be laid down beforehand: it must be agreed which observable situation, if actually observed, means that the theory is refuted." For Kuhn, on the other hand, this choice between one position and another is a choice between disjunct frameworks where it would be impossible to secure agreement on the criteria of refutation before any test. While he is prepared to assert that in debates between scientists on the choice of a theory proper to their subject matter "neither party has access to an argument which resembles a proof in logic or formal mathematics," he is equally anxious to claim that the choice is based on good reasons. Scientists choose one framework over another because of its accuracy, scope, simplicity, fruitfulness, or because the display of what normal science would look like under one or other of the frameworks is convincing.

We need not pursue this matter farther. The genuine differences between these two positions must not disguise the emphasis each places on the concept of the scientific community. Indeed, for both the Kuhnian and critical rationalist positions, choice between scientific frameworks (or theories) is based on communitarian criteria, not timeless principles of rationality. Over and over in these positions, as in the work of Polanyi and Ziman, one is led back to the importance of the scientific community. It plays a crucial part in the training of scientists, in the conduct of research, in the reconstruction of those inquiries into publications, and in the process of judgment which confirms or denies a knowledge status to those publications.

It is now time to explore more directly the frankly rhetorical character that the substitution of "audience" for "community" provides to an understanding of the creation of scientific knowledge. In so doing, we shall consider the individual scientist as a "speaker," research as a "rhetorical situation," the scientific community as an "audience," and the discourse which scientists address to their communities as an "argument."

Science as Rhetoric

Earlier it was suggested that the failure of the positivisms to
provide a convincing analysis of the production of scientific knowledge, and the emergence of alternative accounts of that process, was signalled, in part, by the collapse of late, decadent logical positivism (neo-positivism?) into a consensual theory of truth. Yet with the appearance of these alternatives that took seriously the importance of social and historical factors in the construction of scientific knowledge there also comes a recognition that this knowledge is not *ipso facto* self-evident, but that it depends upon an audience for whatever epistemic status it achieves.

Since Descartes the primacy of logical deduction as a form of proof has gone virtually unchallenged. Logical positivism itself was a philosophical movement that strove to assert an apodictic standard for meaningful statements very much in a Cartesian mood. In general, this has meant that the very rationality of non-logical (non-deductive) arguments has been highly suspect. Once, however, a communitarian theory of scientific knowledge is seriously entertained (and scientific knowledge is knowledge *par excellence* from the Cartesian position), then the continuing neglect of argument whose status as knowledge depends on an interaction with an audience becomes more serious. The application of rhetorical analysis to the process of constructing scientific knowledge is clearly important.

Nevertheless, rhetorical scholarship has been little turned to argumentation in the various sciences. Of course, Kenneth Burke has always maintained that scientific explanations (rationalizations), although of contemporary pre-eminence, are only one among a number of competing rhetorics of motive. Campbell's essay on *personae* in scientific (really social scientific) discourse draws much of its interest from an impoverished use of a Burkean approach. The most adventurous recent work is Weimer's thoughtful attempt to formulate science as a rhetorical transaction whose rationality is obtained in an openness to criticism. That psychologist's comment that "as yet there is little on the audience in science... the scientific community as a rhetorical audience... has received little attention from any discipline or perspective" finds some answer in this sociologist's essay. These independent contributions from persons marginal to
rhetoric are an interesting expansion into the humanities of the well-known "creativity" that marginals have displayed in scientific discovery and invention.

These "marginal" ventures stand in contrast to the major contemporary effort by a rhetorician to examine scientific argumentation. Chaim Perelman has deliberately restricted his approach to "the methods of proof used in the human sciences, law, and philosophy, [in opposition to] philosophers who endeavor to reduce reasoning in social, political, and philosophical matters by taking their cue from the models provided by the deductive or experimental sciences. . . ." Therefore, in suggesting the instrumentality of a rhetorical approach to understanding the production of scientific knowledge of all kinds, it must be recognized that such analysis has, at yet, limited support in contemporary rhetorical scholarship.

In a brief sketch of a rhetorical perspective on the construction of a scientific knowledge one can do little more than suggest the kind of issues a rhetorician might want to examine. One way to do this is to fill in the concrete location in that process of four familiar, rhetorical concepts: "speaker," "situation," "audience," and "argument." I shall represent these concepts as four stages in the production of scientific knowledge. This pattern is developed from (and is meant to resonate with) the earlier discussion of the scientific community:

1. The education of young scientists transforms them into licensed speakers about matters that concern their communities.
2. Scientists engage themselves in research situations that are necessary conditions for their speech.  
3. As a result of their engagement in research, scientists construct and publish arguments which offer plausible reasons to their audiences for judging the conclusions of these persuasions to be valid.
4. Over periods of time scientific audiences provide authoritative judgment on the status of these arguments as scientific knowledge.

Yet this four-stage pattern is incomplete. Even a modest familiarity with Burke's pentad would suggest the absence of any specification of the purpose of offering persuasive arguments to a
validating audience. Ziman's position that science seeks for consensus through the persuasive presentation of research findings corresponds neatly to this incomplete pattern; and both are little more than disguised sophistry — the purpose of scientific argumentation is to be persuasive. Such a viewpoint cannot suffice.

Surely a major contribution that a rhetorical analysis can bring to the production of scientific knowledge is its critical capacity to raise questions about the purpose of such persuasive discourse. A rhetorical analysis, therefore, can consider to what end persuasive scientific discourse is and (critically) should be directed without surrendering to the obvious, that its purpose is the production of scientific knowledge. Rhetoricians are in a position to ask the pen-tadic questions about scientific knowledge: knowledge as what, for whom, by whom, through what means, for what? But such questioning is necessarily a long-term proposition that will depend on the acceptance of a rhetorical approach to science. Let me, here, turn to the more modest task of amplifying the four-stage pattern in the context of the possibility of these critical questions.

The first stage of this process of the construction of sociological knowledge deals with the production of speakers. To speak as a scientist is a privilege, not a right. Many people use scientific terms, but they are not speaking scientifically; to do that requires that one be recognized by other scientists as one of themselves. To be so recognized involves individuals in a lengthy training program to which few are called and from which fewer still are chosen.

In such a training program individuals learn what is the basis for speaking scientifically. They learn what kind of experiences are valued by other scientists, and what experiences, therefore, they should seek out for themselves. They learn where to look for things to talk about, how they can look, and for what in particular they should search. They learn how to talk about these experiences in plausible terms and what part of their searches are to be discussed and what ignored. Finally, they acquire an understanding of the nature of the audience that will "enjoy" their reports. Not only do they learn what are its relevant characteristics, they also meet many of its members, and, of paramount importance, they learn that they are themselves part of that audience. Thus, the first stage of this process is one in which scientists become equipped to construct
scientifically interesting discourse and become legitimized as members of an audience that evaluates similar discourse from their peers. As a result individuals can contain within them the collective dialogue between speaker and audience, reproducing in their thinking and practice the rhetoric of knowledge production.

The second stage of the production of scientific knowledge relates to the development by the trained speaker of a topic for discussion through an engagement in a research situation. The choice of topic and procedures of investigation will tend to be guided by the possibilities that these offer for reconstruction into a convincing research report. It is here that individuals acquire the personal experience about which they must speak effectively in order to persuade some audience of the scientific reality of their endeavors. To a large extent one would expect that scientists' appreciation of the nature of persuasive argumentation will influence what is investigated and how it is studied. Until recently, there was no plausible way to speak about extra-sensory perception to scientific audiences: the topic was not investigated. Thus, through individual scientists' perception, their audience is able to influence the research process itself.

As we move from stage two to stage three, we shift from experience itself to a reconstruction of that experience in the form of a persuasive argument. It is with publication that scientists speak about the rhetorical situation of their research so as to persuade a skeptical audience of the universal character of their personal conclusions. At what point one can talk about "publication" is not clear. In general, however, I would argue that one can place publication as that point in the process of knowledge construction where the individual or collective involved in research relinquishes exclusive control over the meaning of that work and subjects it to a critical audience that is not internal to the investigation. While this point may refer to telephone conversations, pre-prints, sending manuscripts to journals, and the like, it is not unreasonable to associate it with actual publication in a journal. At this stage, traditional methods of rhetorical analysis could come into play in order to examine the process of argumentation employed by scientists in efforts to persuade their audience of the validity of their conclusions.⁴⁰
Stage four of this process of scientific knowledge construction is temporally the longest. Here scientific audiences provide authoritative judgments on the published argumentation of their colleagues, deciding whether or not they should be treated as knowledge. As a rhetorical process we are here dealing with criticism of arguments that is based on their success or failure at the presentation of evidence, their persuasiveness, or some technical defect in the use of argumentation. These critical judgments may be displayed in conversations, published comments, citations, review articles, and so on. The consensus of scientific audiences, however, is available only after some time, when these critical judgments have been assimilated and general agreement reached on the status of a particular piece of work. It is this consensus which transforms a published argument into knowledge or nonsense.

The Analysis of Scientific Discourse: The Case of Sociology

The easiest place to begin the application of this rhetorical model is with an examination of stages three and four, where the published arguments are susceptible to the tools of rhetorical analysis. In choosing a scientific discipline as an exemplar, however, it is likely to be most convincing if I use one of the social sciences; thus I draw on Perelman’s pioneering efforts. Clearly, the natural sciences are a tougher nut to crack. Sociology recommends itself because I am myself a sociologist, because what is taken as sociological knowledge is being increasingly pressed into practical service by government and private institutions, and because there are sufficient reasons to believe a priori that its argumentation is far from deductive in form.

The kind of framework which Perelman provides may well be unique in contemporary rhetorical analysis. Given his intention to develop a theory of argumentation for the human sciences where non-deductive reasoning is used, I have chosen to adapt and expand his work. The resulting analytic scheme has proven helpful (when applied to journal articles) in tracing the various ways in which researchers provide their audiences with a sense of the inevitable reproducibility of their findings and conclusion. In this
brief essay the scheme has to be skeletal in its presentation. In essence, the scheme directs the analyst to a series of inquiries about each article in order to understand how it may be reconstructed as a rational attempt to establish particular conclusions as plausible. Moreover, in practice, analysis should be guided by the organization of specialties in sociology, whether one conceives them as based on a research tradition, on a particular technique (experiment, survey, observation, and the like), on an invisible college, on broad or narrow paradigms and exemplars, or whatever. Here I shall present the scheme quite generally.

First, authors need to use a language that is shared with their audience, thus one needs to discover what special vocabulary (in addition to terms shared by all English-speaking sociologists) is employed. Second, there have to be some agreements on how the argument is to begin and how the attention of an audience is to be secured; however, most pieces begin by an attempt to secure the audience's attention.

There are, moreover, specific agreements that a speaker has to presume in order to make an argument. For sociological discourse, there will be assumptions about the character of society and the persons that are its constituting elements. Second, the speaker has to presume that what is being reported can be justified as important to investigate and that the techniques employed are appropriate. Finally, some set of "facts" will have to be asserted as a starting point. Of course, in this display of "facts" one finds evidence of the current epistemic status of other work in the field.

Nonetheless, the core of such a rhetorical analysis must be to reconstruct the form of argument employed so that one can find how the conclusion drawn is plausible. I have employed four broad types of argument. First, one might find more or less complete deductive arguments; a variety of inferential forms can be employed.44 Of considerable interest here must be the sources of the various premises that ground such arguments. Logical forms are included here in order to insist on the rhetorical character of any argument when made before an audience. Logic is not self-uttering, and even self-evident conclusions may fail to persuade! Second, one can expect "quasi-logical" arguments, those which acquire their plausibility from a gross similarity to logical forms, e.g., incompatibility, ridicule, comparison.
The other two types of argument are perhaps less familiar. These try to establish the "reality" of some factor (a cause, an intention, a general rule of conduct, and the like) by establishing their connection to either some already accepted "real" or establishing a general reality from a particular one. The classic and most familiar form of the argument, which tries to establish a connection of some event to what is recognized as a "reality," is the causal argument. Here, through an establishment of sufficient and necessary conditions, through correlation, or simply through assertion, a "real" effect is shown to be related to a cause that may then plausibly be taken as equally "real." Another form of this kind of argument, familiar to sociologists, asserts the "reality" of some individual's conduct as caused by the agreed "reality" of the individual's membership in some social group.

The last type of argument, the establishment of the nature of some general "reality" through a particular instance, can best be illuminated in the argument from example. Here some particular instance of social conduct is treated as providing evidence "real" enough to justify drawing a conclusion about the "reality" of a general rule that it particularizes. The technique of drawing conclusions about specific populations of individuals from limited samples is a very characteristic use of this argument.

In addition to the general argument that serves as the basis for a speaker's conclusions, there will also be a variety of subsidiary arguments that buttress this main line. One may use precisely the same four types of argument to analyze their variety, placement, and function in the overall texture of the article. Finally, one may need to consider "non-technical" arguments, those pieces of evidence that are excluded from the formal rhetorical argument. Thus, the prestige of the speaker's institutional affiliation, the speaker's reputation, the kind of foundation or granting agency that funded the research, whether or not the paper has been read previously, and the like, may all affect the plausibility of the argument.

Having developed a series of rhetorical descriptions of journal articles in sociology, what is one to do with them? The instrumental pursuit of rhetorical analysis is meant to provide an explanation (even if partial) of the conditions (particularly the rational conditions) which influence validating audiences in establishing the
epistemic status of attempted contributions to knowledge. Once one is satisfied that scientific (in this case, sociological) knowledge is produced through non-deductive techniques, then it is of some considerable moment, in the face of the preeminent legitimacy of scientific rationalizations, to understand just what techniques are used.

In the case of sociology, then, it would be necessary to investigate the relation between various forms of argument and the actual, or probable, acceptance of the conclusions presented through them as knowledge. Are all forms of argument equally plausible? I would suspect that they are not; rather, that there are quite well-defined audiences which are differentiated by their expectations about argumentative form. Experimentalists, for example, regard all conclusions as anecdotal unless they are presented through hypothetical inferences and the consequent affirmed or denied experimentally. On the other hand, the vast majority of conclusions in sociology is presented through much less plausible arguments. It is by no means obvious the extent to which argument, rather than pre-existent intellectual commitments, influences acceptance of conclusions. Only some considerable investigation can answer such a question.

Conclusion

It has been alleged that when we conceive the construction of scientific knowledge as involving persuasion, consensus, and presumably, therefore, politics, we fall into a relativism that fails to explain the cumulative success of scientific knowledge in understanding the world around. Further, it has been alleged that such an approach rejects the privileged position of scientific knowledge on behalf of a new variant of an intellectually destructive "romanticism." Perhaps.

It is certainly clear that this discussion of the scientific community, coupled as it is with an invitation to a rhetorical analysis of science, may undermine the mystique which some reconstructions of scientific practice have fostered. Yet, if such an approach leaves the cumulative success of scientific knowledge unexplained, must we throw it out? Do we allow the disguised sophistry of an appeal
to successful practice (and hence, in my formulation, successful argumentation) stand unexamined? If we are to reach beyond relativism and commitment to privileged knowledge, then it should not be on the basis of a run-down Cartesianism, or a sophistry which masks the consensual character of its own criteria of success.

Few would doubt that scientific knowledge holds a privileged position in this contemporary world. But debate still rages on over what is, or should be, the basis for this privilege. Is our capacity to wonder so eroded that the spectacle of a human community producing a privileged body of knowledge out of its activity and discourse leaves us unamazed? A rhetorical analysis provides a chance to wonder systematically how individual beliefs can become collective and privileged knowledge through a process of persuasion. In our world, where the allegedly self-evident character of scientific knowledge so often blocks attempts to question the appropriateness of action based on such knowledge, a rhetorical approach to understanding how scientific knowledge is formed can restore questions of the purposes of scientific inquiry, or practical action based on scientific knowledge, to rational discourse.*

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NOTES

1 Three recent sources can serve as an introduction to this tradition. Joseph Ben-David and Teresa Sullivan, "Sociology of Science," in Annual Review of Sociology, vol. 1, ed. Alex Inkeles and Neil Smelser (Palo Alto, California: Annual Review Inc., 1975), pp. 202-22 provides a functionalist overview of recent literature. On the other hand, the papers of two recent symposia that appear in American Behavioral Scientist 19, no. 6 (July-August 1976), and Social Studies of Science 6, nos. 3 and 4 (September 1976) offer a different, and from a rhetorical viewpoint, more stimulating series of approaches in the sociology of science.


3 This is well summarized in Criticism and the Growth of Knowledge, ed. Imre Lakatos and Alan Musgrave (Cambridge: At the University Press, 1970).


Herbert Feigl, "Operationalism and the Scientific Method," Psychological Review 52 (1945), 257.


Polanyi and, after him, Kuhn and Ziman, established the injunctive character of scientific training. For these three scholars, neophyte scientists learn research by following the authoritative practice of their tutors, and Weimer's grasp of this relationship as "injunctive tuition" is in accord. However, it is not clear, as he also asserts, that scientific communication "very literally is a 'cookbook' endeavor — it is a matter of recipes for conceiving, perceiving, and doing, and the recipes are given as injunctions" (p. 12). Certainly, Polanyi insists that there are no such formally explicit rules for doing research. As persuasively, H. M. Collins, "The Seven Sexes: a Study in the Sociology of a Phenomenon or the Replication of Experiments in Physics," Sociology 9, no. 2 (May 1975), 205-24, presents evidence that the command of scientific communication (presumably, "Go thou and do likewise; i.e., replicate") is not easy to obey. If scientific argument persuades through its injunctive character, then perhaps it succeeds best when the audience accepts this injunction as a subjunctive conditional. If they were to follow the command, they would have considerable difficulty in producing a replication!

Ziman's views on replication are similar to Weimer's. However, Ziman does not base the plausibility of the experiment on its actual replication: "The guarantee of the validity of the information is not that the recipe has been tried ten thousand times successfully... but that the original experimenters are well-trained, skilful and honest, and know that their assertions could be verified by anyone willing to take so much trouble" (ibid., p. 35).

Kuhn, Scientific Revolutions, p. 177.

Ibid., p. 181.

Ibid., p. 179.

Ibid., p. 24.

"Popper" and "critical rationalist" serve, wherever possible, as eponyms for the whole tradition of critical rationalism and falsificationism. The distinctions between critical and comprehensively critical rationalism, between naive and sophisticated falsification and the like, are not generally pertinent here.


Ibid., p. 215.

Ibid., p. 129.

Ibid., p. 177.

"Ibid., p. 24.


Ibid., p. 261.


Chaim Perelman and L. Olbrechts-Tyteca, The New Rhetoric (Notre Dame: University of Notre Dame Press, 1969), p. 10. Of course, it should be recognized that Perelman assumes science to have a demonstrative form of argument and to be, therefore, exempt from rhetorical analysis. Such an assumption is more likely to be evidence of the influence of positivism's reconstructed logic of science than a conclusion based on investigation.

Here I am alluding to Lloyd Bitzer, "The Rhetorical Situation," Philosophy and Rhetoric 1 (1968), 1-14. Clearly his view that "the scientist can produce a discourse expressive or generative of knowledge without engaging another mind . . ." (p. 8) is not one to which this essay subscribes.


Gilbert argues that while readers make their own conclusion, or conclusions, "readers . . . may be of the opinion that the paper includes significant conclusions or knowledge claims which the author had not recognized would be so regarded" (p. 295).

Gilbert suggests (correctly, in my view) that "a knowledge claim can only be judged to be knowledge while some group of scientists accepts it as true; when the group disperses, it reverts to being just a claim unless some other group also endorses it" (pp. 299-300).

There have been some efforts, none systematic, to apply rhetorical insights to sociological discourse. The more important are Overington, "A Critical Celebration" (see n. 35, above); Derek Phillips, Abandoning Method (San Francisco: Josey Bass, 1973), pp. 151-79; Andrew Weigert, "The Immoral Rhetoric of Scientific Sociology," American Sociologist 5 (1970), 111-19; and Zollschan and Overington.
Preliminary testing of a classificatory framework for the rhetorical analysis of sociological speech, "Rationality in Sociological Discourse," indicates that it may well be a useful guide to such analyses in the hands of persons with some experience in both rhetoric and sociology. The major difficulty that this framework suffers from at this time is securing agreement among analysts on what is to be taken as the main argument of an article.

The adjunctive conditional, which Weimer takes as the characteristic form of scientific inference, is not a reconstruction that my students and I have employed. Sociologists concerned for logical forms of argument are usually so persuaded of the logical positivist's view as to make hypothetical inferences a more likely reconstruction. Certainly, we have discovered no arguments that are explicit in the use of the adjunctive conditional.

Weichert discusses these arguments in sociology and H. M. Collins, in "The Seven Sexes" (see n. 10, above), illustrates their importance in physics. Rhetoricians who are willing to accept an instrumental use of rhetorical analysis in understanding the production of scientific knowledge will, at some point, have to come to grips with comments like Barkla's that "Certain schools of sociological thought view verbal social interaction [presumably, therefore, scientific discourse] as a form of rhetoric, overlaying and rationalizing a tacit world of power and practice" (p. 337). Perhaps non-technical argument is the top of the iceberg of "power and practice" that emerges in discourse.

Lloyd Bitzer and Edwin Black first introduced me to the work of Kenneth Burke and Chaim Perelman and encouraged my early adventures in rhetorical analysis. Warren Hagstrom and Donna Kasdan were helpful with their criticisms of an earlier version of this essay which was presented at the Tenth Annual Conference of Atlantic Anthropologists and Sociologists, March 21, 1975. This latest revision is further indebted to an anonymous reviewer and the Philosophy and Rhetoric editor's good offices in providing a gallery proof of Walter B. Weimer's important paper (Philosophy and Rhetoric, 10 (1977), 1-29. I remain responsible for whatever errors this essay still contains.