The Birth of Molecular Biology: An Essay in the Rhetorical Criticism of Scientific Discourse

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In his introduction to the Norton Critical Edition of *The Double Helix*, Gunther Stent assigns a birthday to the science of molecular biology: April 25, 1953, the publication date of James Watson and Francis Crick's paper sketching the double helical structure they had devised for the DNA molecule.[1] Others have confirmed the view that this paper was pivotal in establishing molecular biology as a science. The editors of the journal *Nature* carried a series of papers under the collective title "Molecular Biology Comes of Age" just twenty-one years and a day after its publication, and they used a facsimile of it as the title page for the retrospective.[2] Horace Freeland Judson's exhaustive history of the field quotes from scores of scientific papers but reproduces *in toto* only this one.[3] More clearly than any single scientific paper in recent years, this one stands near if not precisely *at* the center of what Thomas Kuhn would call a scientific revolution.

I have written elsewhere of the rhetorical implications of Kuhn's view of the nature of science, and in a general way of the rhetorical dimensions of Watson and Crick's work.[4] This essay might be regarded as a sequel to that earlier one, but I don't intend to pursue further the theoretical implications of Kuhn's ideas for developing a rhetorical analysis of science. I want instead to develop a more thorough critical analysis of Watson and Crick's 1953 paper. Rhetoric has traditionally been a strongly empirical field of study in that it places great emphasis on the particular case. The job of the rhetorical critic is to discover what in the particular case were the available means of persuasion, and judge whether the rhetor managed them well or badly. The particular case commands his or her attention as something worth knowing in itself, apart from any general principles that might be abstracted from it. But while a number of scholars have been arguing theoretically that science is rhetorical, very little attention has been paid to particular cases of scientific rhetoric.[5]

This essay comes at the rhetoric of science from a critical perspective; I want to explicate a particular case that is surely worth the effort. Ultimately, I hope to show that the Watson-Crick paper establishes an *ethos*, a characteristic manner of holding and expressing ideas, rooted in a distinctive understanding of the scientific enterprise.

I am here consciously echoing lines from an essay by Edwin Black in which, while he does not use the term *ethos* as I am using it, he develops a very similar concept in connection with nineteenth-century American oratory:

> Groups of people become distinctive as groups sometimes by their habitual patterns of commitment—not by the beliefs they hold, but by the manner in which they hold them and gIve them expression. Such people do not necessarily share ideas; they share rather stylistic proclivities and the qualities of mental life of which those proclivities are tokens.[6]

The most general point I hope to make in this paper is that scientific communities can be bound together in this fashion. While the specific beliefs they hold—the *logos* of the discipline—may be crucial to a scientific community, their identity as a community may rest equally on "stylistic proclivities and the qualities of mental life of which those proclivities are tokens," that is, on what I am calling *ethos*. I will begin by concentrating rather closely on a single scientific paper, then try to place it in a larger context.

"A Structure for Deoxyribose Nucleic Acid" was the first published announcement of the double-helical structure Watson and Crick had devised for DNA, the molecule that had by the early 1950s been identified as the transmitter of genetic information.[7] While the story of how Watson and Crick arrived at their discovery has been told elsewhere,[8] certain facts bear retelling here, by way of outlining the rhetorical situation. First, there was a degree of competition surrounding the work: Linus Pauling in California was known to be working on the problem of DNA's structure; Maurice Wilkins and Rosalind Franklin at King's College in London were also working on it, and there was a vague sense in England that the problem belonged to them; Watson and Crick were supposed to be working on other matters at Cambridge, but they hoped to be first to the solution of the DNA molecule. Second, while the structure of the DNA molecule was regarded as an important research problem, no one knew beforehand just how important its solution would turn out to be. No one knew or even hoped that genetic information would turn out to be transmitted by a straightforward mechanical process, and that knowing the structure of DNA would therefore suggest the possibility of mastering and ultimately manipulating the process.

Because of the competitiveness of the situation and of the unanticipated significance of the discovery, Watson and Crick chose to publish their discovery in *Nature*, a journal that would publish the article promptly and reach a broad scientific audience. *Nature* is published weekly and, like the U.S. Journal *Science*, is not specialized in a particular discipline. An arrangement was made for Wilkins and Franklin to publish simultaneously with Watson and Crick results of their most recent x-ray diffraction studies, which tended to support the proposed model. What appeared in *Nature*, then, was a trilogy of articles under the collective title "Molecular Structure of Nucleic Acids." The first is Watson and Crick's paper; the second is "Molecular Structure of Deoxypentose Nucleic

Acids," signed M. H. F. Wilkins, A. R. Stokes and H. R. Wilson; the third is "Molecular Configuration in Sodium Thymonucleate," signed Rosalind E. Franklin and R. H. Gosling.[9] This apparent attempt to portray the discovery of the double helix as a broad-based team effort failed. The Watson-Crick paper is reprinted in three places that I know of with the title "Molecular Structure of Nucleic Acids," but without the Wilkins and Franklin papers that properly go with it under that title.[10]

The paper consists of fourteen paragraphs totaling just over 900 words. It contains one figure—a "purely diagrammatic" representation of two helices wound around a central axis—and no formulas. The text is organized as follows:

Paragraph 1—introduction: "We wish to suggest a structure for the salt of . . . (DNA). This structure has novel features which are of considerable biological interest."

Paragraphs 2–3—review of selected literature: Models of DNA proposed by Pauling and Corey (paragraph 2) and Fraser (paragraph 3) are considered and rejected.

Paragraphs 4–12—body of the paper:

Paragraphs 4–5 sketch the broad outlines of the model (two helical chains wound around each other).

Paragraphs 6–8 describe the "novel feature" of the structure, the mechanism by which the two chains are bound together. Each chain contains four bases in a sequence that "does not appear to be restricted in any way." The bases on one chain form hydrogen bonds with those on the other according to a fixed pattern (adenine bonds only to thymine, guanine only to cytosine), so that the sequence of bases on one chain automatically determines the sequence on the other.

Paragraph 9 notes that experimentation has shown adenine equal to thymine and guanine equal to cytosine in DNA.

Paragraph 10 speculates that the structure will not be found in RNA.

Paragraph 11 considers the status of the proposed model relative to available x-ray data. The model is "roughly compatible" with the data, "but it must be regarded as unproved. . . . Our structure . . . Rests mainly though not entirely on published experimental data and stereochemical arguments."

Paragraph 12: "It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material."

Paragraphs 13–14—conclusion: Paragraph 13 promises a more detailed picture of the structure to be published elsewhere; paragraph 14 acknowledges the help of a few other scientists, including Wilkins and Franklin.

There are, as I see it, three substantial arguments put forward in support of their model by Watson and Crick. The most important is the great elegance of the model, particularly the base-pairing mechanism (described in paragraphs 6–8) that holds the two helical chains together. This argument is in their paper left entirely implicit, as it probably had to be. They assume that the description in paragraphs 4–8 will appeal strongly to the reader's sense of theoretical elegance. The argument is in effect an enthymeme whose missing premise is a scientific *topos* so basic and powerful that it would be gauche in the extreme to state it openly in a technical paper. But in *The Double Helix* Watson makes the argument from elegance explicit: "a structure this pretty just had to exist."[11] (Future developments in the history of molecular biology would demonstrate that a structure could be extraordinarily "pretty" and still not exist. The story of the comma-free code—"an idea of Crick's that was the most elegant biological theory ever to be proposed and proved wrong"—is too intricate to warrant retelling here, and it is told with appropriate elegance by Judson.[12] I mention it to underscore my point that for the scientist elegance functions not as an absolute principle, but as a rhetorical *topos*, a premise for argument in contingent cases.)

The second argument offered by Watson-Crick is that the proposed model provides a very precise theoretical explanation for what before had been simply a curious fact—the observed ratios of adenine to thymine and guanine to cytosine, referred to in paragraph nine. Given the base pairing mechanism that holds the DNA molecule together according to the model, the ratios become inevitable rather than just curious. What is interesting rhetorically about this argument is that Watson and Crick leave it implicit just as they had the argument from elegance, though in this case they might have made their point explicit without any impropriety. Instead they merely juxtapose a statement of the observed ratios immediately following the descriptonof the base-pairing mechanism, and expect the reader to make the connection. The argument is another enthymeme resting on the *topos* of explanatory power.

The third argument is negative: the proposed model is not inconsistent with any available experimental data. Unlike the first two arguments, this one is laid out explicitly, in paragraph 11. But notice how carefully qualified the statement is.

Argumentatively, then, the paper is understated, and the rhetorical effect is to communicate a sense of supreme confidence. The claim that the proposed model is of "considerable biological interest" is advanced boldly in the first paragraph, and the arguments in support of the model are assumed to be so persuasive that they need no bolstering of emphasis.

Stylistically, the most striking quality of the paper is its genteel tone. Note, for example, the diction of the introductory paragraph: "We *wish* to *suggest* a structure for the salt of deoxyribose nucleic acid (D.N.A.). This structure has *novel features* which are of *considerable biological* interest" (italics added). Note too the delicate fashion in which they reject the model that had been proposed by Linus Pauling and his colleague: "*In our opinion*, this structure is *unsatisfactory* for two reasons: (1) *We believe* that the material which gives the

X-ray diagrams is the salt, not the free acid. Without the acidic hydrogen atoms *it is not clear* what forces would hold the structure together. . . . (2) Some of the van der Waals distances *appear to be* too small" (Paragraph 2; italics again mine). That this is a consciously contrived style becomes apparent in light of *The Double Helix*, from which we know that Watson and Crick regarded the Pauling-Corey model as an incredible blunder, a violation of the most elementary fact of chemistry. They were astonished and jubilant to find the great Pauling guilty of what they regarded as a gross error. If we can believe *The Double Helix*, the genteel style of Watson and Crick's first published paper reflects a rhetorical persona, perhaps fabricated with a bit of intentional, tongue-in-cheek irony; in the flesh they were obstreperous and irreverent.

Note finally the one sentence paragraph that concludes the body of the paper, a sentence in which the genteel style becomes a transparent burlesque: "It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material." One can almost feel the elbow in one's ribs.

The effect of Watson-Crick's self-consciously genteel style is to give the paper a highly personal tone that is somewhat unusual in scientific prose. There are a number of conventional devices by which scientific prose is depersonalized. Simplest and most frequently noted by critical readers is the passive voice construction: "It was observed that . . ." Rather than "I observed . . ." Somewhat more subtle and rhetorically interesting is a device that one finds with increasing frequency in academic writing across the disciplines, a device that amounts to the manufacture of abstract rhetors: "*The data* show that . . ." Or "*This* paper will argue that . . ." The effect of the device is to suppress human agency, to imply that what are essentially rhetorical acts—arguing, showing, demonstrating, suggesting—can be accomplished without human volition. Watson and Crick are noteworthy in that they generally avoid this convention, particularly in putting forward their own case. They claim the argument quite explicitly as their own: "We wish to suggest" "In our opinion" "We believe" "We wish to put forward" "It has not escaped our notice" "We have postulated." By contrast, the Wilkins et al. paper that appears immediately following Watson-Crick in the April 1953 *Nature* is bloodless and impersonal in the manner more typical of scientific prose: "The purpose of this communication is . . ." "It may be shown that . . ." "It must be decided whether . . ." "The . . . significance of a two chain nucleic acid unit has been shown . . ."

Both argumentatively and stylistically, then, Watson and Crick put forward a strong proprietary claim to the double helix. What they offer is not *the* structure of DNA or *a* model of DNA, but Watson and Crick's structure or model. Moreover, in staking their claim they enact a distinctive way of adhering to ideas in public; they dramatize themselves as intellectual beings in a particular style. The paper articulates a recognizable public persona, an *ethos*. The Watson-Crick *ethos* does not necessarily overturn established conventions of scientific rhetoric, though we shall see that it offends at least one authoritative sensibility. What I believe it does is shape a particular image of *the scientist speaking*, within a broader set of more vague and general norms that apply to all scientific discourse. And this *ethos,* I contend, is an important aspect of what Kuhn would call the paradigm offered to the broader scientific community.

This claim is a rather large critical speculation based on my general sense of how rhetorical norms operate in scientific communities. Some evidence in its support can be found by comparing Watson and Crick's rhetoric with that of other biologists, and I will turn now briefly to a paper without which Watson and Crick's own work might not have been possible.

The effort devoted to discovering the structure of DNA—not just by Watson and Crick, but by Wilkins, Franklin, Pauling and a great number of others—was of course based upon a consensus that DNA is indeed the substance that transmits genetic information from one generation of cells to the next. The first published demonstration of this crucial fact was a paper by Oswald Avery and two associates that appeared in 1944 in the *Journal of Experimental Medicine.*[13] Avery and his colleagues had used a well-known experimental procedure in which one strain of Pneumococcus bacteria is transformed into a genetically distinct strain. By a series of tortuously executed procedures, they isolated the "active principle" involved in the transformation and identified it as DNA. Prior to the publication of their work, DNA was thought to be a genetically irrelevant substance, and most biologists assumed that genes consisted of some form of protein. In a sense, then, one might date the "revolution" in molecular biology from the appearance of their paper rather than Watson and Crick's. The simplest reason for not doing so is that Avery's work did not have an immediate revolutionary effect. Although it is now regarded as an air-tight demonstration of DNA's role in heredity, scientists were slow to accept it as such and focus on DNA's structure as a biologically important problem. According to Gunther Stent, Avery's discovery was "premature" because it could not at the time be connected with canonical knowledge in the field.[14]

An obvious feature of the Avery et al. paper is that by comparison with Watson and Crick's on the double helix, it is much longer and more dense with technical detail. Whereas Watson and Crick simply sketch in broad outline the results of their work, Avery and his colleagues rehearse in painstaking detail the experimental technique by which the "active principle" was isolated and then the analytic techniques by which it was identified as DNA. In effect they present their case according to the method of residues, recording a sequence of technical procedures that gradually narrows the explanatory possibilities down to the single conclusion that the substance responsible for the phenomenon in question is DNA. A characteristic point of their argumentative strategy is that the paper does not state its thesis in the introductory section and in fact does not even mention the substance DNA until roughly half-way through its 7500 word length. They make no strong claims about the importance of their discovery, and in fact introduce the paper as simply a "more detailed analysis" of the already well-known transformation phenomenon. They observe all the conventions of depersonalization: events transpire in the passive voice, data suggest conclusions without human assistance, and Avery and his colleagues take on that ultimate *nom de plume*, "the writers."

I am tempted to suggest that the "prematurity" of Avery's work was owing in part to his rhetoric in presenting it to the scientific community.[15] But while I think that a persuasive case could be made for such a claim. I am more concerned here simply to point up the contrast between Avery's *ethos* and that of Watson and Crick. The character that speaks to us from Avery's paper is that of a cautious skeptic who is forced somewhat unwillingly to certain conclusions. That of the Watson-Crick paper is quietly confident, so much so that "he" can indulge in a gentle bit of leg-pulling. I put quotation marks around "he" for the obvious reason that the voice in which the paper speaks is in a sense that of two men speaking in unison, a fact that points up the corporate, conventional, public nature of the phenomenon I am trying to capture. What interests me here is not the unique personality of an Oswald Avery or a Francis Crick, but the public role of *scientist* as dramatized by them. They offer two sharply contrasting versions of that role, two images of a fitting way for scientists to hold ideas.

A larger view of the contrast comes into focus if we consider the notions of form and strategy. At a simple level, the Watson and Crick paper offers something very close to a textbook illustration of Burkean form: the "arrows of desire" are pointed in the opening sentences,[16] which promise that the proposed structure has *novel features* which are of *considerable biological* interest. The promise of novel features is satisfied by paragraphs 6–8, which describe the crucial base-pairing mechanism, introducing it with the phrase, "The novel feature of the structure is . . ." The promise of biological interest is partially satisfied by the last substantive paragraph (12), which hints at an explanation of the genetic process. But of course this is simply a further pointing of arrows, and the appetite so aroused is in turn satisfied by a second paper that appeared in *Nature* just five weeks later, this one speculating on how the DNA molecule (as described in "our model") might transmit genetic information by means of an essentially mechanical process.[17] And, since neither of these two brief papers develops a sufficiently detailed picture of the model for other scientists to begin working with it, both together serve to create an appetite for two more technically elaborate papers that followed in more specialized journals.[18] Finally, just eighteen months after the publication of the original brief paper in *Nature*, Crick published an essay in *Scientific American* reviewing the state of the art in molecular biology and placing his work with Watson in this larger context.[19]

The April 1953 paper, then, is really just the initial move in a rhetorical strategy aimed at gaining and holding the attention of an audience. As such, it presumes an understanding of science as a human community in which neither facts nor ideas speak for themselves, and the attention of an audience must be courted. By contrast, Avery and his colleagues present their work in a single technical paper structured in a reportorial pattern which implies that facts *do* speak for themselves. Their strategy seems to presume that the work of the scientist is simply to give oneself up to the facts. Avery speaks from within an essentially positivistic, pre-Kuhnian view of science, Watson and Crick from within what Frederick Suppe calls a *Weltanschauungen* view.[20] They recognize that a discipline includes tacit assumptions about what is and what is not a legitimite question, and that in order to gain a hearing for a new theory, one may have to suggest what use the theory might have, what new questions it might both pose and answer, what new lines of research it might open up.

The success of the Watson-Crick strategy is indicated by the publication of Crick's essay in *Scientific American*, a periodical that addresses the entire scholarly community and carries a very substantial weight of authority. In this piece, Crick is no longer speaking only for himself and Watson, but for the community of specialists in molecular biology as well. When he writes of what "we" know to be the case and what "we" regard as an important research problem, he speaks for an international scientific community, defining their view of the world to an audience that embraces scholars and scientists in all disciplines. The essay does not yet speak of the double-helical model of DNA as one of the established facts of biology; the model is still in effect the private property of Watson and Crick. But as "owner" of that theory, Crick has gained the authority to say what the facts are in biology and to place the theory before the entire scholarly community in the context of those facts. The implication is that the theory is a strong candidate for admission to the canon of established knowledge in biology.

Perhaps I should be explicit here on a point that I hope would go without saying: none of this is meant to deny the importance of Watson and Crick's model in its technical particulars. To say with Perelman that a *fact* is defined by its claim to the adherence of the universal audience is not to deny that a fact must also correspond to an observation of the world.[21] The double-helix and its attendant explanation of genetic information transfer would not have survived as a theory had it failed to work in predicting experimental observations. My point is to bring into focus another aspect of Watson and Crick's work, a rhetorical aspect that falls under the heading of *ethos*. In offering their model of DNA to the scientific world, they simultaneously offered a model of the scientist, of how he ought to hold ideas and present them to his peers. I believe that this ethical aspect of Watson and Crick's work contributed to the speed with which their model of DNA gained prominence as a theory, but I have been more concerned simply to explicate the *ethos*.

One question remaining is whether the confident, personal, rhetorically adept *ethos* of Watson and Crick was effective *as a model*, whether it was adopted by other scientists. A strongly persuasive answer to that question would have to rest upon close rhetorical analysis of the work of later biologists. That analysis remains to be done. In the meanwhile, I can offer two somewhat weaker arguments in support of my belief that Watson and Crick *have* become a rhetorical-ethical model for others.

First, there is today an adventuresome, entrepreneurial, slightly irreverent spirit associated with the field of molecular biology and genetic engineering, a spirit that on its face strikes me as a recognizable offspring of the Watson-Crick *ethos*. The irreverence is apparent in the breezy, somewhat whimsical terminology current in the field: "gene splicing" is done with the assistance of a "gene machine"; segments of "genetic gibberish" on the DNA molecule are thought by some to act as "genetic errand boys."[22] I am inevitably reminded of the touches of delicate irony in the original Watson-Crick paper. The entrepreneurial spirit is evident in the enthusiasm with which researchers have welcomed opportunities for commercial exploitation of knowledge, even to the possible detriment of traditional academic values. According to an article in a recent issue of *Science 81*,

> Currently there is not a single top-ranking molecular biologist at an American university who has not signed up with one of the new genetics companies. The development dismays some biology watchers. One of science's brighter points as a human endeavor has been the traditional willingness of scientists to share time, information, and even specimens and equipment. That scientific knowledge should be considered private property is a concept repugnant to many scientists.[23]

I would contend that this notion of scientific knowledge as private, profit-making property is simply a logical extension of the manner in which Watson and Crick laid proprietary claim to their original discovery.

It is also a reduction to a peculiarly simple form of a tendency present generally in modern science. In an essay on the nature of scientific discovery, Kuhn points out that "to make a discovery is to achieve one of the closest approximations to a property right that the scientific career affords."[24] In her study of the cultural effects of print technology, Elizabeth Eisenstein makes a similar point about the importance of clarifying the proprietary claims of authors as a condition for the development of modern science: the incremental building of knowledge—science—presumes a certain niceness in identifying the individual components in the developing structure.[25] Heretofore the scientist's claim to a discovery was a rather inexact approximation of a property right, in that one exercised this right most fully by having others make free use of the property; a discovery became a "contribution" in the very moment that it became one's own property. There was, perhaps, some residue of the much older rhetorical tradition, within which all knowledge was in a sense *commonplace*—available for use by anyone—and such notions as plagiarism, copyright, and product patent would consequently have made little sense.

My second argument rests on the testimony of one of the original actors in the revolution in molecular history. The chemical ratios in DNA which Watson and Crick offered as evidence for their model (paragraph 9 of the paper) had been established in the laboratory and documented in the literature by Erwin Chargaff. These equalities were in fact known as "Chargaff's ratios," and Watson and

Crick's failure to mention his name (except as a footnote) in connection with this crucial piece of evidence might be regarded as a slight. It is perhaps worth noting that while Watson and Crick generally avoid the passive voice, in paragraph 9 they use it with the result that Chargaff's contribution becomes anonymous: "It has been found experimentally that. . . ." In any event, Chargaff has become a strong critic of the direction that biology has taken since the discovery of DNA's structure, and the tenor of his critique tends to support my view that the Watson-Crick *ethos* has been adopted by others. For example, in the 1974 *Nature* retrospective, "Molecular Biology Comes of Age," he recalls reading the original Watson and Crick papers in this way: "The tone was certainly unusual: somehow oracular and imperious, almost decalogous. Difficulties were brushed aside in *the Mr. Fix-it spirit that was to become so evident in our scientific literature*" (italics added).[26] In Chargaff's mind, Watson and Crick have influenced not just the ideational content of biology, but the manner in which ideas are pursued, the spirit in which science is done. The "Mr. Fix-it spirit" that he deplores is what I have been calling an *ethos*.

Assuming that my analysis of the Watson-Crick *ethos* is at all persuasive, it has one very important implication for rhetorical studies of scientific discourse: except at the most general level, it may be misleading to speak of *the* rhetoric of science. While there is a sense in which what Oswald Avery was doing rhetorically is "the same as" what Watson and Crick were doing, the contrasts are profound, and they suggest the possibility that other particular cases of scientific rhetoric will exhibit their own peculiarities. A detailed understanding of the rhetoric of science will have to include some sense of the permissible range of variation. To achieve this sense, we need a body of critical literature on particular cases of scientific discourse.

I am suggesting the need for a program of what Black calls "emic"criticism, criticism that begins with the particular instance and aims toward the development of theories comprehending more general principles that operate across larger bodies of discourse.[27] To do this with scientific discourse is particularly difficult simply because the discourse is specialized and highly demanding. It is a daunting experience to take up a technical paper in, say, molecular biology, equipped with an education in the hard sciences that ends somewhere around the time of Matthew Arnold and T. H. Huxley. That it can be done responsibly is demonstrated by Horace Freeland Judson, who started as a journalist and wrote what is generally regarded as a definitive history of molecular biology. That it should be done by scholars in rhetoric is suggested by the increasing importance of scientific matters in the arena of public affairs, the traditional realm of rhetoric. Science is itself an increasingly public enterprise, both in the sense that the public supports it financially and in the sense that it offers monumental threats and promises to our well-being. Science also serves as warrant for many of the arguments about traditionally non-specialized, civic questions—war and peace, ways and means for promoting the public welfare. To understand public discourse in the closing decades of this century, we must have some understanding of scientific discourse.

Notes

1. James D. Watson, *The Double Helix: A Personal Account of the Discovery of the Structure of DNA*—Text, Commentary, Reviews, Original Papers, ed. By Gunther S. Stent (New York: W. W. Norton & Company, 1980), xi.
2. "Molecular Biology Comes of Age," *Nature*, 248 (April 26, 1974), 765-88.
3. Horace Freeland Judson, *The Eighth Day of Creation: Makers of the Revolution in Biology* (New York: Simon and Schuster, 1979), 196-98.
4. "Technical Writing and the Rhetoric of Science," *Journal of Technical Writing and Communication*, 8 (Spring 1978), 77-88; reprinted in *Technical Communication* (fourth quarter 1978), 7-10, 13.
5. Here follow some of the more interesting studies of scientific rhetoric: John Angus Campbell, "Charles Darwin and the Crisis of Ecology: A Rhetorical Perspective," *QJS*, 60 (Dec. 1974), 442-49; Paul Newell Campbell, "The *Personae* of Scientific Discourse," *QJS*, 61 (Dec. 1975), 391-405; Joseph Gusfield, "The Literary Rhetoric of Science: Comedy and Pathos in Drinking Driver Research," *American Sociological* Review, 41 (Feb. 1976), 16-34; Carolyn R. Miller, "Technology as a Form of Consciousness: A Study of Contemporary Ethos," *CSSJ*, 29 (Winter 1978), 223-36; Michael A. Overington, "The Scientific Community as Audience: Toward a Rhetorical Analysis of Science," *Philosophy and Rhetoric*, 10 (Summer 1977), 143-64; Herbert W. Simons, "Are Scientists Rhetors in Disguise? An Analysis of Discursive Processes Within Scientific Communities," in Eugene F. White (ed.), *Rhetoric in Transition: Studies in the Nature and Uses of Rhetoric* (Univ. Park: Penn. State U. Press, 1980), 115-30; Philip C. Wander, "The Rhetoric of Science," *Journal of Western Speech Communication*, 40 (Fall 1976), 226-35; Walter B. Weimer, "Science as a Rhetorical Transaction: Toward a Nonjustificational Conception of Rhetoric," *Philosophy and Rhetoric*, 10 (Winter 1977), 1-29. Of these, only Campbell's paper on Darwin, and Gusfield's on drinking driver research are *critical* essays in the sense that they include close analysis of particular rhetorical transactions.
6. Edwin Black, "The Sentimental Style as Escapism, or The Devil with Dan'l Webster," in Karlyn Kohrs Campbell and Kathleen Hall Jamieson (eds.), *Form and Genre: Shaping Rhetorical Action* (Falls Church, Va.: SCA, n.d.), 85.
7. J. D. Watson and F. H. C. Crick, "A Structure for Deoixyribose Nucleic Acid," *Nature*, 171 (April 25, 1953), 737-38.
8. In addition to *The Double Helix* and *The Eighth Day of Creation*, see Robert Olby, *The Path to the Double Helix* (London: Macmillan, 1974).
9. *Nature*, 171 (April 25, 1953), 738-41; both papers are reprinted in the Norton Critical Edition of *The Double Helix*, 247-57.
10. Both the 1974 *Nature* retrospective and Judson's *Eighth Day* reprint the Watson-Crick paper in this somewhat misleading way. The same error is committed by Mary Elizabeth Bowen and Joseph A. Mazzeo (eds.), *Writing About Science* (New York: Oxford University Press, 1979). Stent's Norton edition of *The Double Helix* includes the Wilkins et al. and Franklin-Gosling papers, but it places them following a second Watson-Crick paper that appeared in *Nature* more than a month after the original trilogy of DNA papers.
11. *The Double Helix*, 120.
12. *The Eighth Day of Creation*, 318 ff.
13. Oswald T. Avery, Colin M. MacLeod and Maclyn McCarty, "Studies on the Chemical Nature of the Substance Inducing Transformations of Pneumococcal Types," *Journal of Experimental Medicine*, 79 (1944), 137-58; reprinted in Harry O. Corwin and John B. Jenkins (eds.), *Conceptual Foundations of Genetics: Selected Readings* (Boston: Houghton Mifflin Company, 1979), 13-27.
14. Gunther S. Stent, "Prematurity and Uniqueness in Scientific Discovery," *Scientific American*, 227 (Dec. 1972), 84-93.
15. Judson speaks of the Avery et al. paper as "a model of reasoning from and about experiment" (*The Eighth Day of Creation*, 37). I see no conflict between this view and my own belief that the paper is *rhetorically* weak.
16. I am thinking of Kenneth Burke's notion of form, as developed in the essays "Psychology and Form" and "Lexicon Rhetoricae," both in *Counter-Statement* (Berkeley: University of California Press, 1968), 29-44 and 123-83. My belief that Watson and Crick make use of what Burke calls the psychology of form in presenting their model of DNA would require some qualification of the views Burke expresses about science in these essays.

17. J. D. Watson and F. H. C. Crick, "Genetical Implications of the Structure of Deoxyribonucleic Acid," *Nature*, 171 (May 30, 1953), 964-67; reprinted in the Norton Critical Edition of *The Double Helix*, 241-47, and in Crown and Jenkins, *Conceptual Foundations of Genetics*, 52-55.
18. J. D. Watson and F. H. C. Crick, "The Structure of DNA," *Cold Spring Harbor Symposia on Quantitative Biology*, 18 (1953). 123-31; F. H. C. Crick and J. D. Watson, "The Complementary Structure of Deoxyribonucleic Acid," *Proceedings of the Royal Society*, A, 223 (1954). 80-96. Both papers are reprinted in the Norton Critical Edition of *The Double Helix*, 257-74 and 274-93.
19. F. H. C. Crick, "The Structure of the Hereditary Material," *Scientific American*, 191 (Oct. 1954), 54-61.
20. Frederick Suppe (ed.), *The Structure of Scientific Theories* (Urbana: University of Illinois Press, 1974), 125-220. In addition to Kuhn, Suppe identifies Stephen Toulmin, N. R. Hanson, Paul Feyerabend, Karl Popper, and David Bohm as proponents of *Weltanschauungen* views of science.
21. Ch. Perelman and L. Olbrechts-Tyteca, *The New Rhetoric: A Treatise on Argumentation*, trans. By John Wolkinson and Purcell Weaver (Notre Dame: University of Notre Dame Press, 1969), 67-70.
22. Graham Chedd, "Genetic Gibberish in the Code of Life," *Science 81*, 2 (Nov. 1981), 50-55.
23. Boyce Rensberger, "Tinkering with Life," *Science* 81, 2 (Nov. 1981), 47-48.
24. Thomas S. Kuhn, "Historical Structure of Scientific Discovery," *Science*, 136 (1 June 1962), 760.
25. Elizabeth L. Eisenstein, *The Printing Press as an Agent of Change: Communications and Cultural Transformation in Early-modern Europe* (Cambridge: Cambridge University Press, 1979), 119 ff.
26. Erwin Chargaff, "Building the Tower of Babel," *Nature*, 248 (April 26, 1974), 778. See also Chargaff's "A Quick Climb Up Mount Olympus: A Review of *The Double Helix*," *Science*, 159 (29 March 1968), 1448-49. According to Stent, Chargaff refused permission for this piece to be reprinted together with other reviews in the Norton Critical Edition of *The Double Helix* (p. 168).
27. Edwin Black, "A Note on Theory and Practice in Rhetorical Criticism," *Western Journal of Speech Communication*, 44 (fall 1980), 331-36.