

- This is relevant to the "punctuated equilibria" controversy because Mayr, an ornithologist, appears to be the source of much of Gould's understanding of genetics. See Sewall Wright, "Genic and Organismic Selection," *Evolution*, 34, No. 5 (1980), pp. 825-43.
33. Stephen Jay Gould, "The Meaning of Punctuated Equilibrium and its Role in Validating a Hierarchical Approach to Macro-evolution," in *Perspectives on Evolution*, ed. R. Milkman (Sunderland, MA: Sinauer Associates Inc., 1982), pp. 83-104.
 34. Duane T. Gish, *Evolution: The Fossils Say No!* (San Diego: Creation-Life Publishers, 1979).
 35. Creationists turn the tables on evolutionists in other ways. A favored tactic is to set up strict criteria for what would constitute a legitimate theory, e.g. that a scientific theory must be falsifiable. See Gish, p. 13. The claim that evolutionary theory is not falsifiable is interesting, as one cannot readily imagine science rejecting the concept of evolution, although evolutionary scientists accept substantial revisions in Darwin's original thinking. To creationists, a pervading belief in evolution smacks of dogma. But this is because creationists fail to understand that the weight of rational and empirical support for evolution is distributed across many scientific specialties, and that *in principle* data can be imagined that could falsify the theory, even if such data never actually existed. Darwin frequently set up such possible scenarios for the destruction of his theory, including one criterion that, with a century of hindsight, appears to be false, the notion that evolution by natural selection requires phyletic gradualism. In 1859, when *On the Origin of Species* was published, "punctuated equilibria" might have been too much for the theory to bear. But "punctuated equilibria" are fully compatible with the much-revised "modern evolutionary synthesis." See Stebbins and Ayala.
 36. At a press conference during the 1980 election campaign, Ronald Reagan was asked if he thought that the theory of evolution should be taught in public schools. He responded, "Well, it is a theory, it is a scientific theory only, and it has in recent years been challenged in the world of science and is not yet believed in the scientific community to be as infallible as it once was believed." Asked if he believed in the theory of evolution, the presidential candidate replied: "I have a great many questions about it. I think that recent discoveries down through the years have pointed up great flaws in it." See "Republican Candidate Picks Fight with Darwin," *Science*, 12 Sept. 1980, p. 1214.
 37. For example, see James Gleick, "Stephen Gould: Breaking Tradition with Darwin," *New York Times Magazine*, 20 Nov. 1983, pp. 48 ff.; "Darwin is on the Run Again," *People*, 8 Dec. 1980, pp. 151 ff.; See, then, Stephen Jay Gould, "Darwinism and the Expansion of Evolutionary Theory," *Science*, 216, 23 April 1982, pp. 380-87.
 38. Gould, 1982 in *Science*, p. 381.
 39. Gould, 1982 in *Science*, p. 386.
 40. Gould, 1982 in *Science*, p. 386, n. 29.
 41. Vigorous debates about the role of chance and the force of selection on individuals, groups, populations, and species predate Gould's notion of hierarchies by at least 40 years. Sewall Wright summarizes circumstances under which chance might be important (see note 32). Evolutionary biologist George C. Williams put much of the controversy to rest with an influential book, *Adaptation and Natural Selection: A Critique of Some Current Evolutionary Thought* (Princeton University Press, 1966), in which he used intuitive arguments to demonstrate that the differential success of groups could be traced to the differential success of their members. Geneticist Richard Lewontin came to the same conclusion in 1970 with a more tightly reasoned argument in the lead article, "Units of Selection," of a prestigious new periodical, *Annual Review of Ecology and Systematics*, 1 (1970), 1-18. It is unlikely that biologists are skeptical of Gould's claim to a revolutionary insight about "hierarchical evolution" because the insight is new or revolutionary. More likely, most biologists probably see Gould as trying to sell a dead horse as a champion stud.
 42. See Roger Lawin, "Punctuated Equilibrium Is Now Old Hat," *Science*, 231 (1986), 672-73.
 43. We do not suggest that social and technical discourses are indistinguishable. We concur with Farrell and Goodnight (299-300) and others that the distinction can be made. Our point is that there are different ways to credit expertise. This is especially obvious when experts misread each other's technical dialects and fail to share key assumptions. What is expert in one scientific discipline may well be naive in another, even if to the laity it all carries the authority of being "scientific."
 44. See: Lyne, "Ways of Going Public."
 45. Paul N. Campbell, "The *Personae* of Scientific Discourse," *Quarterly Journal of Speech*, 61 (1975), 391-405.

The Rhetorical Construction of Scientific Ethos

Lawrence J. Prelli

Aristotle stressed that a central means of persuasion is a rhetor's perceived character or *ethos*.¹ To inspire confidence in claims advanced discursively, a rhetor must display the qualities of intelligence, moral character, and good will that are held in esteem by an intended audience.² Research on the sociology of science makes clear that scientific rhetors are also subject to the constraints of *ethos*.

The groundbreaking work on scientific *ethos* was undertaken by Robert K. Merton.³ Merton sought to identify the binding institutional norms that constrain the behavior of scientists, and facilitate establishing and extending certified, objective knowledge of the physical world.⁴ The norms of science, Merton believes, encourage behaviors that minimize distortion during systematic observation and maximize efficient dissemination of certified knowledge:

The ethos of science is that affectively toned complex of values and norms which is held to be binding on the man of science. The norms are expressed in the form of prescriptions, proscriptions, preferences, and permissions. They are legitimized in terms of institutional values. These imperatives, transmitted by precept and example and reinforced by sanctions are in varying degrees internalized by the scientist, thus fashioning his scientific conscience or, if one prefers the latter-day phrase, his superego. Although the ethos of science has not been codified, it can be inferred from the moral consensus of scientists as expressed in use and wont, in countless writings on the scientific spirit and in moral indignation directed toward contraventions of the ethos.⁵

The scientific *ethos* binds both technically and morally. It is technically binding because it prescribes efficient procedures for securing the extension of certified knowledge. It is morally binding because it is believed to assert what is right and good.⁶ The norms identified in Merton's original formulation are as follows: *Universalism* requires that knowledge claims be subjected to pre-established, impersonal criteria that render them consonant with observation and previously established knowledge. *Community* or *communism* prescribes that research is not a personal possession but must be made available to the commu-

nity of scientists. *Disinterestedness* requires that scientists strive to achieve their self-interests only through satisfaction in work done and prestige accrued through serving the interests of a scientific community.⁷ *Organized skepticism* mandates that scientists temporarily suspend judgment in order to scrutinize beliefs critically against empirical and logical criteria of judgment.⁸

Merton added to his original list the norms of *originality* and *humility* when he studied priority claims in scientific discovery.⁹ *Originality* is a source of esteem for one's work because through this quality scientific knowledge advances. *Humility* ensures that scientists will not misbehave at the rate that they would if importance were assigned only to originality and to establishing priority in scientific discovery.

Much research done in the wake of Merton's studies has supported his functional norms, but there has also emerged evidence that there exists in science a second set of "counter-norms" wholly incompatible with those identified initially by Merton.¹⁰ For example, in a well-documented study of NASA "moon scientists," Mitroff identified some of these counter-norms.¹¹ The list includes *particularism*, which counters universalism. Scientists often regard as legitimate judgments about research reports and proposals that are based on personal criteria, such as the ability and experience of authors, rather than on the strictly technical merits of the research claims themselves. Similarly, *solitariness* opposes communality. Scientists often do exercise property rights regarding their work. From this counter-normal vantage point, secrecy is appropriate conduct and, indeed, helps scientists avoid disruptive priority disputes and ensures that they do not waste colleagues' time by rushing immature work into print. *Interestedness* opposes disinterestedness. This counter-norm promotes conduct that serves scientists' special communities of interest (for example, their invisible colleges). Finally, *organized dogmatism* counters organized skepticism. This counter-norm prescribes that scientists do not incessantly doubt their own and others' findings, but instead assent fervently to their *own* findings while doubting the findings of others.¹²

Evidence of conflicting commitments among scientists has led some sociologists to question whether the constituents of Merton's scientific *ethos* should be considered "normative" at all. This question remains unsettled, but I do not intend to address it here.¹³ My contention is that when scientists resort to these common themes in discussing, justifying, or evaluating actions, the alleged "norms" and "counter-norms" of science serve a *rhetorical* function, regardless of whatever other functions they might be said to serve. Specifically, the constituents of scientific *ethos* function like rhetorical *topoi* for inducing favorable or unfavorable perceptions of scientific *ethos*. Scientific *ethos* is not given; it is constructed rhetorically. Rhetors respond to, or seek to avoid creating, ambiguities and conflicts about their scientific credibility. They do this by choosing from among a range of strategic options those that are best suited to situational contingencies. What sociologists of science have been calling "norms" and "counter-norms" are, in fact, rhetorical *topoi*.

index the available range of discursive strategies for establishing negative or positive audience perceptions of scientists' *ethos*.

First, we need to recognize that attention to the constituents of scientific *ethos* becomes salient only when the discourse of one scientist is made and evaluated by others in scientific situations that are rhetorical; that is, in problematic or ambiguous situations that involve inducing adherence to ideas presented as "scientific."¹⁴ Barnes and Dolby make the point that scientists stress the significance of such intellectual qualities as "rationality" or "skepticism" in situations that involve celebration, justification, or conflict.¹⁵ Within such situations successfully resolving problems or clarifying ambiguities hinges partly on the perceived qualifications of the proposing thinker or researcher. What scientific qualities will be most valued can vary from rhetorical situation to rhetorical situation.

Considerations such as secrecy, communality, objectivity, and emotional commitment may or may not be situationally relevant to evaluating a scientist's professional work. The situation in which scientific discourse is being evaluated determines the relevance and salience of each such quality. This is to say that when a scientist makes knowledge claims to colleagues and implies that these claims are "scientific" it *may* be relevant, though it will not always be imperative, to weigh the claimant's objectivity or emotional involvement, openness or secrecy, skepticism or enthusiasm, and the like. In various situations *either* of such opposed qualities *may* be relevant and be judged a *scientific* virtue or vice.

The professional *ethos* of a rhetor as a scientist becomes specially relevant when there is reason to believe that his or her primary aims are tied to such "nonscientific" pursuits as securing personal celebrity with lay audiences, achieving political or religious aspirations, or perpetuating beliefs that have occult or supernatural implications.¹⁶ Even those seeking explicitly to popularize science risk jeopardizing their *ethos* with expert audiences. They must find "common ground" with technically unskilled audiences, leaving themselves open to charges that they are pursuing objectives other than those that are properly "scientific" or "educational."¹⁷

In science as elsewhere, when the status of claims is problematic the *ethos* of the claimants can become pertinent. As Aristotle said, "We believe good men more fully and readily than others: this is true generally whatever the question is, and absolutely true where exact certainty is impossible and opinions are divided."¹⁸ Then, a claimant's "rationality" or "skepticism" can become important considerations for audiences charged with adjudicating technical claims.¹⁹ Similarly, one might dwell on a scientist's reputation, experience, and technical skill and on such grounds give problematical technical claims the benefit of the doubt.²⁰

Whatever is said or done to influence perceptions of a scientist's *ethos* will arise from a finite set of values implied by the notion of "doing good science".

verbally or implied by actions; they supply scientists with persuasive means for inducing adherence to their aims and claims as scientifically "reasonable" or "legitimate."

As soon as we recognize that we are dealing here with rhetorical *topoi*, the seeming conflict between alleged "norms" and "counter-norms" dissolves. The themes having to do with scientific *ethos* are not law-like or even "rules"; they are lines of thought that bear on a scientist's credibility in this or that rhetorical situation. Mulkay's description of how the constituents of scientific *ethos* actually come into play reflects their rhetorical topicality:

In science... we have a complex moral language which appears to focus on certain recurrent themes or issues; for instance, on procedures of communication, the place of rationality, the importance of impartiality, and of commitment, and so on. But... no particular solutions to the problems raised by these issues for participants are firmly institutionalized. Instead, the standardized verbal formulations to be found in the scientific community provide a repertoire or vocabulary which scientists can use flexibly to categorize professional actions differently in various social contexts.²¹

Topoi for building or diminishing a scientist's *ethos* are imprecise in the sense that any of the "recurrent themes" can be applied and developed in many ways. It is the abstract quality of *topoi* that makes them useful generative devices for yielding multiple specific applications. Most scientists will have some degree of commitment to such *topoi* as impartiality, objectivity, commitment, novelty, humility, and communality, and their opposites. Each scientist identifies with the *topoi* in the abstract; and then by moving in thought from the abstract to concrete applications, each can discover an array of specific arguments supportive of his or her overall position. Conflict often turns on whether one is for or against a specific *application* of a *topos*, say, humility, or whether one is for or against a *comparative ranking* of the *topoi* according to their alleged significance in a situation. For instance, there can be disagreement about whether novelty or skepticism is most important. Although the terminology of topical theory is not used, Mulkay's explanation of how "standardized verbal formulations" operate is in fact a discussion of how rhetorical *topoi* relating to scientific *ethos* are chosen:

A major influence upon scientists' choice of one verbal formulation [what I am now calling a "topic"] rather than another . . . is likely to be their interests or objectives. It can be assumed that, for a given scientist or group of scientists, these interests will vary from one social context to another. Thus . . . when researchers were frustrated by the apparent reluctance of others to make significant findings available to them, they tended to select principles favoring communality which justified their condemnation of the others' behavior and added weight to their exhor-

tations. In contrast, those scientists who had made the discovery were able to find principles in favor of personal ownership of results [secrecy]. In different circumstances, a person's or a group's choice of rules can be entirely reversed. Not only is it possible to vary one's choice of formulations as one attempts to identify the evaluative characteristics of different acts, but it is also possible to apply different formulations to the same act as one's social context changes.²²

One kind of rhetorical situation in which concerns about professional, scientific *ethos* become particularly salient is marked by questions about demarcation criteria. If it were possible to draw a sharp line of demarcation between science and nonscience, there would be little ambiguity involved when classifying discursive aims and claims as "scientific" or other; hence, there would also not be any need for rhetoric to clarify the scientific standing of those aims and claims. However, whenever we seek to differentiate "science" from "nonscience" there will always be working ambiguities. In these rhetorical situations, scientists will likely choose rhetorical strategies that help them construct "boundaries" that are favorable to their own professional goals and interests and unfavorable to their competitors. For instance, researchers might make rhetorical appeals that construct "narrow" or "rigid" boundaries, inducing adjudicating audiences to distinguish their "scientific" work from the research of those they allege are unorthodox or unscientific "outsiders." In Gieryn's view, scientists engage in "boundary-work" not for the lofty epistemological reasons philosophers often cite (for example, preservation of scientific truth), but as a rhetorical means of solving practical problems that can block achievement of professional goals.²³ Scientists draw sharp contrasts between themselves and "nonscientists" to enhance their intellectual status and authority vis-a-vis the "out groups," to secure professional resources and career opportunities, to deny these resources and opportunities to "pseudo-scientists," and to insulate scientific research from political interference. We should notice, too, that researchers with "unorthodox" aims and claims will also compose rhetoric about scientific *ethos*; but they will typically seek to "broaden" or "soften" the "boundaries" of science as they are defined by defenders of scientific orthodoxy, attempting to show that they, too, are scientists and that their claims should also be taken seriously as reasonable scientific contributions.

Audiences called upon to adjudicate "boundary disputes" are often comprised of scientifically illiterate laypersons, who nevertheless have great respect for the authority of science and its practitioners.²⁴ When "experts" disagree about matters of public policy, the laity usually cannot decide issues on technical grounds. Rhetors will therefore seek to settle demarcation exigencies by constructing "boundaries" in view of some ideal, public image of science and its practitioners. Both orthodox and unorthodox scientists will choose from among the special *topoi* of scientific *ethos* to construct public images favorable to their respective interests and objectives.

The Topoi of Scientific Ethos: A Case Study

What follows is a case study of *topoi* used to attack and defend scientific *ethos*. The case concerns Francine Patterson's and Eugene Linden's *The Education of Koko*, and Thomas A Sebeok's review of that book.²⁵ In the book, Patterson explains how she taught Koko, a gorilla, to use American Sign Language. Most scientists would agree that Koko makes some kind of gestures but many also conclude that Patterson has gone far beyond her data when she claims that the animal has learned its lessons so well that it can ask questions, lie, insult, joke, apologize, and even express grief.²⁶ Sebeok, Professor of Anthropology, Linguistics, and Semantics at Indiana University, is one of the most outspoken critics of language acquisition studies of apes in general and of Patterson's research in particular. In his review, "The not so sedulous ape," Sebeok attacks Patterson's scientific *ethos* on the grounds that she lacks technical and communal qualities that should be displayed by "credible" scientists. Specifically, Sebeok implies that Patterson and her co-author are not "real" scientists; they are scientific "outsiders" because they display qualities of thought and behavior that are, at best, in conflict with the Mertonian "virtues" of scientific *ethos*: *universality, communality, skepticism, and disinterestedness*.²⁷

Sebeok suggested that Patterson is not a "real" scientist because her technical claim-making is, in a word, "bizarre"; he repeatedly illustrated how Patterson's claims for Koko's linguistic cleverness conflict with an implied universal consensus about what is already known about the language behavior of apes. He referred approvingly to Herbert S. Terrace's conclusion, in his book *Nim*, "that there is no evidence at all that apes can either generate or interpret sentences." Sebeok buttressed his claim by arguing that Terrace's "hardly surprising" conclusion, in direct contrast with Patterson's claims, is consistent with what has been revealed by "informed" linguists, and "responsible" ethologists: "Terrace's results are . . . in perfect conformity with the long held judgment of informed linguists from Max Muller (1889) to Noam Chomsky. They accord equally well with the view of responsible ethologists, such as Konrad Lorenz, who declared, in 1978, 'that syntactic language is based on a phylogenetic program evolved exclusively by humans,' and that anthropoid apes ' . . . give no indication of possessing syntactic language.'"²⁸ The conclusion that Sebeok would have us draw from this passage is clear. Patterson's research claims are not grounded in and consistent with established research conclusions in respectable fields of scientific inquiry. Accordingly, as a scientist, she can only be held in contempt as "uninformed" and "irresponsible."

Sebeok also criticized Patterson's research claims on the ground that she is an incompetent experimenter. Specifically, he charged that Patterson had not accounted for possibilities that the animal was behaving in response to clues given by experimenters, an experimental problem popularly known as the "Clever Hans" phenomenon. According to Sebeok, this fallacy is one "by which Koko's entire ten-year curriculum has been errantly nag-ridden. . . ." To support this

The eminent Bristol neuropsychologist, Richard Gregory, also concluded, in 1981, that apes do not exhibit either "human language or intellectual ability", and wisely admonished: "There are so many experimental difficulties and possibilities of the animals picking up clues from the experimenters, given unwittingly, that extreme caution is essential."

Sebeok repeatedly assailed Patterson's research with arguments that questioned the reliability of both her data and her interpretations of data. For instance, Patterson's competence became an issue when Sebeok challenged the interpretive claim that Koko was able to tell lies:

Much is made of her aptitude for lying, which, according to the authors, "of course, is one of those behaviors that shows the power of language." Here, however, lurks a terminological confusion, one that, furthermore, begs the question. Many kinds of animals—the most remarkable case on record is that of the Arctic fox, *Alopex lagopus*—give, or give off, deceptive messages, in a word, prevaricate. But a lie must, by definition, be "stated," which Koko simply cannot do.²⁹

From this and other instances, Sebeok would have us conclude not only that Patterson's data are suspect but also that her ability to render "scientific" judgments is highly questionable. The claims she advances cannot be accepted unless one replaces "pre-established impersonal criteria" of science with Patterson's idiosyncratic standards of judgment. In a word, the qualities of Patterson's thought and conduct lack affinity with the scientific virtue of "universality."³⁰

Sebeok continued his attack on Patterson's *ethos* by drawing arguments from the powerful *topos* of *organized skepticism*. For example, Sebeok amplified both Patterson's and Linden's proclivity to minimize the importance of emotional detachment and systematic doubt in their research by associating them with proponents of a field whose scientific status has been thought questionable. They are said to be "addicted to the use of ploys familiar from parapsychology, such as that the presence of a skeptic tends to ruin experimental results." Among these "ploys" is their emphasis on emotional rapport between experimenters and animal subjects as a precondition for making successful experimental results. Sebeok quotes directly the authors' belief "that one cannot really understand the mental workings of other animals or bring them to the limits of their abilities unless one first has true rapport with them." He then asserted that the "obverse of this claim is that the intimacy between Patterson and her beloved Koko had hopelessly overclouded her scientific objectivity and judgment. . . ." In this way, Sebeok would lead us to conclude that Patterson's research claims must be technically unacceptable because her conduct resonates with an obstinate, dogmatic will to believe that Koko makes and uses language.

A scientist's professional *ethos* depends not only on how audiences perceive her competence, but also on how that scientist's "place" within a legitimat-

ing scientific community is viewed. To the extent that a rhetor's connection with his or her scientific community becomes confusing, so too does the legitimacy of his or her aims and claims. I have so far shown that Sebeok sought to discredit Patterson's and Linden's *ethos* by arguing that they lack the scientific "virtue" needed to make credible, technical claims. He also repeatedly challenged the authors' credibility by making their relationship with a professional scientific community seem ambiguous to the reader. Sebeok did this by using three lines of argument based on the *communality topos*. First, Patterson and her co-author do not have memberships in any legitimate research institution, so neither is a "real" scientist. Patterson had written that she was not able to analyze Koko's spoken language in detail despite an "enormous" amount of data collected. Sebeok offered this "translation": "In plain text, this citation means that since Miss Patterson's connections with Stanford University has been severed, she no longer enjoys free access to its computers." In other words, Patterson has no scientific standing because she has lost the resources of a legitimizing institution. Sebeok attacks the scientific *ethos* of Patterson's co-author, Eugene Linden, with even less subtlety. Linden is depicted as "a wrestler-turned-journalist, perhaps best known to the public for his *Apes, Men, and Language* (1974, 1981), surely the most gullible, as well as defensively emotion-laden, popular account of attempts at linguistic communication with any of our collateral ancestral species so far published...." The implication here is that Linden lacks connections with legitimizing research institutions; he is, at best, a popularizer of science—and not a very credible one at that.

In addition to arguing that Patterson and her co-author do not "belong" to the scientific community by virtue of position, Sebeok "reads them out of" the community of scientists on grounds that Patterson has been unable to secure ample public funds to support her research. Sebeok was explicit: "While millions of dollars in federal funds were being squandered on the futile search for language in chimpanzees and orangutans, Patterson continued her work, without a proper institutional base, with the support of private sources, including a large, so-called 'non-profit' commercial enterprise,³¹ supplementing her income by minor grants from small Foundations." He further asserts that the lack of public subsidy is "one respect" in which Patterson's Project Koko differs "sharply" from other studies of ape language capacities. The reader is presumably to understand that any minimally competent student of language in primates would have had access to and used the all-too-available resources for doing the job "right," scientifically.

A third line of argument involves questioning Patterson's legitimacy as a scientist because she has failed to submit her claims for authorization by competent scientists.³² She had success reaching popular audiences through such channels as *National Geographic* magazine, *Reader's Digest* and a documentary film,³³ but her publishing in more technical sources has been minimal, giving Sebeok warrant for asserting, "If Penny Patterson tried to publish in a scientific atmosphere, then she would be laughed out of court."³⁴ She was attacked as having a "warped perspective" and a "lack of receptivity to well-intentioned

criticism."³⁵ Accordingly, the reader is left to conclude that Patterson is an "outsider" who lacks credible standing within the professional scientific community.³⁶

Sebeok also challenges Patterson's and Linden's scientific *ethos* with remarks that imply they are not sufficiently *disinterested* to be pursuing "legitimate" scientific objectives with the publication of their book. "Real" scientists identify their self-interests with winning community recognition for "making contributions to the development of the conceptual schemes which are of the essence in science."³⁷ In contrast, Sebeok makes innuendo about Linden's "real" interests: "Her co-author's stake in this enterprise—as well as, of course, his bond of personal relationship with the gorilla—is clearly of a different order." The implication one can draw from this assertion is that Linden's primary motivations are not scientific, but are tied to making money. Sebeok asserts that Patterson's motives for conducting research amount to nothing more than a "desperate reaching out for media recognition (of which this unfortunate book represents but one example)."

My initial focus in this study has been on examining topical choices that Sebeok made when evaluating Patterson's and Linden's scientific *ethos*. At this point, I want to show how the two authors rhetorically constructed a "revolutionary" public image for science and scientists that sharply contrasts with the public image prescribed by Mertonian "normal" science. Patterson argued that objections to claims that nonhuman primates have language abilities are symptomatic of a deeper, intellectual revolution within the behavioral sciences concerned with language. After R. Allen Gardner and Beatrice T. Gardner published findings that the chimpanzee Washoe was able to use language, *Science* published rebuttals written, according to Patterson, by "the most distinguished names in the behavioral sciences." In the face of vigorous resistance by eminent scientists, Patterson claimed that the Gardners' "success" with Washoe presented "one of the most basic tenets of modern life" with an "anomaly;" there was now evidence that humans are not unique in their possession of language. She contended that the hostile reaction to these anomalous claims indicated that something like Kuhn's notion of scientific revolution was taking place in studies of language acquisition in primates.³⁸ Once readers embrace this revolutionary scenario, they are easily led to conclude that "credible" scientists can and do display such qualities as *individuality, particularity, dogmatism, and interestedness*.

Once readers agree that the Gardners have hurled fields like linguistics into "crisis" there is no warrant for esteeming *communality* as a special, scientific virtue; there is no legitimizing community consensus during periods of "crisis." Judgments about "legitimate" research institutions, "appropriate" sources of research funding, and "qualified" adjudicating audiences become partisan points of contention during "revolutionary" science. This image of non-human primate research allows Patterson to extol qualities of *individuality* as characterizing credible scientific *ethos*.³⁹ We are to conclude that Patterson, by following the Gardners, displays intellectual courage by making bold judgments to abandon

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outmoded but comfortable notions about language, and by pursuing a pioneer line of thinking even when confronted with eminent hostile opinion. Thus, the conflict of her position with conventional knowledge is raised to the heights of scientific virtue.

Throughout the book, *universality* is minimized as an important quality of scientific thought and conduct. In revolutionary contexts, scientists cannot turn to "pre-established impersonal criteria" to settle controversy. In the case of linguistic science, they cannot rely on previously certified knowledge because, Patterson says, "there is very little that can be said about language today that is not open to question or controversy."⁴⁰ Nor can scientists settle disputes by making carefully constrained observations and using empirically rigorous experimental designs. Patterson contends: "There is much about language that does not lend itself to reduction to statistics and hard data, and some linguists have recently reacted against the rigid, formalized treatment of language. . . . This is not to justify vagueness but to illustrate that it is very difficult to speak with any confidence of 'facts' about language."⁴¹ What scientists must do, says Patterson, is supplement the "rigid" experimental work with "interpretive" case studies.

Case studies require special interpretive sensibilities that allow researchers to achieve what I shall call, for lack of a more fitting expression, *experimental rapport* with subject beasts. The line of thought, based upon the *topos* of *particularity*, is that adequate scientific investigations of the behaviors of sentient beings require that researchers possess special abilities for developing emotional attachments with animal subjects. Patterson uses this line of thought to amplify her scientific credibility and minimize the scientific credibility of her critics. Only those scientists able to establish true rapport with the animal whose behavior is being studied will meet a necessary condition for gleaning positive evidence of language acquisition in non-human primates; those who retain rigidly objective experimental stances toward the animal will confound possibilities for discovering significant language use by the animal. Accordingly, the "best" research with primates encouraged development of rapport between scientist and animal:

In none of these cases did the experimenter [Terrace and Premack] allow himself to develop a true, close rapport with his chimp. This was justified in the laudable name of objectivity, but given the sensitivity of the animals involved—Koko's signing is affected by even slight disruptions in her routine—it is hard not to wonder whether the different conclusions about ape language abilities reached by these scientists ultimately trace back to the different relationships between experimenters and subjects and to the persistence that has marked the efforts of those of us who have established close rapport with our subjects. If this is the case, I am reaffirmed in my belief that one cannot really understand the mental workings of other animals or bring them to the limits of their abilities unless one first has true rapport with them. Even the critics admit this possibility. What they fail to see is that the problem really is a misunder-

standing of the purpose of language. Once that misunderstanding is straightened out and we accept language as a communicative behavior, the evidence of Koko's abilities is compelling for those who want to see it.⁴²

Implicit in Patterson's discussion is a view of researchers as fundamentally *dogmatic*. Scientists cannot skeptically avoid dogmatic adherence to assumptions, so the best that they can do is choose and adhere to assumptions that allow them to comprehend the full range of non-human primate linguistic ability. The choices are clear for Patterson: researchers either assume that Koko is a "dolt" and dismiss all apparently innovative and intentional linguistic acts as "mistakes," or they recognize that the animal is "a bright, playful, creative creature capable of quite sophisticated innovation."⁴³ "Rigid" experimentalists take the former stance, while those who supplement formal testing procedures with anecdotal, case-study material choose the latter, Patterson asserts. For Patterson, only the second alternative can yield persuasive evidence for Koko's linguistic creativity.

In Patterson's view of scientific *ethos*, credible scientists must be intensely *interested* in the consequences that their research claims have beyond the knowledge-oriented interests of scientific communities. The interested scientist addresses research claims about nonhuman primates' language abilities beyond scientific communities to expose and to falsify religious and philosophical systems that make humans the uniquely "linguaging" animal. One consequence of exposing this "false" belief is an ethical challenge to humans' "rights to experiment with or harvest natural resources. . . ." ⁴⁴ In sum, "virtuous" scientists are interested in the religious, philosophical and ethical implications of technical claims for society at large. They are not focused narrowly and solely on the knowledge-oriented concerns of their specialized communities.⁴⁵

When Patterson's discourse is contrasted with Sebeok's we can see how topical selection and development can influence how situated, lay audiences will judge an individual's or a group's scientific ethos. Sebeok constructed a Mertonian image for science, and on the basis of that image implied that Patterson and Linden were not "real" scientists because they lacked the scientific virtues of communal-ity, universality, skepticism and disinterestedness. Patterson and Linden constructed a revolutionary image for science, and worked from that image to show that Patterson's thought and conduct displayed the esteemed qualities of individuality, particularity, dogmatism, and interestedness and, moreover, that her critics, Sebeok included, lacked those "virtues." Both Sebeok and Patterson and Linden strategically selected *topoi* in order to construct a scientific ethos that was favorable to securing and justifying their respective professional interests and objectives, given the constraining influences of their particular rhetorical situation.

Topoi and Rhetorical Boundaries

The case study illustrates how scientific "insiders" and "outsiders" can rhetorically construct conflicting perspectives on scientific *ethos* when responding discursively to demarcation problems. Sebeok composed a review that invoked

sharp "boundaries" between science and non-science, implying that Patterson and Linden were not "real" scientists because their thought and conduct did not adhere to the "virtues" of universality, communality, skepticism, and disinterestedness. Readers are urged to dismiss Patterson and Linden as incompetent or pseudo-scientific outsiders on the basis of these "scientific" qualities and, consequently, to conclude that *The Education of Koko* must be a scientific ruse. Patterson and Linden's book created a perspective on scientific *ethos* that blurred the boundaries between science and nonscience, suggesting that credible scientists adhere in thought and conduct to virtues of particularity, individuality, enthusiasm or dogmatism, and interestedness—qualities of character that do not sharply differentiate scientists from nonscientists. I surmise that this perspective on scientific *ethos* is often created when those who view themselves as revolutionary outsiders want to challenge and to overturn the claims of orthodoxy, but more evidence is needed to substantiate this conjecture. Nevertheless, in this instance both the rhetoric of insiders and outsiders was addressed to the laity which was left with the task of deciding what qualities credible scientists do and do not have.

Sebeok and Patterson and Linden put forward contrasting public images of credible science and scientists. Sebeok depicts science as a unique intellectual activity characterized by a high degree of consensus and agreement about what "counts" as science. "Normal" scientists—those possessing a credible scientific *ethos*—will adhere to Mertonian standards of thought and conduct. Only pseudo-scientists or eccentrics working at the lunatic fringe will neglect those standards. Patterson and Linden portray science as characterized by dissensus and disagreement. "Revolutionary" rather than "normal" scientists possess qualities that are virtuous given that image. In revolutionary circumstances, Mertonian vices are shaded into virtues. Both public portrayals are *idealizations* of science and scientists. Specifically, linguistic science is neither as "normal" as Sebeok implies through his review, nor is it as "revolutionary" as Patterson and Linden would have their readers believe. These idealized visions of science and scientists can be explained only partly by the rhetors' need to address a scientific laity that is incapable of following the relevant technical arguments. I say "partly" because even scholarly commentators cannot agree about whether the actual scientific endeavor is best characterized by consensus or dissensus.⁴⁶

The best explanation is that neither idealized vision can alone prescribe situationally transcendent and uniformly applicable standards constituting the scientific *ethos*. Science is both consensual and divisive, cooperative and competitive; and scientists can possess both "normal" and "radical" qualities. The alternative public "faces" of science underscore the point that scientific *ethos* is a *rhetorical* construction. Contrasting images of scientific *ethos* show that either of the opposed qualities of universality or particularity, communality or individuality, skepticism or enthusiasm, and disinterestedness or interestedness can be shaded into a scientific virtue or vice. Rhetors often include among compositional decisions those concerning which strategies are most useful for constructing a

scientific *ethos* that can best advance their professional interests, given the problems they must face and the constraints of their audience and situation. It may be that the demarcation exigence and the standard kinds of rhetorical strategies that "insiders" and "outsiders" use in response are generalizable. For instance, demarcation exigences emerge when mainstream scientists argue that parapsychology or "scientific" creationism are not "real" sciences and that their practitioners are not "real" scientists. Analysis of these disputes shows that critics of parapsychology and creationism turn to the kinds of *topoi* Sebeok used to construct perspectives on scientific *ethos* in their rhetorical situations.⁴⁷ This suggests that the *topoi* identified in this study are among the standard and finite sets of themes that scientists choose from when composing discourse in response to demarcation exigences.

When scientists address ambiguities about what it means to think and to act like scientists, they will turn to those kinds of arguments catalogued by the *topoi* related to scientific *ethos*. However, scientists also respond to technical kinds of exigences, and topical analysis of discourse can reveal the kinds of arguments scientists use when trying to resolve those exigences as well. For instance, scientists encounter ambiguities about the scientific significance of evidence or of the constructs that they use to articulate and apply theory. Ambiguities about the intrinsic or comparative scientific value of evidence and theory constitute a special *kind* of technical exigence which demands that scientists choose the right *kind* of *topoi* to make an effective rhetorical response. What some commentators call "good reasons" for making scientific judgments might actually function like rhetorical *topoi* that are specially useful for making arguments in response to problems of scientific "value" or "significance." A preliminary list would include the scientific "values" of accuracy, consistency, scope, simplicity, and fruitfulness.⁴⁸ Future case studies can with profit focus on revealing the kinds of *topoi* that scientists use when responding to this and other kinds of situational exigences.⁴⁹ This might lead to the compilation of topical inventories, organized according to the kinds of situational exigences that scientists confront. As these lists are amended and refined, rhetorical critics can use them heuristically to distinguish the kinds of topical choices rhetors *could* have made from those that they *did* make, and to assess the comparative persuasive efficacy of the rhetors' choices given the *kind* of situational exigence they were seeking to resolve.

The "rhetoric of inquiry" is itself a rhetorical effort at heralding a demarcation crisis in the human sciences; it seeks to indict the present "boundaries" among substantive fields of inquiry as largely artificial and points to the need to redraw or traverse those boundaries in fruitful ways. Scientists must confront concerns about professional *ethos* when addressing demarcation exigences; so, too, must scholars conducting research on the "rhetoric of inquiry." However, before we go too far in our rhetorical "boundary-work" we must first clarify what the present boundaries are. Does each field of inquiry have its own technical expectations and substantive concerns which prescribe for its practitioners special "virtues" of professional thought and conduct? What does it mean to think and to act like,

say, a poet, sociologist, physicist? Do the "habits" that artists and scientists display show that the "two cultures" are converging or diverging? This analysis provides the field-independent methodological approach that allows us to cut across the currently fragmented areas of substantive inquiry and provide answers to these and other questions. By answering such questions, we can become more self-reflective about what it means to be a member of a particular academic community and discern better whether the "boundaries" we invoke to demarcate ourselves from members of other communities are desirable for intellectual or practical purposes, or are merely artificial.

Notes

1. Aristotle, *Rhetoric*, tr. W. Rhys Roberts (Random House, New York, 1954), 1356a5.
2. *Ibid.*, 1378a5-19.
3. Robert K. Merton first published his views on the scientific *ethos* in his essay "Science and the Social Order," *Philosophy of Science*, 5 (1938), 321-37. A more systematic rendering of his ideas appeared in the essay, "Science and Technology in a Democratic Order," *Journal of Legal and Political Sociology*, 1 (1942), pp. 115-26. I am using the reprint of this essay, "The Normative Structure of Science," in Robert K. Merton, *The Sociology of Science: Theoretical and Empirical Investigations*, ed. Norman W. Storer (Chicago: University of Chicago Press, 1973), pp. 267-78. Unless otherwise indicated, all references to Merton's articles will be to this useful compilation.
4. Merton, "Normative Structure of Science," p. 270. In Merton's view, extending certified knowledge is the institutional goal of science.
5. *Ibid.*, pp. 268-69. Merton's scientific *ethos* is the sociological complement to the epistemological position known as the "standard view." For a critique of this philosophical position which grounds Merton's sociology of science see Michael Mulkay, *Science and the Sociology of Knowledge* (London: Allen, 1979).
6. Merton, "Normative structure of science," p. 270.
7. I am relying on Barber's interpretation of disinterestedness. Storer amplifies this idea by explaining that it encourages pursuit of "science for science's sake." See Bernard Barber, *Science and the Social Order* (1952; reprinted Greenwood, Westport, 1978), p. 92; and Norman W. Storer, *The Social System of Science* (New York: Holt, 1966), p. 79.
8. For Merton's discussion of the norms see "Normative Structure of Science," p. 270-78. Disinterestedness, universalism, communalism and skepticism imply that *emotional neutrality* toward ideas and actions is a scientific virtue. Although Merton did not raise objectivity to normative status, other commentators have discussed emotional neutrality as a scientific value. See Barber, *Science and the Social Order*, pp. 88-9; and Storer, *Social System of Science*, pp. 79-80.
9. Merton, "Priorities in scientific discovery," in *Sociology of Science*, especially pp. 239-305.
10. For examples of research strongly supportive of Merton's normative structure of science see John R. Cole and Stephen Cole, *Social Stratification in Science* (Chicago: University of Chicago Press, 1973); Jonathan R. Cole, *Fair Science: Women in the Scientific Community* (New York: Free Press, 1979); Jerry Gaston, *The Reward System in British and American Science* (New York: Wiley, 1978); Barber, *Science and the Social Order*; and Storer, *Social System of Science*.
11. See Ian I. Mitroff, *The Subjective Side of Science: A Philosophical Inquiry into the Psychology of the Apollo Moon Scientists* (Amsterdam: Elsevier, 1974).
12. Particularism, solitariness, interestedness, and dogmatism each imply that *emotional commitment* to one's ideas and actions is a necessary ingredient of science. Some commentators have gone so far as to call emotional commitment the counter-norm to emotional neutrality. See Mitroff, *Subjective Side of Science*, p. 276. For lists containing these and other norms and counter-norms see *ibid.*, p. 79; and Ian I. Mitroff and Richard O. Mason, *Creating a Dialectical Social Science: Concepts, Methods, and Models* (Dordrecht: Reidel, 1981), pp. 147-8.
13. Stehr provides a useful overview of the central issues involved. Barnes and Dolby and Mulkay are among Merton's most vociferous critics. Gaston and Zuckerman issue strong defenses. See Nico Stehr, "The Ethos of Science Revisited: Social and Cognitive Norms," *Sociological Inquiry*, 48 (1978), pp. 172-96; S.B. Barnes and R.G.A. Dolby, "The Scientific Ethos: A Deviant Viewpoint," *Archives Européennes de*

- Sociologie*, 11 (1970), pp. 3-25; Mulkay, *Science and the Sociology of Knowledge*, especially pp. 3-12; Gaston, *The Reward System*, pp. 158-84; and Harriet Zuckerman, "Deviant Behavior and Social Control in Science," in Edward Sagarin (ed.), *Deviance and Social Change* (Beverly Hills: Sage, 1977), pp. 123-8. I am following Lloyd F. Bitzer's idea that rhetorical situations are characterized by exigences or ambiguities that can be solved, clarified, or modified through discourse. See "The Rhetorical Situation," *Philosophy and Rhetoric*, 1 (1968), pp. 6-7.
- Barnes and Dolby, "The scientific ethos," p. 13.
- As examples, consider some standard attacks on the scientific *ethos* of creationists and parapsychologists. See Thomas I. Gieryn, George M. Bevens, and Stephen C. Zehr, "Professionalization of American Scientists: Public Science in the Creation/Evolution Trials," *American Sociological Review*, 50 (1985), especially pp. 399-405; and H.M. Collins and T.J. Pinch, "The Construction of the Paranormal: Nothing Unscientific is Happening," in Roy Wallis (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge*, Sociological Review Monographs 27 (University of Keele, Staffs, UK, 1979), especially pp. 246-7.
17. On this point, see Thomas M. Lessl, "Science and the Sacred Cosmos: The Ideological Rhetoric of Carl Sagan," *Quarterly Journal of Speech*, 71 (1985), p.176.
18. Aristotle, *Rhetoric*, 1356a6-8.
19. When there is ambiguity or cause for doubt about technical claims, qualities exhibited through presentations and prior reputation are factors for doubt about technical claims, qualities exhibited through presentations some examples. A leading advocate of the molecular memory transfer hypothesis exhibited qualities through presentations that some thought were less than "scientific." The scientist conducted scientific work without being sufficiently "earnest." Another scientist, involved in the discovery of the solar neutrino anomaly, drew upon his established reputation for being careful, modest, and open with results as an important persuasive resource for establishing the anomalous claim. See David Travis, "On the Construction of Creativity: The 'Memory Transfer' Phenomenon and the Importance of Being Earnest," in Karin D. Knorr, Roger Krohn, and Richard Whitley (eds), *The Social Process of Scientific Investigation, Sociology of the Sciences Yearbook 4* (Dordrecht: Reidel, 1981), pp. 177-8; and T.J. Pinch, "Theoreticians and the Production of Experimental Anomaly: The Case of Solar Neutrinos," in *ibid.*, pp. 94-5.
20. According to Zuckerman, she and Merton found evidence for this point when they examined referees' reasons for recommending or rejecting submissions to the physics journal, *Physical Review*. Specifically, referees were more likely to endorse unorthodox ideas when authored by established scientists than young or rank-and-file scientists. Zuckerman interprets this as evidence for the influence of "performance-based authority" on reviewers' judgments. Unfortunately, this idea was not developed in Zuckerman's and Merton's article on the subject. See Harriet Zuckerman, "Theory Choice and Problem Choice in Science," *Sociological Inquiry*, 48 (1978), p. 70; and Robert K. Merton, "Institutionalized Patterns of Evaluation in Science," in Merton, *Sociology of Science*, especially pp. 476-91.
21. Mulkay, *Science and the Sociology of Knowledge*, p. 71.
22. *Ibid.*, pp. 71-2.
23. Thomas E. Gieryn, "Boundary-work and the Demarcation of Science from Nonscience: Strains and Interests in Professional Ideologies of Scientists," *American Sociological Review*, 48 (1983), pp. 781-95.
24. Gieryn affirms Mulkay's claim that what I am calling the special *topoi* of scientific *ethos* become especially useful when scientists address professional rhetoric to lay audiences. See *ibid.*, p. 783; Gieryn et al., "Professionalization of American scientists," especially pp. 403-4; and Michael Mulkay, "Norms and Ideology in Science," *Social Science Information*, 15 (1976), p. 646.
25. Francine Patterson and Eugene Linden, *The Education of Koko* (New York: Holt, 1981); Thomas A. Sebeok, "The Not So Sedulous Ape: Review of *The Education of Koko* by Francine Patterson and Eugene Linden," *Times Literary Supplement*, 10 September 1982, p. 976. Unless otherwise indicated, all references to Sebeok's rhetoric are to this review.
26. For instance, see Laura A. Petitto and Mark S. Seidenberg, "On the Evidence for Linguistic Abilities in Signing Apes," *Brain and Language*, 8 (1979), pp. 162-83. Also see the critical review of the techniques and conclusions of language experiments with nonhuman primates, including Patterson's, in Mark S. Seidenberg and Laura A. Petitto, "Signing Behavior in Apes: a Critical Review," *Cognition*, 7 (1979), pp. 177-215.
27. Most of Sebeok's review is directed toward discrediting Patterson's *ethos*, with less attention devoted to Linden's. This is appropriate given that the book is based on Patterson's research, which is presented to the reader in Patterson's voice.

28. Sebeok's appeal to Terrace's study has added rhetorical force because Terrace had initially believed the chimpanzee, "Nim Chimpsky," was capable of creating a sentence. Terrace admitted the "fantasies" about what he could accomplish with his communicating chimp, including using the chimp as a translator of ape communication in the wilds; but after further analysis of his data he concluded that Nim said little on his own was merely imitating behavior in an effort to get rewards—Nim was not constructing sentences. When those holding "heretical" opinions publicly reform their ideas they are often example provides conservative defenders of orthodoxy with powerful means of persuasion; thus, the reformation casts doubt on the legitimacy of heretical beliefs and renders ambiguous the authenticity of those who hold them. Sebeok used the reformed Terrace's "true confessions" as persuasive means of questioning the legitimacy of Patterson's claims and the scientific authenticity of her motives. The film explained his changed views in *Signs of the Apes, Songs of the Whales* (film, produced by Linda Healy, Nova, WGBH-Boston, PBS, 1983).
29. Sebeok sought to discredit Patterson's scientific *ethos* by asserting throughout his review that his interpretations of Koko's behavior are riddled with "anthropomorphic" (read: "unscientific") tendencies. After all, gorillas do not willfully make false statements; only humans can lie. However, Sebeok was inducing readers to make a sharp dichotomy between "anthropomorphic" and "scientific" interpretations that is not always so easily drawn. Gould suggests that there will always be anthropomorphic tendencies when humans investigate animal behavior because "we cannot write, study, or even conceive of other creatures except in overt or implied comparison with ourselves." Human entanglements with animals is a fact for Gould; what is at issue is the nature and degree of those entanglements. See Stephen Jay Gould, "Animals and Us: Review of *The Chimpanzees of Gombe: Patterns of Behaviour* by Jane Goodall", *New York Review of Books*, 25 June 1987, p. 20.
30. Judgments about scientists' *ethos* often overlap with considerations pertinent to appraising the technical reasonableness of research claims, or what classical rhetoricians would have called the *logos* of scientific discourse. Although closely interrelated, I believe that the arguments just reviewed are best read as an attack primarily on Patterson's *ethos* rather than as a simple critique of technical *logos*. Sebeok is trying to induce readers to doubt Patterson's technical virtuosity as a scientist. The *topos* of universality recommends the fitting line of thought: that "real" scientists willingly and capably test their claims against pre-established, impersonal standards. Patterson fails to display her technical virtuosity by neglecting two such impersonal standards: (1) the intellectual consensus among scientists regarding what is accepted and rejected knowledge; and (2) empirical observations (due to her alleged experimental incompetence).
31. I surmise that Sebeok is here referring to Patterson's Gorilla Foundation. In her book Patterson makes a tacit but obvious appeal to the laity for financial support of the Gorilla Foundation. See Patterson and Linden, *Education of Koko*, p. 213.
32. Of the three arguments that Sebeok draws from the *topos* of communality, only this one approximates Merton's idea of intellectual communism. From Merton's perspective, Patterson's scientific conduct is questionable because she does not willingly "share" her research by publishing in legitimate journals. Merton's idea of communism makes "secrecy" a vice because it is assumed that what is not being shared with the community is *worth* knowing. However, during demarcation crises *ability* to share can become as much an issue as willingness to share. I found it necessary to reinterpret the *topos* of communality as recommending this more encompassing line of thought: that "real" scientists actively participate in the intellectual life of their community. This is the persuasive basis of Sebeok's three arguments. Patterson fails to display community participation because she lacks a credible position, fails to secure "legitimate" research support, and will not or cannot share technical claims with scientific audiences.
33. Francine Patterson, "Conversation with a gorilla," *National Geographic*, 154, Oct. (1978), pp. 438-65 (condensed in *Reader's Digest*, 114, Mar. (1979), pp. 81-6; *Koko, A Talking Gorilla* (a ninety-minute 16mm film by Barbet Schroeder, available from New Yorker Films).
34. Quoted in Cynthia Gorney, "Gorilla Koko hasn't convinced everybody that she can talk," *Houston Chronicle*, 4 February 1985, section 5, p. 5.
35. Another way of putting this is that she lacked "good will" toward members of relevant knowledge communities. Sebeok recounts how Patterson and Linden responded to Terrace's conclusion in *Nim* that there is no evidence showing that apes can generate or interpret sentences by hurling "such epithets as 'muddle-headed' (Patterson), 'apostate' (Linden), and worse."
36. All of these statements imply that Patterson violated a major unwritten taboo in scientific life: she appealed "to the populace at large" rather than to the "well-defined community of the scientist's professional compeers," as Kuhn had put it. Scientists are especially vigorous in their public condemnations of those who seek to gain lay acceptance for unorthodox and unauthorized claims. The former is especially

- provides a case in point. See Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd edn (Chicago: University of Chicago Press, 1970), p. 168.
37. Barber, *Science and the Social Order*, p. 92.
38. Patterson and Linden, *Education of Koko*, pp. 24-6.
39. Individuality illuminates Patterson's and Linden's arguments better than solitariness, the purported counter-norm of Merton's intellectual communism. Rather than using "secrecy" to bolster their scientific *ethos*, they instead displayed their unwillingness to sacrifice truth to the authority of tradition and its prominent spokespersons. When anti-authoritarianism is treated as a virtue it often is called *individualism*, as some Mertonian commentators have noted. This *topos* conflicts with the *topos* of communality in the sense that I reformulated that *topos* earlier. See Barber, *Science and the Social Order*, pp. 89-90.
40. Patterson and Linden, *Education of Koko*, p. 194.
41. *Ibid.*, p. 79.
42. *Ibid.*, pp. 210-11. Patterson is not without distinguished company when making arguments of this kind. Barbara McClintock, winner of the 1983 Nobel Prize in Medicine or Physiology, offered a philosophical vision of science based not on objective experimentation but on what she called developing "a feeling for the organism." For her, intimacy with and sympathetic understanding of the objects of knowledge is required to have genuine scientific knowledge. Lest this application of "rapport" be dismissed merely as McClintock's philosophical speculation, let us also turn to Jane Goodall's studies of a chimpanzee colony at Gombe in Tanzania. According to Gould, Goodall's work exemplifies a scientist's internal struggles to strike the proper balance in research between emotional involvement with the chimpanzees and the need to secure "maximal distance" so that the chimpanzees' behavior does not become distorted through the scientist's intrusions. These internal struggles illustrate that rapport can have important influences on scientific research. Gould says of Goodall's work: "You can't just march off into dense foliage and find chimps; you must first make contact and build trust in order to win acceptance and establish the possibility of following in the wild" ("Animals and Us," p. 24). Goodall's experience also underscores the fact that topical choices among skepticism and enthusiasm, or objectivity and rapport, are not merely arbitrary. There are circumstances in which scientists can clarify ambiguities about their professional conduct through appeals to "rapport" as a virtue comparatively superior to "objectivity." See Gould, "Animals and Us," pp. 23-5; and Evelyn Fox Keller, *A Feeling for the Organisms: The Life and Work of Barbara McClintock* (New York: Freeman, 1983), pp. 197-207.
43. Patterson and Linden, *Education of Koko*, p. 207.
44. *Ibid.*, p. 25.
45. This argument is a revolutionary variation on the theme that virtuous scientists can serve special communities of interest. Patterson and Linden are virtuous not because their interests coincide with those possessed by members of an informal college, but because their interests are linked with issues that concern general society.
46. For an overview of the key points-at-issue see Larry Laudan, "Two Puzzles about Science: Reflections about Some Crises in the Philosophy and Sociology of Science," *Minerva*, 20 (1982), pp. 253-68.
47. When orthodox scientists attack parapsychology and creationism as "pseudo-science" they include among their means of persuasion efforts to discredit the scientific *ethos* of practitioners. They do this by claiming that parapsychologists and creationists do not think and act like "real" scientists. Typically the standard Mertonian arguments are adduced in support. Parapsychologists and creationists are said to be: (1) openly defiant of both the "universal" consensus on accepted and rejected knowledge and the need for empirical confirmation of technical claims (*universality*); (2) pursuing extra-scientific motives including advancement of beliefs in the supernatural (*disinterestedness*); (3) dogmatically attached to their allegedly "scientific" claims (*skepticism*); and (4) incapable of participating in the "real" scientific community, as indicated by their inability to secure visible positions, "legitimate" research funds, and publications in orthodox journals (*communality*). Parapsychologists have been far more successful than the creationists at legitimizing their scientific *ethos*, and they did this largely by emulating Mertonian virtues. Nevertheless, like the creationists and Patterson and Linden, they also attempt to diffuse orthodox criticism by creating a revolutionary scenario for advancing their "radical" claims. However, more critical work is needed before we can safely generalize that these and other "intellectual revolutionaries," like Patterson and Linden, draw arguments from the *topoi* of particularity, interestedness, dogmatism and individuality. On the use of Mertonian *topoi* against the creationists, see Gieryn, et al., "Professionalization of American Scientists," pp. 401-3. My claims about the parapsychologists are based on Paul D. Allison, "Experimental Parapsychology as a Rejected Science," in Wallis, *On the Margins of Science*, especially pp. 277-88; Collins and Pinch, "Construction of the paranormal," *passim*; and Jim Palmer, "Why is Science Spooked?" *Washington Post*, 8 Mar. 1987, sec. B, p. 3.

48. Kuhn describes functional features of these "good reasons" that reflect their rhetorical topicality: "Individually the criteria are imprecise: individuals may legitimately differ about their application to concrete cases. In addition, when deployed together, they repeatedly prove to conflict with one another," (Thomas S. Kuhn, "Objectivity, Value Judgment, and Theory Choice," in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (University of Chicago Press, Chicago, 1977), p. 322). Like the *topoi* related to scientific *ethos*, these technical *topoi* have evocative, inventional powers due to their working ambiguities, thus allowing generation of varied and sometimes conflicting arguments both within and across rhetorical situations. See *ibid.*, pp. 321-5.
49. For an example of how topical analysis can be applied to technical articles see S. Michael Halloran, "The Birth of Molecular Biology: An Essay in the Rhetorical Criticism of Scientific Discourse," *Rhetoric Review*, 3 (1984), especially pp. 73-4. In this volume, see Halloran. pp. 42-3.

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