Scientific: The principal motive is the establishment and organization of knowledge. To the extent that it attempts to influence human action and opinion, or relies for verification upon communal values . . . , it moves in the direction of rhetoric.

—Walter Beale, 
*A Pragmatic Theory of Rhetoric* (96)

If the technical expert, as such, is assigned the task of perfecting new powers of chemical, bacteriological, or atomic destruction, his morality as technical expert requires only that he apply himself to his task as effectively as possible. The question of what the new force might mean, as released into a social texture emotionally and intellectually unfit to control it, or as surrendered to men whose specialty is professionally killing—well, that is simply “none of his business,” as specialist, however great may be his misgivings as father of a family, or a citizen of his nation and of the world.

—Kenneth Burke, 
*Rhetoric of Motives* (30)

It was once common to think of scientific discourse as somehow above rhetoric—the practice of the courtroom, the political arena, the pulpit and revival meeting—or at the very least, different from rhetoric. In recent years, however, the view has emerged that science definitely has a rhetoric but that it is *rhetorically distinct* from public rhetoric and from writing that *appeals* to science without *doing* science. Writing that appeals to science and the writing of science are produced and consumed from perspectives that are all but mutually exclusive.

In this chapter, after briefly elaborating some points of recent theory on scientific discourse, we will consider the work of several ecological scientists for whom writing is a means of doing science. Their work is generally characterized by a set of preferences that creates a distance between the activities of mainstream science and those of the public realm of environmental politics by favoring theoretical arguments over naturalistic description, by using a specialized technical language instead of employing familiar uses of the same or similar lexicons, by ignoring general human interest in a science of natural (that is, nonhuman) relations, by privileging basic over applied research, and by making a strong distinction between refereed and nonrefereed literature. With an understanding of this special discourse in the background, we can proceed in chapters 4 and 5 to a study of discourses that describe or appeal to science without doing science—namely, science journalism and the instrumental documents of government control.
The Rhetoric of Scientific Writing

How does scientific writing differ from other kinds of writing? Until recently, rhetorical theorists have been satisfied with the view of scientific discourse that distinguishes it on the basis of its superior objectivity or "referentiality" (see Kinneavy). This view accepts an understanding of reality as given, "out there," something to be described. If the activity of science is primarily careful observation, then the discourse of science is a careful recording of that careful observation. In this view, the goals of scientific rhetoric—if science can be said to have a rhetoric—would thus be clarity and thoroughness. Moreover, scientific writing could be grouped together with other forms of reporting—journalism or police reporting, for instance, or any kind of writing whose primary concern is "the facts," whose principal aim is to refer to a world rather than to construct a world.

The main problem with this understanding is that, while it gives us a way of distinguishing scientific writing from political speeches, poetry, and fiction, it does not allow us to make a strong distinction between writing that is taken as authoritative by scientists themselves and writing that presents scientific information for the purpose of influencing social and political actions (the discourse of scientific activism, for example, or environmental impact statements produced by government bureaus, or fact sheets submitted by oil companies to the local newspaper). Nor does this category of "referential discourse" allow us to distinguish between scientific writing and writing that describes scientific or factual information for a nonscientific audience (science textbooks or science journalism, for example). No working scientist would accept a theory that failed to make such distinctions.

The first step in showing that scientific discourse-practice is different from that of simple reporting is to allow that scientific writing is rhetorical, even though to make such a claim is to break with a tradition of discourse taxonomy that goes back at least as far as Aristotle. Nevertheless, it is clear that scientists engage in rhetorical discourse as defined by Walter Beale: "The kind of discourse whose primary aim is to influence the understanding and conduct of human affairs. It operates typically in matters of action that involve the well-being and destiny of communities (and of individuals within them);... and in matters of value and understanding which involve the communal or competing values of communities" (94). In this sense, all discourse is to some degree rhetorical since writing and speaking are themselves "human affairs" and forms of conduct. All discourse is performative, a means to an end. The difference between science and other discourses is a difference in the kind of practices and communities it supports and moves forward. It is read and written from a different perspective with a different agenda for action and a different set of values. The communal destiny and communal values with which scientific rhetoric is concerned are the destiny and values of the scientific community itself. So the question becomes, how is scientific activity distinct from political or journalistic activity?

Science as a Distinct Agenda for Action

There has been, in the last fifty years, much dispute among philosophers of science about the exact nature of scientific activity. Karl Popper has argued that its chief characteristic is falsification, so that when one scientist puts forth a hypothesis or theory, other scientists attempt with all their resources and vigor to prove it wrong. In a famous demurral from Popper's influential view, Thomas Kuhn substituted a normative image of scientific activity less flattering to the individual working scientist than the Popperian image of heroic struggle but more in line with the idea of science as a social activity. That is, following a revolutionary breakthrough in theory—the work of a Copernicus or a Darwin—"normal science" proceeds to interpret the world in light of the new theory and does so until emerging facts place this normative theory or "paradigm" in crisis and a new paradigm comes to the forefront. Imre Lakatos preserves elements of both the Popperian and the Kuhnian schemes in his concept of the rational progress of science depending on the success or failure of research programs. Accordingly, the value of a given program and its durability is determined by its predictive power; a research program fails and is abandoned when its predictions are found again and again to be wrong.

What all of these theories have in common is a view of science as containing its own history. All suggest that science is more or less
insulated from extrascientific influences. For writing to be scientific, it must do science. That is, it must perpetuate a research program and have no interest in influencing actions that lie outside of that research program—no interest in the kinds of social and ethical actions that form the object of environmental impact statements, no interest in the entertainment value of science news, no interest in the aesthetic or poetic value of the reality it portrays. An assertion that the ultimate aim of science is to contemplate or describe “reality” or “nature” becomes a metaphor that is useful to the scientist as a means of distinguishing scientific activity from other activities. It is a means of value labeling. Activities valued by the community are granted currency when this label of favor is attached. To use the categories established by Walter Beale, we can argue that research is considered “scientific” or “objective” if it is considered to be free from instrumental action (in engineering technology and government, for example), the goal of which is to control the state of things; from policy action (in politics, for example), the goal of which is to change the state of things; and from poetic action (in novels, for example), the goal of which is to supplement the state of things. In fact, science may still do all of these things, but only within the limited realm of action certified by the scientific community. Its characteristic discourse may well involve instrumental efforts to control the state of things (in experiments that manipulate nature); policy efforts to change the state of things (in arguments among members of the scientific community); and poetic efforts to supplement the state of things (in predictive models and theoretical speculations about events not yet observed or recorded). The content of these modes of action will differ drastically from the content of political action or poetic action in the extrascientific world. That content is controlled carefully by the scientific community. “Objective” is the compliment paid to the work of a researcher who is “one of us, doing what we want to do in the way we want to do it.”

How Scientific Rhetoric Creates the Facts

In a particularly insightful analysis of the social construction of scientific objectivity, Bruno Latour suggests that, when it comes to the “facts,” scientists are realists. But the facts, far from being the unchangeable solid realities of the positivist, are rather the conclusions of arguments that have been settled within the scientific community. “Cold” science is the retrospective contemplation of certified truths, of “black boxes” that are “closed.” The popular view of science, the notion that science deals with settled facts, is therefore at least half right. It is also half wrong, for it ignores “warm” science, or science in the process of fact making. This portion of scientific activity involves the reopening of factual black boxes, the introduction of new experimental data, the forming of new arguments, the reconstructing of reality. In the controversies of “warm” science, the participants throw over their realism and become relativists. Scientific reality, according to Latour, is “what resists” (93). Arguments clinched long ago resist reopening, while arguments in the process of settlement are vulnerable and nonresisting (see also Fleck 98, Bazerman 61–62).

“As long as controversies are rife, Nature is never used [by scientists] as the final arbiter since no one knows what she is and says,” Latour claims. “But once the controversy is settled, Nature is the ultimate referee” (97). Science differs from politics and poetry because it appeals to “nature” or “reality” only after a certified set of procedures and a certain number of investigators and a certain quality of argument have produced a certified version of nature or reality. Moreover, it admits into its arena of contemplation no constructions of reality developed in competing traditions and perspectives. No creationist, for example, driven by hypotheses suggested in humanist and poetic texts, will ever be admitted into the field of scientists engaged in constructing an image of the world’s origins. Creationism is a theological, not a scientific theory.

What is considered to be factual in scientific research is not necessarily the same as what is considered factual in other discourses produced from other perspectives. In making this observation, we are beginning to establish the ultimate paradox of the scientists’ construction of their own discourse community: It is founded upon a politics that demands freedom from politics and a value system that claims to be value free.
Scientific Discourse: A Case

To add concreteness to our description of the rhetorical process of fact building and discourse certification in the scientific community, we present in this section a profile of the activities and an analysis of the rhetoric of seven ecological scientists at Memphis State University. We developed these profiles by reading the scientists' recently published refereed papers and then interviewing them in order to clarify their overt aims and intentions in writing these papers and to discover their general objectives in research. What emerges, we hope, is a cumulative portrait of the characteristic discourses connected with ordinary scientific activity at a fairly typical state-funded university.

From our analysis of the papers and more especially from the interviews, we established a set of themes that appeared frequently as major concerns of the scientists and that had a strong bearing on our study. The themes tended to develop as a set of oppositions. We will focus on five opposing pairs:

- Natural history vs. theoretical science
- Familiar language vs. scientific language
- Human interest vs. natural science
- Applied research vs. basic research
- Gray literature vs. refereed literature

The comments of the scientists we interviewed suggested that the values of the general community of ecological scientists tend to be strongly associated with the terms on the right in these pairs (theoretical science, scientific language, natural science, basic research, refereed literature).

Later we will show that many journalists, politicians, and environmental activists, in their quest for a global solution to the environmental dilemma, fail to recognize the strength of these oppositions in the minds of research scientists and thereby earn the contempt of the scientific community. Science may well represent a strong example of communicative action, as Habermas suggests (Theory of Communicative Action 2.91–92), but part of the strength and success of this discourse community derives from a scrupulous patrolling of its borders against intervention by extrascientific interlopers.

Natural History vs. Experimental Science

Horkheimer and Adorno have written, “In science there is no specific representation. . . . Representation is exchanged for the fungible—universal interchangability” (10). In a developing modern science like biological ecology, the move is certainly in this direction: out of the field—the buzzing and blooming confusion of specific and varied details (to paraphrase William James)—and into the laboratory, the world of controlled data and descriptions of models that apply not only to a pasture just east of Memphis, Tennessee, but to any plot of land with similar characteristics.

Raymond D. Semlitsch, one of the scientists we interviewed, studies frogs, salamanders, and most recently insects. But he is not interested, as his nineteenth-century counterpart would have been, in cataloging and naming species and placing jars of collected specimens in the glass cabinets of museums. He is interested in “complex life cycles,” in the description of organisms that have at least two stages of life and that live in two different habitats (amphibians, for example, that live part of their lives in water, part on land). He can range in his research from salamanders to mosquitoes because he is a “process- and concept-oriented” theorist rather than a specialist “interested in a specific problem or a specific species.” Thus he is an experimental scientist concerned above all with theorizing about evolutionary patterns.

In his article “Fish Predation in Size-Structured Populations of Treefrog Tadpoles,” Semlitsch describes an experiment on the ability of large tadpoles to survive in the presence of small-mouthed fish (bluegills) as compared to the survivability of small tadpoles. The experiment could not have been done as a simple observation of fish and tadpoles in a lake. Under such conditions, no reliable counts could have been made. And even more important than this practical difficulty, there would have been a methodological objection to simple observation: If data were collected in the uncontrolled setting of a single pond, this information would be pertinent only to that specific pond. As Semlitsch says, “I'm not studying a specific system or a lake
or pond, but common processes in all lakes or ponds.” Any finding that he records should be “important enough that it will occur in any lake or pond, artificial or natural.” So he and his colleague, J. W. Gibbons, ran a double experiment, Gibbons making his observations in outdoor artificial ponds into which a given number of fish and tadpoles were placed, and Semlitsch running similar tests in plastic dishpans in his lab.

The results are summarized in tabular form in the published paper. Centered on the page in the “window” of text that the eye seeks first (centered vertically, and just above horizontal center), and framed by an ever-so-slight addition of white space, a wide column of numbers in a large table presents the data that establish forcefully the relationship of tadpole size to survivability—a string of zeroes in the column indicating how many small tadpoles (averaging about twenty-seven milligrams) survived in the presence of large fish, yielding as the reader’s eye moves down the column to an ever-higher rate of survivability among medium-sized and large tadpoles. With great economy and visual power, the table shows the main result of the experiment, the finding that bigger tadpoles have a better chance of surviving in the presence of gape-limited predators and of succeeding to their adult phase.

The rhetorical tightness and cleanliness of the tabular presentation of data are reinforced by the use of closely parallel headings and subheadings in the text and by the parallel structure of emphatic interpretive sentences describing the significance of the major findings. The significance is reported in the first sentence under each heading: “The body size of tadpoles had a dramatic effect on their survival in the presence of fish” (Semlitsch and Gibbons 322). “The size of fish predators significantly affected \( P = 0.0019 \) the survival of tadpoles in the laboratory experiment . . .” (323). “The full sib family from which tadpoles came did not significantly affect \( P = 0.1208 \) their survival when exposed to fish predators . . .” (324). The findings are important, the conclusion tells us, because they “indicate that environmental factors affecting the growth rate of tadpoles can dramatically alter their vulnerability” (325) and may therefore “act as an agent for natural selection to increase growth rate” (326).

When we arrive at this conclusion, we begin to grasp the deep purpose of counting tadpoles in plastic dishpans in a biology lab. The scientists’ aim is to advance evolutionary theory. Semlitsch is involved in the effort of ecologists to demonstrate how environment affects evolution. Far from being rhetorically neutral, the presentation of his data is vigorous and challenging, especially his use, and even repetition, of the term “dramatic” and his emphatic structuring of his tables and the results section of his text. He is clearly engaged in argument, not in mere description. He is especially interested in staking out a territory in evolutionary theory, in asserting the presence of environmental factors in natural selection—hence the importance of the negative result involving full sib families.

This conflict between ecologists and geneticists—a conflict that, since its origin in the arguments of Lamarckians against orthodox Darwinians, has taken various turns and forms in the history of biology—is taken up even more vigorously in the work of William H. N. Gutzke, who like Semlitsch, specializes in herpetology and environmental science. The agonistic rhetoric of the debate is well exemplified in this passage from his paper, “Influence of the Hydric and Thermal Environments on Eggs and Hatchlings of Painted Turtles”:

Since the “Great Synthesis,” evolutionary biologists have been primarily concerned with genes and the mechanisms by which their frequencies are affected by natural selection. Environmental effects have been relegated to mere causes of “noise” in the system. However, data are now appearing which indicate that genetic differences are not the sole important agent for intraspecific variability. Because natural selection can only act on the phenotype of an organism, the capacity for phenotypic variation in response to environmental variation (phenotypic plasticity) is of great evolutionary significance and must be considered if the complex interactions that enable organisms to survive in their environment are to be understood. (402-3)

Though many of the stylistic features we will encounter in our description of the environmental impact statement in chapter 5 are present in this scientific paper—the favoring of passive constructions and the oblation of human subjects in sentences, for example—none of the blank neutrality of tone appears. In reading, we are aware of an engaged author, devoted to asserting a particular perspective. The
paper, rather typically, begins with an introduction that places previous research in question (in a "review of the literature") and finishes with a conclusion that asserts forcefully a revision in the overall research program of evolutionary scientists.

The commitment to a point or a perspective and the consequential rhetorical heat of these papers are related to the highly theoretical slant of this work. Though rhetoric has been traditionally associated with the muddy worlds of politics and ethics, where “opinion” predominates over “fact,” argument in science is taken up with passion on topics that have at one time or another been considered matters of fact. As Latour has suggested, the topics that are argued in theoretical science were once “closed”—the dominance of genetics in natural selection, for example—but have been reopened by a new research program, in this case, the program of environmental science. When we asked Gutzke’s opinion regarding Latour’s claim that every word and every figure in a scientific paper is devoted to the clinching of an argument, he agreed without hesitation.

The public perception of science as a field of settled facts results from a confusion of the type of science we have been describing with what is generally referred to as natural history, the detailed account of a specific corner of the world in terms of the settled "facts" about that region. In our interview with Semlitsch, however, he cautioned against oversimplifying the distinction between natural history and theoretical science. Using an approach that had obvious affinities to our own, he suggested that we think of the distinction as a relationship along a continuum. Figure 3 represents the continuum we sketched out in our discussion.

<table>
<thead>
<tr>
<th>observing</th>
<th>observing and measuring</th>
<th>manipulating</th>
<th>manipulating in semi-natural conditions</th>
<th>bringing system into lab</th>
<th>theoretical systems, manipulation with numbers, computer mode</th>
</tr>
</thead>
<tbody>
<tr>
<td>in field</td>
<td>in field</td>
<td>in field</td>
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</table>

**Figure 3. Continuum from Natural History to Theoretical Science**

As scientists determine their activities along this continuum of practice, they must realize that they are involved in a system of trade-offs.

One set of certainties is sacrificed for another. At the point where you move out of the field and into “seminatural conditions” (like Gibbons' artificial ponds), “you are sacrificing realism for experimental rigor,” as Semlitsch sees it, “realism for the ability to control parameters.”

From the perspective of rhetorical analysis, this means that, as scientists move from natural history activities to those of theoretical science, they are leaving the realm of “cold science,” the world of positivist fact, and entering the world of “warm science,” of theoretical disputation. As scientists move into this favored realm, however, they distance themselves from the political and social disputation over the environmental dilemma, which occurs in the world of natural (and social) history. This distancing, one of the major factors in the modern construction of objectivity, casts doubt on the immediate usefulness of the conclusions and assertions of "warm science" in decision making about environmental questions. This is the first trap set for those who would make polemics out of the conclusions of a developing science. There are others.

**Familiar Language vs. Scientific Language**

People commonly think of the language of scientific literature as a jumble of latinate technical terms connected with inscrutable formulas. In the science of ecology, an uninitiated reader will be surprised by the relatively high percentage of recognizable terms. But this aspect of familiarity is deceptive; it is another trap for the unwary humanist or social ecologist.

In describing the theoretical aims of his work with complex life cycles, for example, Semlitsch speaks of the organism’s “decision to go from one stage to another.” The use of the term decision would likely offend the ecological economist E. F. Schumacher, who has criticized his fellow economists for similarly applying terms like “planning” to “matters outside the planner’s control” (211). Certainly a tadpole cannot “decide” when to become an adult frog—at least not in the sense of the term as we use it to describe conscious human volition. But in another sense, in the scientifically certified sense, the body of the tadpole does make a kind of decision. There are probably more technical ways of describing what occurs, but they would be
long and awkward. The familiar term becomes a shorthand expression for a series of events which scientific readers will grasp immediately but which lay readers are likely to puzzle over, dwelling upon the absurdity of the image of the tadpole’s big decision to become an adult.

We discussed this anthropomorphizing effect of scientific language with Ronald Mumme, an ecologist who studies how birds’ behaviors are affected by environmental and genetic factors. We suggested that, even in Darwin’s own writings, the problem surfaces in the construction of the metaphor natural selection. An untrained reader of The Origin of Species is likely to perceive in Darwin’s narrative both a theological residue, a substitution of Nature for God as the agent of creation, and a humanistic residue, a substitution of nature for the human breeder as the agent of selection in the famous comparison of natural and artificial selection in cattle breeding. Darwin and the scientists writing in his tradition frequently use what Mumme calls “a teleological kind of language” for “nonteleological” concepts; that is, they write as if nature were a purposeful force, working toward a telos, a preestablished end (like the ultimate good of all beings); but in fact, they believe that occurrences in nature are accidental, chance combinations of events that produce some good fits between an organism and its environment—combinations which lead to the perpetuation of the species—and some bad fits, which lead to extinction. Natural selection itself is a teleological term suggesting that nature has a plan, a favored outcome, but in Darwinian theory the term describes an accidental outcome (see Dawkins).

In using such terms, the scientist is counting on the audience’s ability to read theory into ordinary language. “I’m writing in a convention,” says Mumme, “using a verbal convenience for people used to this language.” Not that technical language for these processes is unavailable. Any of the familiar terms could be “unpacked” and rendered in “unloaded descriptive terms,” but the resulting prose would be “tedious and almost unreadable.” Following are three typical passages from Mumme’s articles on bird behavior. The “loaded terms” in these cases are those with resonances which, for the nonspecialist reader, suggest parallels to human behavior. For the lay reader, the effects of these terms are startling, misleading, and intrusive, but such effects are presumably absent for the scientific reader:

1. These findings are consistent with predictions of inclusive fitness theory, and have led to the suggestion that kin should receive favored treatment in the form of not only cooperation but also reduced aggression or competition. However, selfish, competitive and manipulative behaviors can evolve among close relatives if the gain in direct (individual) fitness exceeds the loss in indirect fitness.

(Mumme, Koenig, and Pitelka, “Reproductive Competition” 583)

2. The establishment and maintenance of territories requires significant investments of time and energy. . . . Because this cost of territoriality may reduce the amount of time individuals have to devote to foraging and breeding, a variety of ecological factors may influence the amount of time individuals devote to territory maintenance. . . . We attempted to evaluate the effects of three ecological factors, food availability, intruder pressure, and temperature, on the amount of time male Carolina Wrens (Thryothorus ludovicianus) devote to vocal advertisement and defense of their territories during the winter.

(Stain and Mumme 11)

3. In many animal species, males frequently use tactics to increase their confidence of paternity in the offspring produced by their mates . . . . Although apparently widespread, paternity assurance behaviors should be especially prevalent among species with male parental care . . . . In these species, males that do not take steps to ensure their paternity risk becoming victims of kleptogamy (cuckoldry), expending their parental investment on the offspring of the other males . . . . (Mumme, Koenig, and Pitelka, “Mate Guarding” 1094)

Apparently, talk of “selfishness,” “costs,” “kin,” “investments,” “advertisement,” and “confidence” in bird behavior raises no objections and is transparently clear to the scientific readers of these passages. Even for technically trained readers, however, the picture muddies somewhat with the introduction of a term like cuckoldry, which must be coupled with the technical term kleptogamy in the last sentence quoted. Mumme tells us that the use of this term has been criticized. He also says that the effort of some scientists to substitute the term rape for forced extrapair copulations was a linguistic move that proved altogether unacceptable. Even for scientific readers, a term like rape...
is too radically invested with emotion to be looked upon with the dry detachment considered to be typical of the objective scientist.

This last example suggests that between the scientific usage and general usage of words, there is no absolute opposition but once again a continuum. At one end of this scale of acceptable diction, we would place purely technical terms, like kleptogamy (though even this term resonates with familiarity because of its etymological and semantic similarity to kleptomania, a term used widely in a culture concerned with private property). Other terms—selfishness and confidence, perhaps—would fall toward the middle of the scale, having no doubt strong personal and human connotations for scientists who use them technically, but not so strong as to interfere in the context of a scientific paper with their semantic propriety within a theoretical framework. Finally, on the far extreme of the continuum, are terms like rape, whose overtones of the conditions of everyday human life are so vivid and strong as to prohibit their effectiveness in functioning as quasi-technical terms. A term like cuckoldry would likely join rape at the extreme of prohibition were it as widely used in contemporary culture as it was in Shakespeare’s day.

The continuum of usage hints at the opposition between behavioral study as a human science on the one hand and behavioral study as an animal science on the other. If certain language is partitioned for exclusive use in the human realm—thus reinforcing the dichotomy between human life and nature—so also are certain topics of human interest banned in “hard” behavioral science. This observation leads us to our next pair of opposites. Since the aim of theoretical science is to expand the generality of conclusions beyond the realm of single species and single environments, why might wonder, Why would behaviorists in the hard sciences create linguistic and experimental limits that prohibit entry into the human realm?

**Human Interest vs. Natural Science**

We took up this troubling question most directly in our interview with Gutzke. After the publication of several of his papers on the relation of environmental factors in reptile behavior and sexual development, he received “some very strange calls from around the nation,” asking him for opinions on the causes of everything from mass murder to homosexuality. But like most hard scientists, his policy is to “stay away from social issues.” We asked what constituted a “social issue.” It is, he said, a topic that “deals with emotions and norms” of human behavior. “Scientific issues deal with data,” he explained—data that can be looked upon as “mere” data, with the scientist following “an accepted statistical analysis,” one that is “accepted by other scientists, experts in the field.” However, since the social sciences deal with data, are they not also truly “scientific”? In principle, Gutzke said, the human sciences could be truly scientific, but in practice, there are problems. Above all, “your own social history is likely to bias your interpretation of data.” He offered abortion as an example. It is an issue that “elicits strong emotions in humans,” but abortions in the organisms studied in hard science are simply matters of death and life, uncolored by human feelings. In dealing with the sexual habits of adult bull snakes, you “don’t have to deal with perceptions,” Gutzke claims. But the phone calls did come in. Thus certain issues in behavioral ecology have inevitable resonances in the world outside the laboratory.

Consider another example. Beverly Collins, a plant ecologist we interviewed, conducted a series of experiments to determine how “canopy gaps, formed by the fall or snap of one or more trees, create patches of altered environment within forests” (Collins and Pickett 3). The experiments involved cutting trees to create openings in the forest ceiling so that additional sunlight and rainfall could be admitted to the underlying layer of vegetation. This work has clear significance for those interested in defending or attacking current practices in the cutting of timber. But this application does not interest Collins, who claims to be “apolitical” and who told us, “Unless we can understand the basic processes for how environments respond, we cannot even understand the applied problems.”

Somewhat ironically, her work entails manipulative practices that distantly parallel the actions of many of the villains of the environmentalism, the North American lumber industry and the South American cattle industry. “Sometimes,” Collins explains, “the best way to get at the questions is to do manipulative field research.” Her manipulations of natural environments—meager though they are—have not escaped
association with the exploits of clear-cutting. In an otherwise objective review of recent work in the field, an author in the British journal *Nature*, writing of Collins' work, let slip a phrase widely used to describe large-scale cutting of trees—"forest destruction"—then passed blithely back into the standard reviewer's style of scientific reporting: "In a piece of experimental forest destruction, Collins and Pickett created gaps in hardwood forest in Pennsylvania by cutting canopy and understory trees at 1 metre above the ground. Herb layer vegetation shows very little response to such gaps. It is reasonable to suppose that soil disturbance is needed if there is to be additional recruitment from the seed bank" (Moore 313; italics added). Whether the phrase in question was intended as a joke or an implied criticism, or whether it was simply an unconscious slip, neither we nor Collins can tell. The association of scientific activity with the questionable practices of industry is, however, clear enough to be disturbing for a scientist interested in keeping clear of human interests and ethical questions in research about plant ecology.

If this association is troubling for a scientist primarily engaged in "basic research," what about those engaged in what is commonly called "applied research" in ecology?

**Applied Research vs. Basic Research**

This opposition formed the most widely mentioned and emotionally loaded topic in our discussions with the scientists. Semlitsch, Gutzke, and Collins all made a definite point to distinguish themselves as basic researchers. Mumme did the same, explaining that a recently funded study on the habitats of the Florida Scrub Jay, an endangered species, is his first excursion into applied research. But three other scientists we interviewed—Stephen Klaine, Mark Hinman, and Gary Wein—are forthrightly engaged in the kind of applied research that government officials and reporters in the mass media seem to consider the major business of scientific investigation. Our interviews showed, however, that these applied scientists were either somewhat defensive about the status of their work in the scientific community or were openly critical of the "elitism" of basic researchers. In mainstream science, applied research is seen as a marginal activity. It slides away from the theoretical aim of purely scientific discourse and engages the question, *What should people do?* It thereby undermines conventional scientific attitudes about the strong dissonances between the opposing pairs we have been sketching, sometimes favoring natural history over theoretical science, familiar usage over scientific diction, and, most importantly, human interest over natural science.

Stephen Klaine and Mark Hinman are environmental toxicologists and coauthors of several recent papers about a project designed to monitor pesticide migration in west Tennessee. It is one of the largest such projects in the world and could have an important effect on agricultural practices in a region dominated by farming—the Mississippi Delta. The following passages from the introductions of two of the papers show a clear connection between the research and the problems that we associate with the environmental dilemma. In the first passage, the discussion is even framed according to the classic opposition between high productivity and environmental degradation:

Pesticide use has resulted in significantly higher crop yields as agriculture increasingly relies on the chemical industry. Pesticide use increased 40-fold from 1946 through 1976 and this trend has continued to the present. . . . The dependence of agriculture on synthetic fertilizers and pesticides will continue to increase as greater productivity is demanded per unit of land. Concurrent with this increase will be a greater flux of chemicals from treated land to untreated land, surface water and ground water.

Characterization of agricultural pesticide migration has become a primary concern. . . . (Klaine et al., "Characterization of Agricultural Nonpoint Pollution: Pesticide Migration" 609).

Nonpoint source pollution can be defined as the diffuse input of pollutants that occurs in addition to inputs from undeveloped land of similar genesis. . . . agricultural nonpoint sources of pollution significantly altered water quality in 68% of the drainage basins in the United States, and in nearly 90% of the drainage basins in the central region of the United States. . . . agricultural sources are probably the major contributors of suspended and dissolved solids, nitrogen, phosphorus and associated biochemical oxygen demand loadings in U.S. waters. . . .

West Tennessee has the highest erosion rates in the country. . . . This high erosion rate and the related agricultural chemical burden
are the primary reasons why west Tennessee rivers and streams have the worst water quality in the state. . . . (Klaine et al., "Characterization of Agricultural Nonpoint Pollution: Nutrient Loss" 601)

Immediately following the introductions are sections on the methods and instruments used to measure contents of the runoff from an experimental field. Results and discussion are given in a single additional section, breaking with the conventional practice in reports on basic research of separating results and discussion or conclusions into different sections. The focus is clearly on results, which largely substantiate the literature but also hint that the overall picture of nonpoint source pollution is worse than previously depicted. The papers try above all to build a more complete data base of information pertinent to the topic.

Thus, instead of a theoretical conclusion to these papers, we get descriptive passages with little argumentation. As an alternative to advancing theory, we might expect the authors to offer a set of recommendations for action, but they stop short of this. To offer recommendations would mean that they had moved all the way out of the field of science and into engineering. In fact, there are engineering professors among the coauthors. Though the human interest of their work has increased, however, it has not overwhelmed the interest in natural science. The point at which they stop might well be described as natural history. In the terms developed along the continuum of figure 3, this work falls just to the right of absolute natural history—"observing and measuring in the field"—with a minimum of manipulation involved in setting up an experimental watershed.

Klaine, the senior scientist on the project, has spent his career developing methods and collecting and analyzing data about the effects of human-generated substances on biological systems, especially plant systems. He has pioneered several bioassays to determine how algal cultures are affected by contact with such human products as wastes from coal gasification facilities. We asked him if his work was oriented more toward the solving of human problems than was the work of the basic researchers we had interviewed. His answer was equivocal. "You have to understand the problem before you can solve it," he said, and this understanding constitutes the aim of applied research and also sets the limits of objectivity to some degree. How will the data generated by the pesticide migration project be used? Eventually it will lead to "better methods for avoiding migration." But the work of applied science does not go that far. Nor can reliable recommendations yet be made.

Klaine affirms that science has an advisory capacity in the overall society in that the data he produces will be used to alter current methods of agriculture. "One would hope that a person making a regulatory decision would have the scientific data to base that decision on." It nevertheless remains important, he insists, that academic scientists be "objective." "We have right-wing fanatics and left-wing fanatics on all questions," he says, "but scientists should not get involved in those extremes. I get support from conservation groups and from industry, but I make no promises at the outset." Objectivity for Klaine means that the data he produces will not be tailored to serve a decision down the line. Although he refuses to draw a definite line between applied and basic research—"couchd within applied research, I ask basic questions dealing with fundamental processes, like how a chemical is degraded by a microorganism"—the construction of objectivity offered by his colleagues who deal more directly with theoretical questions is clearly different from his own. The problems the basic researchers hope to solve are developed and certified within the confines of theoretical science, while the problems Klaine's data will eventually solve arise from life in the muddy world of agriculture, industry, and politics.

Thus for basic researchers, objectivity means distance from general human interest; for applied researchers, objectivity means the refusal to privilege one human perspective over another in advance of the research act. This distinction has a profound effect on the kinds of literature produced by the different approaches to research. Whereas basic research produces strongly argumentative discourse and conclusions designed to clinch a theoretical point, applied research produces open arguments, leaving the conclusions to engineers and policymakers "down the line." As Mark Hinman, Klaine's former graduate student and coauthor points out, moreover, applied research also supplies data for the makers of theoretical models, particularly computer models of ecological processes. Several modelers have asked to use the data from Klaine's project to calibrate and verify their models.
There exists, therefore, a cycle of data usage, with theoretical science feeding methods and interpretations to applied science, which in turn provides data that may be plowed back into theoretical models.

Hinman thinks the distinction between basic and applied research is overstated and is largely a matter of disciplinary politics. He says an “elitism” prevails in efforts to make the distinction stronger than it really is. The aristocracy of basic researchers in natural science prohibits the people best qualified to “make a difference” in environmental matters from doing so: “A big problem with biologists is that they won’t go out on a limb and make predictions. I think it’s time we trained a generation of scientists to do the job done now by engineers and others who don’t understand biology.” Certainly science educators who write textbooks and the renegade scientists who use the mass media rather than the refereed journals to publicize their findings would agree with Hinman; they appear to have flaunted the scientific conventions for constructing objectivity.

Hinman himself has not yet gone this far. For him, to be objective means “sticking close to your data.” “You have to believe your data to arrive at a better picture of what happens in the real world as opposed to what people think will happen. It all comes down to real numbers, the hard data.” The devotion to instrumentally produced data—a “real world” constituted by a mathematical model calibrated and verified by inputs from observations in the natural world—aligns basic and applied researchers and creates their political position in society. Their politics is a defiant refusal to submit to the foregone conclusions of industry or government. One of the reasons that basic researchers look with suspicion instead of (or in addition to) envy upon the well-funded work of an applied researcher like Stephen Klaine—clearly evident in the labs packed with equipment surrounding his office, the swarms of graduate students, the invitations to speak around the world, the smiles of university administrators counting the overhead dollars generated by his research—may well be that the basic researchers, competing for tighter dollars, may view these advantages as the benefits of submission to the needs of extrascientific society. The politics of independence nevertheless persist as strongly among the applied researchers we interviewed as it does among the theoreticians.

From our perspective, the main differences between basic and applied research lie in the rhetorical approaches to their materials, the tendency to develop closed, tightfisted arguments in basic research as opposed to the tendency to produce open-ended arguments in applied research. This rhetorical difference amounts to a distinction of genre. Another such distinction emerges in scientists’ use of the jargon term “gray literature.”

**Gray Literature vs. Refereed Literature**

Gary Wein may well be one of the new breed of problem-solving biologists that Mark Hinman envisages. Though he is very much aware of the distinctions that separate basic science, applied science, and engineering, Wein has found himself crossing these boundaries at nearly every stage of his career. Trained as a basic researcher in biosystematics and later plant ecology, he went to work before he had finished graduate school as a consultant for a company called Princeton Aqua Science in southern New Jersey. He did surveys and wrote environmental impact statements involving wetland delineation, wetland evaluation, and wetland creation design. He had in effect become an environmental engineer. From this job, he went on to become the Research Manager for the Savannah River Project in South Carolina, serving as a liaison for research scientists in an ecology project associated with a nuclear reactor facility operated by the Department of Energy, helping researchers give bureaucrats “what they wanted” and at the same time helping the scientists to do “real research.” He also did more wetland design and some research of his own.

Several of the papers he produced during his tenure at the Savannah River Project fall into the category of “gray literature,” a term frequently employed in the shoptalk of scientists. Essentially it refers to papers written by scientists but not published in refereed journals and therefore not certified by the standardizing protocol of mainstream science, either basic or applied. Gray literature includes government documents, open-file reports, and in-house documents of various kinds. These documents may range widely in their conclusions from basic to applied science and on to engineering, though they are usually
devoted to solving definite problems in human life and are paid for by consulting fees or straight salaries rather than by funds from university budgets or research grants.

The following is an abstract from one of Gary Wein’s reports that he classifies as gray literature. It deals with wetland design and reconstruction. The paper has clear affinities with applied science, but it differs significantly from the applied studies we have seen that were published in refereed journals. Most scientific readers would see the work as an engineering or technical report:

The history of a large scale mitigation project of a cooling reservoir (L-Lake) for a reactor on the Savannah River Plant, South Carolina, is presented. The National Pollution Discharge Elimination System permit for thermal effluents discharged into the reservoir requires establishment of a balanced biological community (BBC). As a good faith effort toward establishment of a BBC, wetland/littoral vegetation was planted along 427 m (14,000 ft.) of shoreline in 1987. Approximately 100,000 plants were transplanted. Species planted were representative of submergent, floating-leaved, emergent and woody zones found in regional South Carolina lakes and reservoirs. The transplants have been growing well and reproducing, but project success will not be determined until spring and summer, 1988. (Wein, Kroeger, and Pierce 206)

Like papers in applied science, this one presents an argument that is open-ended, but for different reasons. The applied scientist leaves an argument open as a precaution of objectivity, as a protection against allowing the interests of an extrascientific perspective to intrude upon the data. Wein and his colleagues leave their argument open because the project is not yet finished; their paper is a “progress report” generated by the demands of the bureaucracy that is funding the work. The intended audience is clearly not a group of scientists, but rather funding managers and other government officials, whose lack of expertise is indicated by the need to have metric measurements translated for them. The very impetus to write is extrascientific; the purpose is to keep the money flowing. Moreover, the report is laced with political interests, claims of behaving in “good faith” and according to accepted laws, policies, and procedures.

And Wein is no stranger to environmental politics. Despite his background in science, his years of working in government and industry have led him away from the politics of apolitical research. In another paper about his work at the Savannah River Project, published in a book on wetland resources but still not subject to the kind of full review expected in mainstream science, he uses judgmental adjectives and a rhetoric of recommendation that is utterly foreign to academic science. Here is the abstract:

Three factors that can increase the complexity of the wetland mitigation process are 1) scale, 2) multiple regulatory agencies, and 3) conflicting goals and objectives. Scale includes the physical size of the project as well as the magnitude of alterations to ecosystem processes. The mitigation process involved in the restart of a nuclear production reactor is used to illustrate how these three factors could increase mitigation complexity. Two mitigation components of the reactor restart are presented. One component, the establishment of a balanced biological community in L-Lake, had large-scale impacts, nebulous goals and objectives, and involved several regulatory agencies. In contrast, the second component, replacement of lost foraging habitat for the endangered wood stork at Kathwood Lake, had small-scale impacts that could be managed, a singular, well-defined goal, and clear guidance from one regulatory agency. The L-Lake project has yet to succeed, while Kathwood Lake has been immediately successful. (Wein and McCort 41)

The rhetoric of this paper shares much with the rhetoric of basic research. In its review of two experiments, it clinches a tight argument about which of two kinds of project—the one large-scale and diffuse, the other small-scale and closely managed—is most likely to succeed. But the conclusion is oriented toward policy rather than toward theory and is therefore worlds away from basic scientific research. We asked Wein about the use and interpretation of scientifically generated information: When can we say that such data are no longer used “scientifically”? “When you use that information as a lever to get the DOE to change their actions,” he responded, you have moved out of the realm of mainstream science.

This response affirms what we found again and again in our interviews with academic scientists. If as in Walter Beale’s model of the discourse aims, the construction of scientific discourse as primarily
contemplative is constantly undermined by the intrusion of the rhetorical, instrumental, and poetic aims, we must nevertheless realize that scientists themselves distinguish their own rhetoric and their own instrumentalism from that of the world outside the scientific community. When science begins to influence the rhetoric and the instrumental realities of the public realm, it loses its special character and becomes something other than science as defined by scientific authorities. The author who makes recommendations for actions based upon scientific data or who insists upon the factual status of information still controversial in research circles is no longer doing science but is rather using science in a distinctly public rhetorical appeal.

Contrasts: The Discourses of Research, Pedagogy, and Public Action

We have now shown that scientifically certified discourse is different from discourses like scientific activism, which appeal to science without doing science, and that it is structured to resist the intrusions of general politics and human interests. The French philosopher Jean-François Lyotard, in his book *The Postmodern Condition*, usefully conceptualizes the difference between the esoteric discourse of science and the public version of that discourse. Since this analysis is consistent with our view of discourse communities, we might dwell for a moment upon it.

Narrative and Scientific Discourses

Lyotard begins with a distinction between what he calls narrative discourse and scientific or "denotative" discourse. Narrative discourse deals with ordinary human actions in a communal setting. Narrative is the standard means of gaining knowledge in folk communities. Like all discourse, it involves a speaker, a listener, and a subject matter, the latter being always a hero or an heroic action. The speaker's only claim upon the right to speak is that he or she is a member of the community and has something of interest to say. The speaker may have been the hero of the action ("I did so and so"), but not necessarily.

The speaker needs only to have heard the story ("I heard yesterday that . . ."). So after the listeners hear the story, they are certified to become speakers in turn. The participants in the discourse act—the speaker, the listeners, and usually the hero—are known to one another, are familiar with the conventions of narrative, and are empowered to participate in the discourse merely by their membership in the community.

Scientific discourse, on the other hand, is denotative rather than narrative, according to Lyotard. The functions of speaker, listener, and subject matter are pragmatically distinct. The subject matter is, at least theoretically, determinable equally by all participants, but the speaker (or writer) asserts a special claim on the truth and is able to furnish proofs of that truth. The listener is, in theory, the equal of the speaker but can become so in practice only by being able to provide the same kind of proof provided by the speaker. How are these proofs determined to be good proofs? They are established by the rules of the scientific community, by methods and theories approved in advance and agreed upon by a consensus of experts.

Scientific discourse is like narrative discourse in that it requires a communal context, a culture whose rules govern contributions to the discourse. The interchangeability of the speaker and listener are also similar, but here differences begin to emerge. Narrative creates its truth; denotative discourse establishes truth through method. Method requires mastery. A master speaker requires a master listener. To become either requires a teacher.

Thus science divides into a research function and a teaching function. Research is denotative and disputational, a discourse of proofs and challenges, formally arranged and carefully structured. Teaching, on the other hand, provides the occasion for the reemergence of narrative within science. Master researchers tell stories that gradually bring student researchers up to the level of full participation in the discourse community. To perform their narrative function, members of the community take some liberties with the truth. To learn the method, students must start with some statements that are "transmitted through teaching . . . in the guise of indisputable truths"—the facts (Lyotard 25). Students, the untrained listeners (as well as teachers not themselves a part of the research community—secondary school
teachers, for example), tend to grasp at the facts of science as a secure ground in the whirl of method and disputation. Only as they approach the level of the master researcher are these students able to treat the facts as black boxes that the effective application of method may eventually open again, producing controversy and perhaps even falsification. In general, research scientists and theoreticians consider simple narrative discourse with its solid facts to be contemptible, primitive, unsystematic, and underdeveloped (Lyotard 27). They must, however, tolerate a measure of this kind of discourse in order to reproduce their community of speaker-listeners of more or less equal status.

From the perspective of the research community, the trouble with writers like Aldo Leopold, Barry Commoner, and Rachel Carson is that they perform in public the function of scientific pedagogy, thus extending the function beyond the simple needs of the community to reproduce itself. Their narrative discourse—Leopold’s story of the philosophical blacksmith, Carson’s fable of the blighted town, Commoner’s explanation of the facts of entropy and the causes of Lake Erie’s near death—strike out into the political arena with stories for listeners without the special expertise needed to question the assertions of fact. A little knowledge is a dangerous thing. According to the conventions of narrative, these listeners may themselves become speakers in the extrascientific community, armed now with knowledge that bears the esoteric inscription of science: “I heard from a reliable scientist that...”

Metanarrative

Once released into the public at large, the pedagogical discourse of science comes under the influence of another form of discourse, which Lyotard calls metanarrative. Metanarrative is distinguished both from the plain narratives of folk communities and from the denotative discourse of science. Its purpose is hegemonic; it aims to bring other discourses under the control of its broad explanatory power and thereby to influence the use of such discourses in a community that includes smaller communities—such as the Roman Empire, the university as it has developed over the last one hundred years, or the global mass public created by modern communication and transportation technology.

Metanarrative assumes the general shape of the simple narrative but shares with science the concern with legitimation. The metanarrative of Enlightenment, for example, evolved in nineteenth-century Germany into an effort to bring the diverse elements of learning into the project of an all-encompassing knowledge, symbolized by the invention of the university. All areas of learning, the story goes, seek ultimately the same Truth, whose metanarrative is produced and certified by philosophy—hence the grouping of all specialized research under the degree called Doctor of Philosophy.

Likewise, the metanarrative of human liberation has dominated political ideology at least since Hegel’s time. This is the story of progress toward an ever broader human liberty—the freeing of slaves, followed by the emancipation of women, ethnic minorities, workers, and so on. But the range of the metanarrative is rarely limited to the political or social fields. Psychology, for example, has adapted this metanarrative to the story of a person’s “state of mind”; in waking life, the id is said to be encumbered by the ego while the ego itself is enslaved by the superego; but in the dream “state” or in neurotic “states,” the repressed forms of mental activity are liberated. The discourse that promises the greatest progress in the liberation of the world spirit is that which is the most legitimate in the metanarrative of human liberation.

The metanarrative that prevails in American political (and psychological) life is the ideology of progress (which includes aspects of both Enlightenment and human liberation). Ecospeak may be seen, for example, as an effort to take environmentalism—which is actually a broad social concern, a loose collection of little narratives—and reduce it to the status of a protest movement, a rear guard attack on developmentist progress, a regression into primitivism. Environmentalism may also be interpreted (in the manner of Hays) as an avant-garde movement demanding the extension of social progress, represented by “environmental amenities,” to ever greater numbers of people. Along these same lines, environmentalist progress may be aligned directly with the metanarrative of human liberation. For example, the eco-anarchist Murray Bookchin, following Lewis Mum-
ford, has argued that "the very notion of domination of nature by man stems from the very real domination of human by human" (Ecology of Freedom 1) and that "all ecological problems are social problems" (Remaking Society 24). In formulating the land ethic, Aldo Leopold attempted to extend the metanarrative of human liberation to include the liberation of nonhuman nature from the dominion of humanity. (He nevertheless used Hegelian logic and Hegelian language and therefore worked within the old metanarrative.) The deep ecologists, for their part, have followed Leopold in creating a new Hegelian doctrine of the "rights of nature" (see Manes). They have thereby interpreted anew the idea of progress, positing a planetary progress that may well stand in opposition to the kind of isolated human progress favored by reform environmentalists and developmentalists alike.

To think of political change outside of the metanarrative of progress is very difficult, but to do so, either by creating a new metanarrative (such as Habermas' concept of communicative action) or by abandoning metanarrative (as in Lyotard's postmodernism), may be the step that is needed to overcome the environmental dilemma. Science may be helpful in showing the way beyond. For science, as for many folk communities that perpetuate themselves through diverse narratives, progress is largely irrelevant. The main thing is to sustain the scientific research project or the folk community. If we could stop thinking of science as merely a data base for bolstering preformed arguments about environmentalist or developmentalist projects, and if we could consider the form of scientific discourse as a model of success, then new paths might open beyond the tangle of our intractable problems. That form consists of an informational system that is open to the extent that it allows for falsification, even whole paradigm shifts, but closed to the extent that it resists being appropriated by foreign metanarratives.

An environmentalist discourse has begun to develop along these lines in ecological economics, in which the paradigm of sustainability has evolved as a challenge to the story of economic growth that proceeds in an never-ending upward spiral. Sustainability resists this economic version of the metanarrative of progress and thereby promotes maintenance and endurance of the ecological systems on which human life depends. This project, which we will consider in chapter 7, involves a serious rethinking of the idea of progress and potentially breaks the hold of ecospeak.

But first, we must consider other efforts to bring scientifically generated information to bear on public understanding and environmental policy. In chapter 4, we turn from the denotative discourse of mainstream scientific research and from the narrative of science pedagogy, to science news, an attempt to create for a mass culture the equivalent of the culture-sustaining narratives in folk communities. Though it deals with the subject matter of science, this narrative differs from both refereed literature and gray literature, both of which require a writer who is ostensibly a member of the research community; it also differs from narratives constructed for the purpose of scientific pedagogy, which requires the pragmatics of a master addressing novices in a specialized knowledge. In science journalism, a writer, reporting to an audience of equals, tells the story of heroic actions with great import for the future of the community. This mythologizing of scientific learning empowers the listeners to become speakers themselves: "I read in Time magazine that the earth is warming up because of pollution..."