Commentary: When this essay first appeared more than 10 years ago, it built on a small but substantial body of scholarship that declared scientific writing an appropriate field for rhetorical analysis. In the last 10 years, studies of scientific writing for both expert and lay audiences have increased exponentially, drawing on the long-established disciplines of the history and philosophy of science. These newer studies, however, differ widely in approach. Many take the perspective of cultural critique (e.g., the work of Bruno Latour and Stephen Woolgar), whereas others use the tools of discourse analysis (e.g., Greg Myers, M.A.K. Halliday, and J. R. Martin). But, application of rhetorical theory also thrives in the work of John Angus Campbell, Alan Gross, Charles Bazerman, Jean Dietz Moss, Lawrence J. Prelli, Carolyn Miller, and many others. Randy Allen Harris offers a useful introduction to this field in Landmark Essays on Rhetoric in Science *(1997).*

"Accommodating Science" applies ideas from classical rhetoric and techniques of close reading typical of discourse analysis to the question of what happens when scientific reports travel from expert to lay publications. This change in forum causes a shift in genre from forensic to celebratory and a shift in stasis from fact and cause to evaluation and action. These changes in genre, audience, and purpose inevitably affect the material and manner of re-presentation in predictable ways. Two concerns informed this study 10 years ago: the impact of science reporting on public deliberation and the nature of technical and professional writing courses. These concerns have, if anything, increased (e.g., the campaign on global warming), warranting continued scholarly investigation of the gap between the public's right to know and the public's ability to understand.

Accommodating Science

The Rhetorical Life of Scientific Facts

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> Whatever be the subject of a speech, therefore, in whatever art or branch of science, the orator, if he has made himself master of it, as of his client's cause, will speak on it better and more elegantly than even the very originator and author of it can. (Cicero, 1970, p. 19)

Two thousand years ago, Crassus, speaking for Cicero in the dialogue *De Oratore*, could have been describing those public intermediates, the orators of magazine and newspaper columns who interpret

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the wonders of twentieth-century science for lay readers, accommodating new knowledge to old assumptions and trying to bridge the enormous gap between the public's right to know and the public's ability to understand. It is undoubtedly true that, with a few famous exceptions, the accommodators of science speak of it more elegantly than the very scientists themselves. They communicate where the originators of new knowledge might only confuse. Nevertheless, the doubt is bound to occur, what happens to scientific information in the course of its adaptation to various noninitiated audiences? What, if any, changes does it undergo as it travels from one rhetorical situation to another? And how, in turn, is the discourse containing such information transformed?

In a foreword he wrote for Lincoln Barnett's (1968, p. 9) popularization of the theory of relativity, Albert Einstein defined the Scylla and Charybdis of accommodated science writing:

> Anyone who has ever tried to present a rather abstract scientific subject in a popular manner knows the great difficulties of such an attempt. Either he succeeds in being intelligible by concealing the core of the problem and by offering to the reader only superficial aspects or vague allusions, thus deceiving the reader by arousing in him the deceptive illusion of comprehension; or else he gives an expert account of the problem, but in such a fashion that the untrained reader is unable to follow the exposition and becomes discouraged from reading any further. If these two categories are omitted from today's popular scientific literature, surprisingly little remains.

Einstein wrote this in 1948, and one cannot help wondering what his opinion would be after the explosion of scientific popularization that has taken place in the last fifteen years. To illustrate, among older established magazines, the circulation of *Science Digest* grew from 150,000 to 530,000 and *Scientific American* from 425,000 to 710,000 between 1970 and 1984. Over the same period, several new magazine rack popularizations of science have appeared such as *Discover*, *Technology Illustrated*, *Omni*, *Physics Today*, and *High Technology* (*Ulrich's International Periodicals Directory*, 1984). The American Association for the Advancement of Science, the publishers of *Science*—which itself grew in circulation from 155,000 to 700,000 in the last fifteen years—sponsored what may be the most successful of these organs of accommodation at a circulation of almost 800,000: the magazine that changes its name every year, *Science79* to *Science85*. Established magazines like *National Geographic* and *Smithsonian* have also changed their editorial policies within the last few years to include more coverage of scientific subjects.[1]

At the same time, there has been a wonderful proliferation of book-length translations of science—not all of them by Isaac Asimov. Many are written by practicing scientists who, like Carl Sagan, Stephen Jay Gould, James Trefil, and Lewis Thomas, have discovered a public voice. We have, for example, the prestigious *Scientific American Library* in which scientists of the stature of geneticist Richard Lewontin, physicist Steven Weinberg, or chemist Peter Atkins explain, respectively, human diversity, and subatomic particles, and the second law of thermodynamics. And we have series by accommodators like Jonathan Miller that aim a bit lower, *Darwin for Beginners* and *DNA for Beginners*, and even the "classic comics" of science, *The Cartoon Guide to Computers* and *The Cartoon Guide to Genetics*. One wonders how much of all this avoids Einstein's double pitfalls of obfuscation and oversimplification.

Although the sociology of science and the corollary investigation of the rhetoric of scientific communication have grown in recent years (Bazerman, 1984; Gilbert & Mulkay, 1984; Latour & Woolgar, 1979; Myers, 1985), the study of the accommodation of science from expert to lay audiences is a relatively untouched subject. To investigate accommodated science writing from the rhetorician's perspective, I have located a number of paired communications that cover similar subjects but are addressed to audiences with different levels of background information and different degrees of interest. Much of my evidence comes from matched articles in *Science* and *Science82*, *Science83*, *Science84* and *Science85*. Using this selected data, I want to make three interrelated observations: first on the genre shift that occurs between the original presentation of a scientist's work and its popularization, second on the change in "statement types" that occurs when a larger audience is addressed, and third, on the usefulness of classical stasis theory in explaining what goes on in the "rhetorical life" of a scientific observation.

THE GENRE SHIFT

Aristotle's tripartite division of kinds of oratory provides a continually useful system for classifying discourse. Basically, Aristotle distinguished three types of persuasive speech—forensic, deliberative, and epideictic—according to purpose, audience, situation, and the time domain concerned. Forensic oratory is the oratory of the law courts where litigants argue over the nature and cause of past events. Deliberative oratory has its place in legislative assemblies convened to debate the best possible course of future action. And epideictic oratory concerns a current, here-and-now judgment over whether something deserves praise or blame; funerals and awards ceremonies are the natural settings for epideictic discourse that ultimately aims at solidifying the values of its audience (Aristotle [sic], 1984, pp. 2159-2161).

A case can be made for classifying original scientific reports as forensic discourse. Scientific papers are largely concerned with establishing the validity of the observations they report; thus the swollen prominence of the "Materials and Methods" and "Results" sections in the standard format of the scientific paper and the prominence given to tables, figures, and photographs that stand in as the best possible representation for the physical evidence the researcher generated. Of course scientific papers are also to some extent deliberative; they cannot ignore creating a reason for their reporting. The point of making the reported observations has to be established in the opening paragraphs and their place in an ongoing debate and the suggestions they yield about future work have to be established in the concluding "Discussion" section. But much of the relevance of scientific articles is extratextual, not spelled out in the discourse but supplied by context, by the assumed inferences the intended audience will make. One need only think of the deceptively simple statement with which Watson and Crick closed their initial paper on DNA: "It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material" (quoted in Judson, 1979, p. 198). The original *Nature* audience immediately recognized the enormous consequences of the discovery; Watson and Crick could afford to be coy. In a similar way, scientific papers are, for the most part, explicitly devoted only to arguing for the occurrence of a past fact; significance is largely understood.

Accommodations of scientific reports, on the other hand, are not primarily forensic. With a significant change in rhetorical situation comes a change in genre, and instead of simply reporting facts for a different audience, scientific accommodations are overwhelmingly epideictic; their main purpose is to celebrate rather than validate. And furthermore they must usually be explicit in their claims about the value of the scientific discoveries they pass along. They cannot rely on the audience to recognize the significance of information. Thus the work of epideictic rhetoric in science journalism requires the adjustment of new information to an audience's already held values and assumptions.

Science accommodators who attempt to bring things down to the level of the *National Geographic* or *Newsweek* or one of the science magazines have, at bottom, only two basic appeals to make in their epideictic arguments. For convenience I will call these "the wonder" and "the application" appeals corresponding to the deontological and teleological appeals in ethical argument. A deontological argument attempts to praise or excoriate something by attaching it to a category that has a recognized value for an audience. In science popularizations, all references to the amazing powers and secrets of nature or of the breakthroughs and accomplishments of the scientists themselves are basically deontological appeals. A teleological argument claims that something has value because it leads to further benefits. An epideictic argument praising the space shuttle, for example, would use the "wonder" appeal if it talked about the "never before" achievements of the machinery, astronauts, and engineers, and would use the "application" appeal if it pointed out spin-offs from the space program. If a scientific subject cannot be recast under these appeals, it is not likely to make its way to a wider audience. As a science writer for the National Institute of Dental Research put it, "Unless it's going to cost less or hurt less, the public doesn't want to hear about it."[2] Subjects in biology and medicine are naturals for these appeals; and so are disproportionately represented in science journalism. Subjects in mathematics, chemistry, and physics are much harder to accommodate.

THE CHANGES IN INFORMATION

Under the pressure of this genre shift from the forensic to the epideictic, it is not surprising that something happens to the information from one kind of discourse to another. To illustrate this change, we can look at a simple example from a pair of articles, the first an original report of research that appeared in *Science* on the discovery of a carrion-eating bee, the second a short accommodated version of this article appearing in *Science82*. Both these magazines are published by the American Association for the Advancement of Science but they are of course aimed at overwhelmingly different audiences, different in background information as well as purpose for reading. Accommodating the scholarly piece for the nonscholarly magazine is not, therefore, simply a matter of translating technical jargon into nontechnical equivalents. Though "mandible" becomes jaw, "carrion" becomes "dead animals," "masticate" becomes "chew" (interestingly enough "regurgitate" stays "regurgitate"), the true accommodation involves finding the points of interest in the topic that will appeal to readers who are not apiologists or even specialists in any life science. (Some accommodation to a wider audience has gone on even in the original piece, which, after all, is not appearing in a journal devoted to bee experts.) In the different rhetorical setting, some of the "information" has changed. We can pinpoint some changes exactly by comparing sentences in the two versions. The original piece makes the following claim:

> (1a) No other protein sources are used by *T. hypogea* [the bee species under consideration], and pollen transporting structures have been lost, making this species an obligate necrophage. (Roubik, 1982, p. 1059)

In the *Science82* version this becomes,

> (1b) Though other bees have teeth, this is the only species that cannot carry pollen. ("Vulture Bees," 1982, p. 6)

The change here is a subtle but significant one; the addition of "only" in (b) gives the second claim a greater degree of certainty than the first. The scientist who wrote the original report and who had just discovered a species of tropical bee unknown before was not about to claim that no other similar species exist and that he had found the "only" one. Less cautious, the *Science82* writer has shifted this information a degree up in certainty. What prompts such a shift is undoubtedly the desire to add to the significance of the subject by claiming its uniqueness, its one-of-a-kind status. In the *Rhetoric,* Aristotle pointed out the perennial epideictic appeal that "a thing is greater when it is harder or rarer than other things" (Aristotle, 1984, p. 2171).

The accommodated version also claims that the bees "eat any animal," an inferential extension from the diet observed and recorded in the *Science* piece. This change is perhaps no more than an innocent hyperbole. But again it is an exaggeration in an interesting direction because it helps to glamorize the danger of the bees—if they eat any animal they could eat us—and glamorizing is the writer's purpose throughout the accommodation, part of his heavy task of bringing a deliberately dry research report into the realm of interesting journalism.

The claim of "uniqueness" serves the epideictic "wonder" appeal so well that we can find evidence of the science accommodation emphasizing the uniqueness of its subject, whereas the original science report downplays it. The following paired statements come from articles about how cheetahs show amazingly similar blood profiles.

> (2a) The cheetah is unusual but not the only mammalian species with low levels of variation [in blood profiles]. The northern elephant seal (30), the moose (31), the polar bear (32), and the Yellowstone elk (33) have been reported to have diminished levels of variation. (O'Brien, Wildt, Goldman, Merril, and Bush, 1983, p. 461)
>
> (2b) Such remarkably high levels of genetic uniformity are usually found only in specially bred laboratory mice. ("Copycat Cheetahs," 1983, p. 6)

The scientist-authors of (2a) want to diminish the singularity of the phenomenon they have observed; because their purpose is to convince readers of the validity of their observation, the rarer the phenomenon is, the harder their job. Their observations are more plausible if other similar ones have been made, so they naturally cite analogous reports. But the science accommodator wants to make readers marvel at something, so he leaves out any mention of species that have shown similar genetic invariance and makes his subject seem more wonderful by claiming in effect: "Here we have animals in nature exhibiting the genetic conformity of those bred for that very quality in the laboratory." The science accommodator is not telling an untruth; he simply selects only the information that serves his epideictic purpose.

The same pair of cheetah articles shows the tendency to exaggeration that also serves an epideictic purpose.

> (3a) The estimate [of genetic variety, or in this case, lack of it] is derived from two conventionally studied groups of genes: 47 allozyme (allelic isozyme) loci and 155 soluble proteins resolved by two-dimensional gel electrophoresis. . . . The entire cheetah sample was invariant at each of the 47 loci. (O'Brien et al., 1983, p. 460)

> (3b) But all the cheetahs carried *exactly the same form of every one* of the 47 enzymes. . . . In another test of *more than* 150 proteins, 97% of them matched in the cheetahs. [emphasis added] ("Copycat Cheetahs," 1983, p. 6)

The original does not editorialize about the information it reports, but (relying on readers to do so) the *Science83* accommodation uses intensifying phrases—for example, "more than 150" when the total is precisely 155, and "exactly the same form of every one," a phrase that adds the ring of the carnival barker, whereas the original (3a) simply announces, "invariant."

Along with claims of rarity and exaggerations, any assertion that something is "the first" of its kind is also a way to argue for its significance and value as the following pair from articles on homosexuality demonstrates.

> (4a) This sex difference in the LH response to a neuroendocrine challenge is a critical feature in any evaluation of hormone responsiveness and sexual orientation: to our knowledge, this is the first simultaneous direct comparison of heterosexual and homosexual men with heterosexual women. [second sentence, fifth paragraph] (Gladue, Green, & Hellman, 1984, p. 1497)

> (4b) Some homosexual men have been shown for the first time to differ from heterosexual men in the way they respond to hormones. [first sentence of the article] ("A Biological Basis for Homosexuality?" 1984, p. 8)

In the accommodated article for lay readers, the claim that the study reported is the first of its kind is heightened by giving it the prominence of first sentence position; the original mitigates this claim both by hedging it ("to our knowledge") and by burying it in the text.

Looking again at the articles on scavenger bees, we find another significant difference between an original report and its translation for lay readers. Based on his field observations, the scientist-writer who found the carrion-eating bees makes the following highly qualified claims:

> (5a) The bees masticate and consume flesh at the feeding site. They do not carry pieces of flesh to the nest, but *appear* to hydrolyze it with a secretion produced by either mandibular or salivary glands, which gives the feeding site a wet appearance. Individual bees captured while feeding, then forced to expel the contents of their crop were carrying a slurry of flesh, measuring between 37 and 65 percent dissolved solids

> by volume.[17] [Note 17 gives more precise information on how the collecting and testing were done, giving a "regress" of specificity for more inquisitive readers.] Bees tagged while foraging in the morning continued to depart and arrive at a carcass throughout the day, *suggesting* that animal food is passed by trophallaxis to other workers in the nest. Nest mates *may* then convert flesh into glandular substances. [emphasis added] (Roubik, 1982, p. 1060)

But in the accommodated version of essentially the same information, the "appears" and "suggests" have vanished.

> (5b) The bees chew flesh after coating it with an enzyme that breaks it down. [The hydrolysis mentioned in the first version requires an enzyme.] They partially digest it, then fly back to the nest, where the substance is regurgitated to fellow worker bees. ("Vulture Bees," 1982, p. 6)

In the space limits of a short notice in a magazine of popularized science, there is no room for the qualifications a more knowledgeable audience would demand, qualifications that show the author's awareness of the criticism and refutation that an expert audience could raise against his inferences. To protect himself from such refutation, the scientist-author has naturally hedged his account. But because he fears no such challenge, the accommodator is far more certain of what is going on among the tropical bees. When qualifications are omitted, the result is greater certainty for the remaining claims. These omissions once again serve the accommodator's epideictic purpose, for only certainty can be the subject of panegyric. To address the public on these subjects requires claiming their significance, and there is simply no way to address the public with the significance of findings that are so carefully hedged their reality seems questionable.[3]

Science accommodations also show another interesting tendency to replace the signs or data of an original research report with the effects or results, once again increasing the significance and certainty of their subject matter. Scientists-as-authors will retain wording as close as possible to their observed results, even though such a practice leads to complicated and verbose phrasing, whereas a popular account will naturally replace these substantives-as-signs with substantives-as-effects. In other words, accommodators will leap to results, whereas the original authors stay on the safe side of the chasm. We can see this process going on in the following two excerpts from original and spin-off articles on the possibility of identifying a cancer genome.

> (6a) A similar analysis performed on the DNA taken from either the patient's normal bladder adjacent to the tumor, or from peripheral blood leukocytes, *showed the same two bands at 410 and 355 nucleotides,* indicating the presence of the same two alleles as were present in the patient's carcinoma (Fig. 3; other data not shown). Thus, *the alteration identified in this gene at the Nae I or Msp I site by restriction enzyme cleavage* appears to be in the germ line and must have existed before development of the bladder carcinoma.... Thus, it is tempting to speculate that there is an association between *this point mutation in the c-ras*$_1{}^H$ *gene* and the bladder carcinoma. Although we have not information at present regarding the frequency of the mutant *c-ras*$_1{}^H$ *gene in bladder tumors, we do know that this change is infrequent in the general population since analysis of DNA from 34 individuals revealed the presence of the Msp I/Hpa II site.* (Muschel, Khoury, Lebowitz, Koller, & Phar, 1983, p. 855)

> (6b) Researchers from the National Cancer Institute and Yale University Medical School believe they have found, in both normal and diseased cells of a bladder cancer patient, a mutant gene that may have caused this malignancy. Their finding indicates that people may inherit certain genes that predispose them to developing some types of cancer. ("A Cancer Gene?" 1983, p. 10)

The noun phrases underlined in the first passage show the authors' tendency to stay close to their precise experimental data; the bands were generated by gel electrophoresis and the sites are positions where particular enzymes have cut the DNA. These are signs. The popularization uses only the inferred artifact, the "mutant gene."

Science and *Science85* contain a pair of articles on bears that demonstrate still another telling difference between expert-to-expert communication and the overhearing that goes on in accommodation (Nelson, Beck, & Steiger, 1984, and "Hibernation: the bear's metabolic magic," 1985). Science popularizations not written by scientists themselves are not usually based on published research alone; the compiling editors of science magazines also consult the original researchers in telephone or personal interviews. Thus the accommodated pieces often contain direct quotations from the scientists in wording more straightforward than they are likely to use in writing. In interviews the consulted scientists also make observations and conclusions not found in the original articles aimed at peer audiences. Thus the accommodated paragraphs on hibernating bears contain the principal author's assertion, nowhere mentioned in the original research report, that the bears bring the level of urea in their blood down by converting urea into protein and in effect digesting it, a striking claim given that it suggests that a mammalian metabolic system has evolved the ability to turn a waste product into food, ("Hibernation: the bear's metabolic magic," 1985, p. 13) The *Science85* piece also claims that research into the metabolism of hibernating bears may someday "lead to substances that can promote similar processes in humans with kidney ailments" who now depend on dialysis, a very desirable spin-off from basic research indeed. It is easy to imagine the prompting question from the science accommodator who wants to elicit a practical application, fulfilling the second of the two major appeals that accommodated science articles can have. Because such speculative applications are rarely mentioned in reports to peers, they must be solicited "off the cuff." But are these speculations claims that the researchers could support before a more critical readership? Or have they come "down" too quickly?

A slight legerdemain in phrasing, changing qualified claims into certainties, omitting contradictory evidence and giving space to unsupportable claims, hardly seems of more than academic importance when the topics are, among the articles sampled, bees and lizard tails and fly larvae and hibernating bears and sailing clams and horses and jet lag. But what about subjects like the role of viruses in cancer or arthritis, the cholesterol factor in the diet, or the potential of recombinant DNA research? I have selected one topic where the consequences of misunderstanding are far from benign and have followed it into popular accounts: the reported inferiority of girls to boys in mathematical ability.

In 1980, two Johns Hopkins psychologists reported in *Science* that seven years of screening for the mathematically precocious had netted far more boys than girls (Benbow & Stanley, pp. 1262-1264). Because the researchers tested seventh graders who presumably had all had the same academic exposure to math, the results weakened the hypothesis that a disparity in scoring was due to the fact that boys take more math courses than girls do. With astonishing rapidity, Benbow and Stanley's work found its way into *Newsweek, Time, The New York Times, Reader's Digest, People's Weekly, Science Digest, Ms, Psychology Today* (and perhaps even the *National Enquirer*). These popularizations show the same tendency, observed above, to increase the certainty of the claims made in the original. There are other subtle and less than subtle differences created by titles, subtitles, artwork, omissions, and the juxtaposition of remaining points, as well as the changes of wording in comparable statements focused on here. To give just a few of these other differences: the original Benbow and Stanley piece was entitled "Sex Differences in Mathematical Ability: Fact or Artifact?" a question that suggests the possibility of genuine debate or contradictory evidence. The title of a spin-off in *Time* magazine is "The Gender Factor in Math" a statement that presupposes the certainty of its referent in a way that a question does not. *Newsweek* at least keeps the question mark, but the title "Do Males Have a Math Gene?" skews their coverage by suggesting that Benbow and Stanley observed a difference caused by inherent aptitude, not to mention the absurd suggestion that a single gene could be responsible for such a complex phenomenon as mathematical ability.[4]

The popularizations give some coverage to preexisting viewpoints that differ from Benbow and Stanley's, but this attention differs in the effect it can have depending on whether or not the article ends with a disagreement or with a reiteration of Benbow and Stanley's position (or their version of it). If Benbow and Stanley have the "last word" about anything, then it seems as if they have made a successful rebuttal of their opponents. In other words, although the newsweekly pieces may be following some journalistic principle of organization, inverted pyramid or "I" structure, they inevitably have argumentative structure and by their arrangement influence, even create, the reader's opinion.

Original and spin-offs also show great differences when we can match comparable statements. I have selected here the authors' concluding statements that are carefully hedged in the original research report but appear much more certain when they are addressed to millions of readers.

> (7a) *We favor the hypothesis that* sex differences in achievement in and attitude toward mathematics result from superior male mathematical ability, which *may* in turn be related to greater male mathematical ability in spatial tasks.[12] This male superiority is *probably* an expression of a combination of both endogenous and exogenous variables. *We recognize, however, that our data are consistent with numerous alternative hypotheses.* Nonetheless, the hypothesis of differential course-taking was not supported. *It also seems likely that* putting one's faith in boy-versus-girl socialization processes as the only permissible explanation of the sex difference in mathematics is premature. (Benbow & Stanley, 1980, p. 1264)

The hedges and qualifications, which have been underlined in the quotation from the original research report above, disappear in the following popular accounts.

> (7b) The authors' conclusion: "Sex differences in achievement in and attitude toward mathematics result from superior male mathematical ability." ("Do Males Have a Math Gene?" 1980, p. 73.)
>
> (7c) According to its authors, Doctoral Candidate Camilla Persson Benbow and Psychologist Julian C. Stanley of Johns Hopkins University, males inherently have more mathematical ability than females. ("The Gender Factor in Math," 1980, p. 57)
>
> (7d) Two psychologists said yesterday that boys are better than girls in mathematical reasoning, and they urged educators to accept the possibility that something more than social factors may be responsible. ("Are Boys Better at Math?" 1980, p. 107, col. 1)

Newsweek in particular tended to sensationalize Benbow and Stanley's data. One of the researchers' samples, eighth-graders who took the test in 1976, was so small, numbering only 22, that Benbow and Stanley explicitly omitted it when they reported the limits of their results: "To take the extreme (not including the 1976 eighth graders), among the 1972 eighth graders, 27.1% of the boys scored higher than 600, whereas not one of the girls did" (Benbow and Stanley, 1980, p. 1263). But *Newsweek,* searching for extremes to heighten the significance of its report, exercised no such restraint: "Among eighth-grade subjects in 1976, more than half the boys scored above 600 of a possible 800, but not one of the girls did" ("Do Males Have a Math Gene?" 1980, p. 73).

We could attempt to formalize observations of such changes in information between original and accommodated versions by borrowing the taxonomy of statement types suggested by sociologists Bruno Latour and Stephen Woolgar (1979, pp. 77-79) in their discussion of scientific discourse. Briefly, Latour and Woolgar distinguish among five types of statement according to the degree of certainty they convey. Type five statements are the most certain; they assert the sort of knowledge that seems self-evident to insiders, knowledge that only surfaces when an outsider's questions force the exposure of presupposed information. Type four statements consist of uncontroversial information that is nevertheless made explicit; scientific textbooks pass on the expressed certainties of type four statements, and accommodated science writing consists of type 4 and occasionally type 5 statements. The following sentences, for example, appear in a *Science84* notice of research on a possible arthritis virus.

> Simpson and his coworkers have now discovered that the agent, which they call RA-1, is similar to paroviruses—a family of viruses rarely found in humans. Paroviruses are extremely small, about a quarter the size of a flu virus; they have single-stranded DNA instead of double-stranded as do most DNA viruses; and they are usually resistant to heat and harsh solvents. ("Arthritis Virus," 1984, p. 8)

Latour and Woolgar would classify this definition of "paroviruses" as a series of factual, type 4 statements. Because the anonymous author of this passage already assumes some scientific knowledge on the part of the audience, type 5 statements do not appear. They would if a naive reader asked for definitions of "virus" or "DNA," or if a science writer, aiming lower, thought it was necessary to explain these basic terms.

Type 3 and type 2 statements have hedges, qualifications, or "modalities" that suggest that the information conveyed is not indisputable. In type 3 statements, the modalities can be subtle; just the citation of a numbered reference or source following an assertion slightly weakens the certainty of a claim because it suggests the need for backing. That is the inverse of what we usually assume citations accomplish, but according to Latour and Woolgar's scheme, a statement like the following is qualified simply because of the numbered citation closing it: "The one example of a viral pathogen causing chronic arthritis of a mammalian host is the caprine arthritis-encephalitis retrovirus that elicits a proliferative synovitis and periarthritis in older goats" (Simpson, McGinty, Simon, Smith, Godzeski, & Boyd, 1984, p. 1425). Citation-like hedges can also appear in the wording of a claim: "It was recently reported . . . that parovirus-like agents can be isolated from the synovial tissue of patients with severe RA disease" (Simpson et al., 1984, p. 1425). Type 2 statements are created when the qualifications are stronger, when, for instance, the wording draws attention to the availability of evidence or lack of it: "There is some evidence to support the notion that a series of events may be required for malignant transformation (2, 14-16) and transformation of NIH 3T3 fibroblasts may represent only a subset of those events" (Muschel et al., 1983, p. 855). Such type 2 statements, prevalent in the examples like (6a) above taken from research reports, include words and phrases like "may," "seems," "suggests," and "appears to be," which convey the tentative status of the claim: "This result suggests that the mutant allele is present in the germ line of EK" (Muschel et al., 1983, p. 855).

Finally, type 1 statements are openly and frankly speculative, admitting the insufficiency of evidence and the very tenuous nature of a claim. Such type I statements are most likely to occur in private discussions among scientists, but they may occasionally appear in a scientific paper: "Thus, it is tempting to speculate that there is an association between this point mutation in the c-ras_1^H gene and the bladder carcinoma" (Muschel et al., 1983, p. 855).

Latour and Woolgar's taxonomy attempts to be very sensitive to minute changes in the certainty of claims, and the changes demonstrated in the paired examples quoted above could be described as changes in statement types according to Latour and Woolgar's scale. Thus the change from 5a to 5b cited above is a change from a type 2 qualification to a type 4 textbook-sounding certainty. In general, accommodated science writing traffics in statements of types 5 and 4, the exposed certainties, and of type 1, the weakly supported and speculative. Latour and Woolgar's scale may, however, introduce a specious rigor into the investigation of what happens to "information" as it travels from limited to larger audiences. After all, the degree of certainty conveyed by a statement may depend more on context than it does on wording. The hedges in Watson and Crick's notice were almost certainly not taken at face value by the original audience.

THE RELEVANCE OF STASIS THEORY

The pressure to be interesting is only one explanation of the changes in statement types and purpose that occur between scientific report and scientific popularization. Another explanation can be reconstructed, oddly enough, from *stasis theory*, a neglected component of classical rhetorical invention. Supposedly developed by second century B.C. rhetorician Hermagoras of Temnos in works now lost (Nadeau, 1964, p. 370), stasis theory was fully explicated by Cicero in his *De Inventione* and *De Oratore*, by Quintilian in the *Institutio Oratoria*, and in the second-century A.D. by Hermogenes in a very detailed treatise, *On Stases* (Fahnestock & Secor, 1983, pp. 136-137). Concerned primarily with legal argument, stasis theory defines and orders the kinds of questions that can be at issue in a criminal case: first question, "What exactly happened and who did it?"; second question, "What was the nature or definition of the act?"; third question, "What is the quality of the act, or, in other words, what were the mitigating or aggravating circumstances?"; and the fourth question, "Who has jurisdiction in this case and what action is called for?" Prosecution and defense tussle over the various issues and if, for example, the defense loses or concedes on earlier accusations, it can take a stand at a higher level: "Well yes, I did take that car on the night of the 18th, but it was really borrowing not stealing" – a defense in the second stasis.

The practical system of ordered questions represented by stasis theory turns out to be a general scheme capable of accounting for the ways issues naturally develop in public forums. People inevitably have to be convinced that a situation exists before they ask what caused it or move to decisions about whether the situation is good or bad and what should be done about it and by whom. We can follow the stasis process with a hypothetical newspaper example: The news media informs us that a jetliner has been hijacked and a certain middle-east faction is responsible; we define this event as an act of "terrorism," instantaneously judge it harshly, and debate over an appropriate response. In the West, and especially in the United States, there is a strong cultural presumption that any situation evaluated negatively (third stasis) demands reform (fourth stasis). So ingrained is this natural logic of issues in our debates that we inevitably move a topic through the four questions.

It is easy to see how stasis theory accounts for the changes in purpose and content between professional and public science reporting demonstrated above. An original, forensic, scientific report engages an issue in the first or conjectural stasis: "Does a thing exist? Did an event or effect really occur?" Claims in the first stasis can be met with denials based on the evidence or on the definitions of key terms. In just this way, Benbow and Stanley's first stasis report was bombarded with contradictions a few weeks after its appearance. For instance, the validity of using the SAT as a test for mathematical ability (as opposed to achievement) was questioned. Notably, these formidable counterarguments appeared in *Science* after the public exposure of Benbow and Stanley's viewpoint, and these rebuttals were not subsequently reported in the popular press. Because they omit qualifications and contradictory evidence, accommodations like those made of the Benbow and Stanley report take it for granted that an issue is settled in the first stasis, and they move on quickly to the next stases: "What is the reason for the effect?" "What value should be placed on it?" and "What, if anything should be done about it?" Thus the *Time* magazine article affirmed male superiority in mathematical reasoning in its first sentence and in its second went on to ask, "Why should this be so?" and finally concluded by quoting the observation that girls should accept the difference and be helped to go on from there, corresponding, as it were, to the last stasis question calling for action.

The movement of a scientific observation through the stases, its "rhetorical life," is an inevitable consequence of changing the audience for a piece of information and thus the purpose of relating it and thus the genre of the discourse that conveys it. A wider, public audience is created by its concern over large public issues that affect the many — such as the mathematical education of boys and girls. And the audience I am talking about here is not so much a demographic classification as it is a reading role that anyone can adopt when reading a large circulation publication. The *New York Times* or *Newsweek* audience would have no interest in staying dispassionately in the first stasis, unresolved between arguments over whether a certain observation was a fact or artifact. Even if the scientific report were translated from insiders' to outsiders' language with the minimum amount of distortion and no attempt to provide an epideictic exigence for the report, the public as readers would move the information themselves into higher stases and ask, "Why is this happening? Is this good news or bad news? What should we do about it?"

CONCLUSIONS

The way that information changes as a function of rhetorical situation certainly deserves scholarly scrutiny beyond this preliminary study, for at issue is the machinery and quality of social decision making in an expert-dominated age. The technique of analysis described in this article could be employed in any number of subject areas so long as the researcher finds similar subject matter being communicated to dissimilar audiences. Of particular interest would be the publications that "translate" legal and financial information — new laws, procedures, entitlements — for the public affected. How, for example, does information about school lunch programs or about small business incentives reach its audience? Can information about a newly available service be separated from an epideictic framework that encourages or discourages an intended audience? Blandly stated information might be interpreted as institutional indifference or even as a warning to stay away, perhaps from an agency that disseminated public funds. The assumptions held by some proponents of the "plain language" movement that meaning can be readily transferred from context to context by mere editorial wizardry needs a second and third look (Siegel, 1985, pp. 98-99).

Another area the writing/rhetorical scholar should investigate is the use of scientific and technical information by political factions and lobbying groups. What happens to technical specifications when they levitate from the engineering manual or report to the briefing memo, the white paper, the money-generating mailing? In these cases, the context clearly switches from one that is fundamentally reportorial or archival to one that is frankly persuasive; the changes in content may be predictable. But what changes occur when the writer's purpose ostensibly remains constant through audience changes, when, for instance, a "second version" purports to be simply a summary or condensed form of the first? Selling summarized information is a growing business in the 1980s as the various segments of our society strive to keep up with one another. Scientists subscribe to abstract services; state and local government officials pay to receive newsletters on a critical federal agency's latest policies. Print can seem too slow, and some of the latest information services come "on line." Yet the above study of science reporting that condenses as it speaks to a different audience suggests that even abstracts and summaries may distort an original in critical ways.

Finally, the fundamental differences demonstrated above between writing for specialists and writing for different publics have certain pedagogical implications, particularly for writing-across-the-curriculum programs. Although the term "writing across the curriculum" and its acronym WAC have been pasted as labels on programs of considerable variety, the "purest manifestations" of the WAC approach, according to a recent review, are the "writing intensive" or "writing emphasis" or "writing concentration" courses in which "students are taught to write by specialists within a particular discipline for the audiences and in the 'modes' and conventions of that discipline" (Griffin, 1985, p. 402). The observations made in this study suggest that the kind of writing students are going to do in such courses will be of a very limited kind indeed; they will learn to write like specialists for specialists. Such writing components cannot replace a full rhetorically based writing course for two reasons: one, they do not give students practice in addressing significantly different audiences and thus practicing the language skills that audience adjustment demands; second, they do not teach the public dimensions and responsibilities of specialist knowledge. The future engineer does not practice public accountability; the English major never tries to convince the uninitiated of the value of literary studies. Furthermore, WAC programs are fueled by certain pieties about "writing as a mode of learning"; they ignore the inevitable "addressed" or rhetorical nature of language and forget that one audience's learning is not another's. There is no "body of knowledge" without bodies of knowers and these are multiple. A WAC program concerned with addressing only nonspecialist audiences would suffer from the same problem in reverse.

Ideally, students in advanced writing programs, who are simultaneously taking courses in their specialties, should have a full writing course that gives them extensive practice in addressing different audiences, specialist and nonspecialist, on subjects drawn from their majors. Only in such a course will students receive the kind of genuine writing instruction that makes "audience addressed" a reason for every language choice. And only in such a course will they experience the problems, moral as well as technical, of accommodating information for different genres, audiences, and purposes.

NOTES

1. Interview, Oliver Payne, Writer, Cartographic Division, *National Geographic*, March 1985.

2. Interview, Pat Sheridan, National Institute of Dental Research, March 1985.

3. Another example of removing the hedges and qualifications comes once again from the articles on homosexuality cited in the text. The original science report explicitly disclaims the causal connection between the hormone response studied and homosexuality, whereas the accommodation suggests the common assumption of a causal relationship between sexual orientation and genetic factors.

> (a) These findings are based on a particular subset of homosexual men and may not apply to all male homosexuals. Since we have measured an adult hormonal correlate of sexual orientation that is causally independent of sexual orientation, a causal relationship should not be inferred. Unknown physiological factors in the adult may account for the differential responses of LH and testosterone reported here. However, even though a developmental relationship between neu-

roendocrine response and sexual orientation is not certain, our findings are not inconsistent with such an interpretation. (Gladue et al., 1984, p. 1498)

(b) Research with animals suggests that some differences between the sexes — females' tendency to be less violent, for instance — are shaped by hormones that begin affecting the brain even before birth. Gladue believes that biological factors may also predispose someone to be homosexual. ("A biological basis for homosexuality?" 1984, p. 8)

4. In responding to letters of criticism in *Science*, Benbow and Stanley once again invoked the precise wording of their original conclusion: "So little of our report is quoted directly [in the letters] that it seems desirable to reproduce our concluding paragraph: [see (7a) in the text]. They were also aware of the misleading nature of popular accounts of their work: "We deeply regret that press coverage of our brief report confused the issues, rather than alerting people to the magnitude of the sex difference." (Benbow & Stanley, 1981, p. 121)

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