Chapter Two

Social Construction in Two Biologists' Proposals

Why begin a study of scientific writing with grant proposals? Research articles (which I consider in chapter 3) are usually taken as the central genre of scientific writing, and popularizations (chapter 5) are more familiar to nonscientists; proposals may seem to be purely administrative documents, necessary to the scientists and agency officials but unexciting to the sociologist of science. But for many scientists heading large laboratories, proposals are in one practical sense the most basic form of scientific writing: the researchers must get money in the first place if they are to publish articles and popularizations, participate in controversies, and be of interest to journalists. For these researchers proposal writing is by no means an occasional administrative duty; it is constant effort that may involve approaches to a number of different agencies, that may take about a quarter of the director's working time, and that requires more and more attention as grants are given for shorter periods, and fewer projects are funded.

Proposals are a promising place to begin a study of scientific texts in that they are the most obviously rhetorical genre of scientific writing: both writers and readers know that every textual feature of a proposal must be intended to persuade the granting agency. The rhetoric can be finely calculated because proposals are written for a very small audience. Most academic scientific research in the United States is funded by the National Institutes of Health (NIH) and the National Science Foundation (NSF), through a procedure of peer review in which researchers' written proposals are evaluated by panels of researchers from the same general field. The proposal is likely to be read only by the members of this panel, and by the Executive Secretary who administers the section covering the proposal topic.

Nonscientists have often raised questions about the fairness of such a system. John B. Conlan, a former congressman from Arizona, has charged, "It is an incestuous buddy system that frequently stifles new ideas and scientific breakthroughs," and Michael Kenward quotes an observer who says it is like a murder trial with "a jury of axe murderers from the same gang." There has been a great deal of study of the peer review procedures of these agencies, both by social scientists and by in-house committees.[1] Both the public criticisms and the studies have focused on what happens in the funding after the proposals are submitted, and have asked what role factors beside "quality of science" play in the decision to fund or not to fund a project; they ask, for instance, if there is an "old boy network," or if the panels tend to reject "high-risk" proposals. But the funding process starts before the panel even sees the proposal. I will argue that the writing of proposals, which takes up such a large proportion of the active researcher's time, is part of the consensus building essential to the development of scientific knowledge. To take the metaphor of the critic Kenward quotes, it brings the axe-murderers into the gang.

There is a paradox in the rhetorical strategy of the proposal, because the proposal format, with its standard questions about background and goals and budget, and the style, with its passives and impersonality, do not allow for most types of rhetorical appeals; one must persuade without seeming to persuade. And yet almost every sentence is charged with rhetorical significance. In classical rhetorical terms, the forms of appeal in the proposal are ethical and pathetic as well as logical; one shows that one is able to do the work, and that the work is potentially interesting to one's audience of other researchers, as well as showing that one is right. The writer describes the work so as to create a persona (a presentation of the author in the text) and insert the work into the existing body of literature. One has a special problem if one sees one's work as new or falling between two specialized fields; one must either present a persona as an established member of one of the fields, or redraw the fields around the work. In either case one places the potentially dissenting idea within a new consensus. The process of writing a proposal is largely a process of

1. See, for instance, S. Cole, R. Rubin, and J. Cole, "Peer Review and the Support of Science," *Scientific American* 237, no. 4 (1977): 34–41 (Conlan quotation, p. 34); J. Cole and S. Cole, "Which Researcher Will Get the Grant?" *Nature* 279 (1979): 575–76; S. Cole, J. Cole, and G. Simon, "Chance and Consensus in Peer Review," *Science* 214 (1981): 881–86; M. Kenward, "Peer Review and the Axe Murderers," *New Scientist* 31 May (1984): 13. F. van den Beemt and C. Le Pair, "Appraisal of Peer Review" (unpubl. paper) studied the mechanisms in the Netherlands. Joop Schopman, of the University of Utrecht, has written a detailed study of the proposals that led to the Center for the Study of Language and Information at Stanford: *The Foundation of the Center for the Study of Language and Information* (Utrecht: Rijksuniversiteit Utrecht, Department of the Epistemology and Philosophy of Science, 1988).

presenting—or creating—in a text one's role in the scientific community. Thus the texts of proposals may have something to tell us about how science changes and defines itself, as well as about how it is funded and how it is communicated.

I have collected all major drafts of proposals by two biologists at the University of Texas, David Bloch and David Crews (I shall analyze two of their articles in chapter 3). In one case, these were successive proposals submitted to several agencies over the course of eighteen months; in the other they were drafts of one proposal written and rewritten over the course of the ten months before its submission. In both cases the authors had comments of readers from which to revise, one using the peer reviews of previous attempts, the other getting comments from coworkers, so I collected these comments and the writers' responses to the comments. Thus I worked with three readings of the proposals: the writers', the readers', and my own.

For each proposal, I noted changes between drafts (first to second, second to third, etc.), sometimes following handwritten changes on the drafts. I categorized changes by what seemed to motivate them and noted especially those changes that seemed to indicate the writer's self-presentation or relation to the research community. I also noted changes of the content of the proposals, but as we shall see, there were few such changes (this might not be the case with many other proposals). In addition to these revisions, the authors made many of the improvements in readability any good writer might make. The changes affecting persona or context in the community are largely specific to the field, and as a nonspecialist, I had to have some clue from the writers or commentators to interpret them. And these categories are themselves matters of interpretation; my categorization of revisions represents one view of the text, which changed as I read on and as I tested my reading against other readings. I interviewed the writers about my interpretation of selected revisions, and I have also had them check, at various stages of my writing, the views in the study as a whole.[2]

2. I noticed, for instance, that in my earlier analyses, I tended to interpret almost all revisions as improving readability or accuracy, whereas later I tended to see more revisions as related to the author's self-presentation or place in the community. This shift may reflect a real difference between earlier and later drafts, as I show in discussing the authors' changes in strategy. It may also reflect a change in my reading in the course of the study. I began as a technical writing teacher, especially aware of ease of reading and precision of statements. As I read more drafts, comments, and letters, and especially as I interviewed the writers, I became more aware of the context in which these changes were made.

As the brief résumés at the end of chapter 1 show, the researchers I studied are in some respects representative of biologists at large research universities: both have supervised laboratories and have published many articles, and both have in the past received grants and reviewed grant applications themselves. But, as we shall see from the responses of referees to their articles (see chapter 3), their work is potentially controversial. The fact that they anticipated resistance to their proposals may have made them more self-conscious about their writing processes, and it certainly makes the rhetorical features of their proposals more apparent to a nonbiologist discourse analyst.

In chapter 3 we shall follow one of David Bloch's first publications in molecular evolution, a new area of research for him. His funding during this period was from a National Institute of General Medical Sciences (NIGMS) grant and from the NSF, for his flow cytometric studies. When he first set out to test his model, he used published data and a computer program written by one of his graduate students and did not have any outside funding for the work except for university grants for computer time and a semester off from teaching. As these studies developed, he applied to the NSF twice (versions 1 and 2 of the proposal) and to the National Aeronautics and Space Agency (NASA) for support, not getting funded but apparently getting much closer. Then he applied to the Public Health Service (version 3 of the proposal) and reapplied to the NSF.

Bloch had, in his favor, a successful laboratory and an original idea on a topic of great theoretical interest. He also had a fresh familiarity with the literature and a demonstrated expertise with, and access to, computers. All this impressed his more favorable reviewers. But as we have seen, he had not gone through a conventional apprenticeship in nucleic acid research; he was not known to the leaders in the field, and he was not oriented toward the structure and function studies that occupy most researchers in nucleic acid sequencing. The most critical of his early reviewers bluntly rejected his proposal as that of a newcomer to a field already full of people doing structure-function research. Until the *Journal of Molecular Evolution* published his article in December 1983, his papers in this area could only be listed as "submitted." And he had a clear enthusiasm for one model, which left him open to charges that he was jumping to conclusions prematurely, on the basis of insufficient data. Bloch was well aware of his rhetorical strengths and vulnerabilities from the responses he had gotten to his papers at conferences, to the articles he had submitted for publication, and to his proposals to various agencies. Like Crews, he was acutely conscious of increased competition for research funds. Bloch's proposals have the same sections as Crews's but since he did not have a long series of separate experiments to describe and justify, they are much shorter, following PHS page limits, with about ten pages of text. His collaborators in his group contributed comments and criticism, but he was entirely responsible for the writing. At the time of this study he had been working on the project for more than two years, with countless drafts (on a text processor) of four article manuscripts (see chapter 3) and five submitted proposals, in addition to applications to the university, between January 1982 and June 1983. Later versions of the proposals did not go through so many drafts. Though he accumulated more and more data in his sequence searches and responded or adapted to criticism, the proposals did not grow any longer. As we shall see, increasingly detailed discussions of data that he had gathered were substituted for passages that described the model.

David Crews's lab, with two postdoctoral and several graduate students, and half a floor of a building, is now funded by grants from NSF (for one species), NIH (National Institute for Child Health and Human Development) (for two other species) and a National Institute of Mental Health (NIMH) Research Scientist Development Award (for him). I studied his work between September 1982 and July 1983 on a proposal for a competitive renewal, after five years, of his current NIH grant. Although he had a strong record as a researcher, he was concerned about the scarcity of research funds. His panel would not be the same one that awarded him the earlier grant, and his work would not be the same work. He had received both enthusiastic and sharply critical responses to his current research reports, and he could not afford even one critical review of the proposal. He would have to face or to sidestep this resistance. Also, the increased competition for federal funds meant he needed to prepare for close scrutiny according to the interests of NIH. He would have to show that his work on lizards had fairly direct application to problems of human reproduction. He would have to justify his field work on behavior, a type of work for which he thought NIH had little enthusiasm, and he would have to show that the large number of experiments he proposed all made a single coherent research project. Since heavily funded researchers with several grants were getting increased scrutiny, he would have to justify the funding of his lab by both NSF and NIH, clearly separating his work on one species from his work on the others.

Crews's proposal was necessarily a long one, with more than ninety pages of texts and detailed lists of experiments and procedures. He took about four months, spending two nights a week, to write the first draft that he would circulate to his research group. He began with his earlier successful proposal and reviewers' comments on it, the NIH guidelines, a list of topics he wanted to include, some boilerplate (the technical writer's term for material that can be reused on many proposals) on materials and methods, and several of his recent review articles. For the main part of his proposal, the prospectus of experiments, he drew on summaries each of his assistants had written, describing their current work and plans. A letter he had recently written in support of his career award contained arguments on the benefits of his work to humans; this proved helpful in drafting the section on "Significance." After about two dozen drafts (done using a text-processing program from his handwritten revisions), he gave a version to his research group. He included the guidelines and reviews of his earlier proposal, since he considered the proposal-writing process part of the education of the postdoctoral and graduate students working with him. He explained the competitive situation: "This has got to be an orgasmic experience for a reproductive biologist."

The Writer's Persona and the Literature of the Discipline

The instructions for writing the body of the proposal included with the NIH application emphasize the panel's concern with ethos and pathos—the character of the writer as researcher, and the interest of his or her work to other researchers:

> Organize sections A–D of the research plan to answer the following questions. (A) What do you intend to do? (B) Why is the work important? (C) What has already been done? (D) How are you going to do the work? (Application)

Crews and Bloch are especially concerned with questions (B) and (C), defining their personae as researchers and the relation of their planned work to the literature. Both these criteria involve contradictions. The form of scientific reports, the syntax of scientific prose, and the persona of the scientific researcher all work against self-assertion. And the definition of scientific importance requires both that the work be original and that it be closely related to the concerns and methods of current research. We shall see these contradictions presented and resolved in the course of the authors' revisions.

The number of revisions each writer made is remarkable considering that the first draft I studied, in each case, was itself the result of many drafts. The number of drafts means little, when the writers are using word processors, but in the five versions of Crews's proposal and the four of Bloch's that I studied, there averaged five to ten large or small changes on each page of each draft, and hardly a sentence remained unchanged over the course of revision. I interpret these changes as improving the readability, defining the relation to the discipline, and modifying the persona.

One kind of revision we might expect to see, besides these three kinds of revisions, is substantive changes in the research proposed. But neither of these authors significantly altered his plan of work to counter possible criticism. Other NIH applicants do add to, delete from, or modify their methods sections, especially if they have gotten detailed criticisms on their pink sheets (pink sheets are the summaries of the study section's evaluation of the proposal, sent out after the decision). That Bloch and Crews did not revise their methods may indicate their relations to the specialties of the study section members, relations in which they are quite different from each other. Bloch's research is so unusual and so isolated from the mainstream that he got little detailed criticism. Reviewers suggested some statistical tests, which he then used, but their own work in microbial genetics seemed to give them little specific to say to help Bloch with his broad evolutionary questions. Crews, on the other hand, had been working for five years on the project for which he was requesting continuing funding, and had fifty or so pages of detailed descriptions of experimental work in progress; his specific methods had already proven themselves to the study group members, and if questions were to be raised, they would probably be questions of logistics and management rather than experimental design.

Many of the changes for readability would have been suggested by any editor. Both writers had served on grant panels, and had learned from the experience of reading piles of proposals that, in Bloch's words, they have to "get the idea across efficiently," and in Crews's words, "they have to be made exciting." For instance, Crews wants his sentences to "flow," so he deletes such unnecessary words as *causal agents* in the phrase, "Disorders of human sexuality are causal agents responsible for. . . ."[3] Both authors cut jargon wherever they

3. Often the biologists gave a different interpretation of a change than I did. This shows, not that they were right and I was wrong, but that the interpretation depends on the point of view, knowledge, and purpose of the reader, as well as the motivation of the writer. I should note that the biologists also pointed out a number of mechanical errors and stylistic problems in the writing teacher's writing.

See Charles Cooper and Lee Odell, "Considerations of Sound in the Composing Processes of Published Writers," *Research in the Teaching of English* 10 (1976): 103–15, on the influence of such considerations on professional writers.

recognize it, so Crews changes *low temperature dormancy* to *hibernation*. A reader criticizes Crews's use of the term *therapy*, which implies he is doing the lizard a favor with these injections of hormones; Crews substitutes the more neutral term *treatment*. Both authors are cautious with neologisms, so Bloch, having apparently coined the term *foward complementarity*, changes it to *reverse complementarity* when a reviewer is confused. Both authors correct, with the help of their readers, dangling participles, faulty parallelism, and the like, though neither they nor their readers would identify these errors by these names.

The important studies of funding decisions by Cole, Cole, and Rubin (see note 1, earlier) take the applicant's relation to the discipline, the status in the research community, as given, as already determined by institution, publications, citations, and previous funding. And the writer cannot do much, in writing a proposal, to change these facts, the most powerful arguments for his or her competence. But the tone of almost every sentence of a proposal can be revised to show that one is cautiously but competently scientific. Often, because of the contradictions of self-assertion in scientific prose, the most effective means of defining one's place is understatement, toning down, not one's claims for one's research, but one's language. In an earlier draft Crews questioned the received idea that "courtship behavior . . . is dependent on androgens"; later he rephrases this idea as, "courtship behavior . . . might depend on androgens." He must be particularly careful about claims of priority. He changes "the implications of this observation have been unappreciated" (which suggests that he was the first to grasp these implications) to " . . . have not been fully appreciated" (which only suggests that there is more to say about them). Asked about this change, he says that the assertion of "total originality" is "sure death" with the review committee. One of the ways he defines his place in the community is by his choice of research animal, so he must be extremely cautious on anything relating to this choice, even in apparently innocent comments on lizards. He changes the phrase "More is known about the green anole lizard than about any other reptile," which could only tempt fans of other species to object, to "A great deal is known. . . ." He must be especially cautious in using the findings of other fields outside his area of research, for instance, those of clinical research on humans. He adds the cautious note to the statement that "sexual experience appears to be the most important factor" in human sexual function, because he thinks a more definite statement, though supported by his reading, "could have gotten nailed."

Bloch also strengthens his argument by backing off from his claims, in ways that are more interesting rhetorically than scientifically. One ratio is followed, in the first version, by "We proposed that. . . ." The ratio was questioned by some reviewers of the article; the explanation of it in a later version begins, "One interpretation would be that. . . ." One of his bolder objectives in the first proposal was to "determine, if feasible, the rates of evolutionary divergence and . . . approximate time of synthesis." But this was criticized by a panel member as a "notoriously difficult" project. The later version says, more cautiously, that he would "use the reconstruction as a guide in studying the early evolution of the coding mechanism," and he refers to "the distant goal of reconstruction." In general, later versions present the interpretation suggested by his model as one hypothesis among several others.

The revisions do not, however, show that the meek shall inherit the grants. As both authors temper their claims, they also assert their authority in their specific areas of research and point to their previous accomplishments. Often this change means just a shift from passive to active voice. Crews changes "mechanisms are revealed" to "I have been able to reveal," and "New light will be shed. . . ." to "I will shed new light." Similarly, Bloch adds paragraphs on data gathered "using a program written in this laboratory." He changes "the finding of increased numbers of homologies" to "our finding, in nearly half the searches. . . ." This change emphasizes the success of the project so far and emphasizes what his own lab has contributed, even though it has not been funded to do its own sequencing (the experimental determination of the order of bases on the nucleic acid). As part of this self-assertion the writers sometimes go out on a limb. Crews adds the loaded phrase "I predict that . . ." before a claim, showing that his hypothesis is, in Karl Popper's term, falsifiable. Apparently this risky language is expected at certain points; Bloch's proposals are full of such explicit predictions and are praised for being "testable."

Perhaps the most powerful component of self-presentation is the tone of the proposal, the persona the author creates in stylistic choices. Tone is not easily traced in textual terms, but clearly both authors are concerned with sounding scientific as well as being scientific. For example, Crews explains a change from "highlighted" to "shed new light on," which was mystifying to me, by saying that the first expression was "too catchy—sounds unscientific." Bloch makes a change in tone when he refers to the object of his search as "an early precursor to both molecules," tRNA and rRNA, rather than as a "primordial molecule," a formulation he had used earlier which suggests more strongly his concern with the origin of things. Interestingly, they both allow themselves to vary their subdued tone when revising sections on the implications of their research. Bloch ended the last version of this proposal with a paragraph on broadly suggested "spinoffs." Crews added to his introduction a paragraph of data on the effects of the stress of concentration camp life on women's menstruation cycles, data he had used in an earlier letter showing the relevance to humans of work on environment and sex hormones in animals. As he explained it, this addition, with its social and emotional weight, was made to support his technical argument. "I wasn't going to use it," he said, "because everybody uses it, but when I reread it, I saw that it was making a valid point about *extreme* stress."

The first major section of the application for an NIH grant must show the significance of the research proposed. But *significance* only has meaning in relation to the existing body of literature of the field. Thus there is a tension in defining one's claim; it must be original to be funded, but must follow earlier work to be science. These writers find their place in the community by making their texts fit in in two ways, with their citations and with their terminology.

In both writers we see a rhetoric of citations, though they use these citations in different ways, Bloch usually demonstrating his familiarity with the latest work in the field, Crews highlighting his own contributions. Bloch does cite his own articles, at whatever stage of review they have reached as he writes, and he attaches a manuscript as an appendix. As he accumulates data, he is able to refer more often to his own studies. He does not usually cite authors to refute them, but to show that he is aware of parallels and contributors of data to his own work. Neither does he cite articles to establish a theoretical base, an authority for his own approach; the only major cited contribution to his method is a program and a database from Los Alamos. Many citations are tactical. The most hostile referee of an early version of one of Bloch's articles (examined in detail in chapter 3) compared Bloch's model to that of Manfred Eigen, and the editor of the journal that accepted it compared the model to that of W. M. Fitch. Bloch cites Eigen and Fitch, both major figures in the study of molecular evolution, in a later version of the proposal, taking the opportunity to show the difference between their approaches and his. This strategy seems to have paid off; one panel's summary of an intermediate version of the proposal says, "The authors have considered alternative explanations and designed their analyses accordingly."

When Crews adds citations to those in his early draft, they are usually to his own work. For instance, he expands his assertion that estrogen "plays a critical role in yolk deposition" into a two-stage description of the depletion and production of vitellogenin, bringing in more references to a successful line of previous work. This sort of change is not made just to display his productivity; his output is obvious enough from the five-page list, required by the proposal format, of publications by his group that are related to the grant. He is known mainly for his laboratory and field work, and he cites this work to support certain theoretical views, he says, "to make a point, to associate myself with these perspectives." There are risks to this approach; a critical referee of one of his articles notes disapprovingly that most of the data supporting the theory are his own. But this may just show that the rhetoric of citations in a review article, which claims to speak for the entire research program or subspecialty, must be more circumspect than that of a proposal, which is expected to give some coherence to one's own previous work. Crews's problem as an established researcher is, then, the opposite of Bloch's as a new researcher; he must interpret his empirical work to associate himself with a new theoretical line, whereas Bloch must present his untried theoretical approach as potentially productive of new data. One cites himself, one cites others, but both are trying to insert new work into an existing literature.

The addition or deletion of terms with meanings or connotations specific to a discipline may be another, more subtle way of indicating one's place in the community. I have noted that both scientists cut jargon wherever they recognize it, but they also add or change some loaded terms. A reviewer of Bloch's earliest proposal says, "Most laboratories that *do research* with either tRNA or rRNA are already analyzing not only homologies but *real* structure-function correlates" (emphasis in the original). The implication is that Bloch's researches are not research (perhaps because he is using published data), that his correlates for molecular structures are not real (because they are selected to study evolution, not to study the biological functions), and that, as the reviewer continues, "the homology results are an offshoot of the main business." After this, if not because of this, Bloch is careful to relate his homology work to the "main business" of sequencing research, to account for the possibility of convergent evolution (which would fit better with this "main business"), and to use prominently the word *function,* even though origin, not function, is his main concern.

In the latest version of the proposal, Bloch makes another significant change in terms; for almost every occurrence of the word *homologies,* the central term of his project, he substitutes *matching sequences* or some equivalent. As we shall see in studying his articles, he stumbled onto the problem, common in interdisciplinary work, of a term that has a more restricted meaning in one field than in another. In molecular biology, as the reviewer's usage quoted in the previous paragraph shows, the word indicates any structural similarity. In evolutionary biology, the word can indicate only those structural similarities that result from common origins. If Bloch used the word in this sense while trying to *prove* common origins, he would be begging the question. But precision is not all that is at issue here; the change is part of the consensus-making process of proposal-writing. To use the word in the more restricted sense it has in evolutionary biology rather than in the broader sense it has in molecular biology is to acknowledge, or assert, that one's work will fit into both disciplines.

Crews's many changes in diction suggest how meanings may vary between members and nonmembers of a discipline. Thus minor revisions improving precision can be seen as part of the adaptation of the writer's style to the literature of the discipline. One zoologist finds his use of *cycles* rather than *phases* jarring in a certain context, and she draws a distinction between them; Crews responds by changing his terminology throughout. She also points out the vagueness of the phrases *behaviorally inactive* and *sexual behavior* to an ethologist who must observe and categorize these activities. Crews substitutes phrases that have more specific meanings to an ethologist: *non-courting* and *courtship and copulatory behavior.* One of his changes shows, like Bloch's deletion of *homology,* the lines between disciplines or approaches. I had interpreted his substitution of reproductive *processes* for reproductive *behavior* as an attempt to describe his comprehensive approach more accurately by using a more general term. In fact, he says, the change is tactical. He believes that studies of behavior, especially field studies, are not being funded by NIH, whereas studies of physiology (which are what the words reproductive *processes* imply in this instance) are more attractive to them. What seems a minor revision relates to the changing fortunes of that notoriously loaded word *behavior* through the 1970s, and indicates the researcher's keen sense of the connotation of the word in various disciplines.

Changing Strategies of Presentation

We have seen that many of these writers' revisions affect their personae as researchers and relate their work to the literature and the discipline. If we look at successive versions of one short but crucial part of the proposal—Bloch's "Abstract" and Crews's "Specific Aims"—we can see how in the processes of writing and rewriting the writers respond to and develop consensus in the field. (See the texts in Appendix 1.) These carefully composed sections are the writers' chances to present the main purposes of their research programs without burying them in detailed methods and data; some reviewers may not read the rest as carefully, especially if the proposal is as long as Crews's. We shall see that, late in the revision process, Bloch and Crews come to opposite strategies of self-presentation. Bloch tries to play down the more "speculative" theoretical aspects of his program and emphasize the data he has collected so far, whereas Crews decides at last to emphasize the larger and more controversial implications of his study. Each shows, in his last version, a closer fit between his work, as he presents it, and his discipline, as he presents it. Both strategies shown in these processes of revision are attempts to deal with increased competition for health-related research funds by relating the proposed work to the consensus in the field.

We have seen that Bloch was criticized by reviewers for being too committed to his model, for being too speculative, and for wandering from the "main business" of structure–function studies. In the three versions of his abstract, we note that the model is first played down and then finally removed, that the accumulation of data is emphasized more than the larger implications, and that alternative explanations for the matches, including function, are given more consideration. The revision of the opening sentence reflects this change in strategy. In the first version he says, "A search is being conducted for sequence homologies"; the subject of the sentence is the author's action, and the tone, as in the last sentence ("An attempt is being made . . .") sounds merely hopeful. The opening of the second version is at once more impersonal and more confident; there he presents data from the research so far as posing a striking problem requiring solution: "Ribosomal RNA is peppered with tracts that are homologous with regions found among different transfer RNAs." The lively verb "peppered" suggests that these data are too insistent to be overlooked, and the reference to "different transfer RNAs" suggests a broad scope of data. In the third version this lively but still vague statement is replaced by a statement suggesting comprehensive and quantifiable findings from many species: "A large minority of tRNAs from all species of organisms studied have stretches whose base sequences are identical or nearly so to stretches found in rRNAs."

Bloch's accommodation of the discipline and his presentation of his work in terms of its consensus are apparent also in his revisions of organization and sentence structure. In the first abstract, the model occupies the central and longest paragraph. He immediately states that he is looking for evidence of common origins, not just explaining homologies; here he tips his hand and lets his critics see his larger program. The rest of the abstract is organized, logically enough, by the researcher's effort: theory, model, predictions. One methodological problem is evident in the gap between sentences 7 and 8; his data are on existing rRNA and tRNA, but he applies them to what he calls "primordial RNA." The fact that the data and the hypothesis must remain in separate sentences suggests he has not yet found the syntax to make the connection. This gap will prove to be important to reviewers.

The structure of the second version allows Bloch a longer discussion of the homologies (sentences 4–7) before he presents the model used to explain them; the focus is on the matches, rather than on the researcher and the theory, until "our work" in sentence 6. Now he mentions function as a possible alternative explanation for the homologies, and offers a test for convergence to determine its role. Still, he can only say at this point that this complicating factor "cannot yet be ruled out." The description of the model and prediction is tightened (sentence 10), giving it fewer words and less emphasis. The gap in the first version between present-day data and primordial hypothesis is not bridged but eliminated; here it is clear that the model only predicts homologies in present-day tRNA and rRNA, and needs no inferential leap into the past.

In the third version of the abstract, the model is not mentioned explicitly at all, though it is still implied in his analysis of the homologies. This version is organized by the sequence of ideas, rather than by the narrative of the researcher's efforts; it offers a sort of theoretical flowchart. Bloch says, more cautiously than before, that the homologies might be due either to function or to common origins. If function is the explanation, it might be either on the DNA or on the RNA level, and if origin is the explanation, it might be the result of either primordial or relatively recent conditions. Now the assertion of the ancient origin of these homologies is in the passive, and is after the data, so that the data, not Bloch, suggest it. The potentially troublesome statement that he is searching for ancestral RNA starts with a long noun phrase that may defuse some resistance, and uses hedging verbs: the overlapping and overlays "suggest" that further identification "should permit" reconstruction. Finally, whereas the first two versions end with this prediction, the third version ends by emphasizing "the correct functions of the transcription–translation mechanism." So for the Public Health Service he emphasizes the possible health applications, which were not mentioned in the earlier NSF versions.

Crews had also been criticized for favoring a "speculative" model that is inconsistent with much of current research, but in the revisions of his "Specific Aims" section, we see a strategy different from that of Bloch, a movement toward emphasizing his controversial model. This change was made in a very late draft, after many other changes, most of them to improve readability, had led to a draft he optimistically labelled the "final final final draft." In this draft he still cautiously plays down the model that proposes dissociated as well as associated reproductive tactics. A two-sentence introduction to his general field and specific interest is followed by a two-paragraph comparison of the green anole lizard to the red-sided garter snake. The model is subordinated to the unexceptionable comparison of two species that happen to exhibit these tactics. His own methods of investigation are not stressed. The third paragraph says that the difference in reproductive tactics has implications, but leaves those implications for the next section, where they are less prominent.

In the later version Crews highlights his more controversial approaches. The safe statement, "I will continue my study of two reptile species," is replaced with a sentence beginning, "The general objectives of my research are . . ." that introduces immediately the ecological views disputed by some reviewers. Further sentences in the first paragraph emphasize his distinctiveness as a researcher, as shown by his comparative approach and his combination of laboratory and field experiments. The second paragraph, which had been organized around the comparison of two species, is now organized around two reproductive tactics, further emphasizing his theoretical framework. He highlights the definitions of the terms he has coined by putting them in separate sentences (returning to the phrasing of a much earlier draft, written before he had started downplaying the newness of his work). No specific species are mentioned yet; the lizard and the snake are introduced only in the last paragraph, as "one representative species of each reproductive tactic." His "goal is to compare the two tactics," to look for broad knowledge of mechanisms rather than just specific knowledge on one or two species. He emphasizes the "broad approach" and the search for important generalizations. The concluding sentence of the earlier version had put direct, immediately applicable findings first, with fundamental concepts in the second part of the sentence; here it is the direct findings that come after the "also," in the position of secondary importance to the fundamental concepts.

Though the two researchers follow different tactics, they both try to relate the proposed work to the consensus in the field. Bloch saw that his proposals and articles were getting more favorable review as he gathered more data and discussed alternative explanations. Thus he presents himself as a new but well-informed and cautious member of the existing RNA sequencing program, and plays down wherever he can what he feels are the controversial aspects of his project. He need not insist on the newness of his thesis; its boldness will be apparent, to anyone likely to accept it, from the striking tendencies in the data collected so far. But he is not just persuading the panel and the discipline with these tactical changes; his reviewers are persuading him in some ways as well. Since he must discuss the alternatives to his model, he becomes more involved with structure-function relations, if only to dismiss their influence here, so the context of his research is changed by the process of applying for funding.

Crews's last-minute revisions may seem to indicate a strategy of defiance of the consensus of his subspecialty, just as the previous version seems to indicate a tactical appeal through the less controversial elements of his research. But these changes may also be seen as part of a consensus-making process, one that goes beyond the boundaries of the subspecialties of herpetology and classical neuroendocrinology to include an audience of comparative biologists and evolutionary theorists. To put this strategy in more practical terms, he may have reasoned that if only about 5 percent of the proposals to this panel were to be funded, no amount of interesting new data on anole lizards and red-sided garter snakes would be considered worth funding if it just supported existing models based on other species. If he stuck to the consensus, he might not be criticized, and might even get favorable comments from the reviewers, but he wouldn't generate enough enthusiasm to get him across what the reviewers call "the payline," the priority score cutoff for funding. He would have to present a bold idea, and present himself as a researcher capable of a uniquely broad and ambitious project. He knew, after a few hostile reviews of his related article, that in taking this approach he risked a rejection if the panel was persuaded by one of his critics. But that risk was apparently preferable to cautious dullness.

My study ends with these submitted versions of the proposals, since I am interested in how the researchers write the proposal, not in how the decision to fund is actually made. But the decisions, in this case, support the researchers' senses of appropriate strategy. The "pink sheet" summarizing the decision on Bloch's application shows the study section members were intrigued by the homologies he pointed out, but were still suspicious of his advocacy of a model attributing these homologies to a common ancestor. The summary says he needs to consider critically the other possibilities, especially convergence due to function. So he has not convinced them that his work gives sufficient attention to the work being done on structure–function relations. The major criticism of the proposal, though, is that it lacks a sufficiently detailed theoretical framework, specifically an explanation for how he will relate the present-day homologies to the ancestor molecule–how he will cross that gap noted in the structure of the first summary. This too can be interpreted as an indication that Bloch still stands outside the consensus of the subspecialty; he is being told he has not demonstrated a theory that both takes into account current concepts and also allows him to go beyond the current line of work. Though Bloch's proposal was not successful, the strategy of downplaying the model and emphasizing his awareness of structure-function studies seems to have been the only strategy that would have had a chance. Bloch's comment was that he would have to "talk to them through more publications"; that is, he would have to establish himself as a known contributor to the field before applying again. And his eventual success in getting published (described in the next chapter) seems to have helped; after an article appeared in *PNAS*, he received some funding, not from a government agency, but from two private Texas-based foundations.

Before the decision on Crews's proposal was reached, the study section scheduled a site visit at his lab to observe its work. Such a visit illustrates the consensus-forming function of the proposal process. Site visits can be scheduled by the Executive Secretary of the section (the NIH administrator) to resolve differences or doubts on the panel; they are usually made in cases of applications that are close to being funded. In some cases, the fact of the site visit would indicate a seriously split panel trying to reach some sort of agreement. But Crews's interpretation is that the administrator thought that some members of the panel were just unenthusiastic about the proposal, so that it might not get the very good priority score necessary for funding by NIH under current budget conditions. If this were the case, his strategy of emphasizing the broad implications of his work was probably wise, because the panel's conception of him as an ambitious researcher turned out to be more important than their awareness of his controversial relation to his research community.

The site visit was, in a way, a second proposal, this time presented orally, with the lab itself as the most persuasive illustration. Crews prepared by going over his proposal carefully with a number of colleagues, and he planned to temper somewhat the tone of the submitted version of the proposal. He said that he didn't want them to think he was claiming to have the last word on the relations between hormones and sexuality. As it turned out, he spent most of his presentation demonstrating that his lab was capable of such a large project. He showed a very detailed notebook of experimental prospectuses drawn up by his assistants to demonstrate his careful quality control. He emphasized the lab's publications over the last five years to demonstrate it had the capacity to handle so many projects. There was no arguing over controversial theories. Though he knew beforehand that one of the visitors would be a critic of his approach, he wasn't even sure afterwards which one this was. In the visit, as in the written proposal, persona and relation to the literature and the discipline are crucial. Crews consulted with colleagues, adjusted his tone, prepared still more textual evidence to present himself as a competent researcher and as an accepted contributor to the literature, all to enable this group to come to an agreement within itself. If his proposal had been rejected because of opposition by one powerful reviewer, this view of proposal writing as a consensus-making process would be meaningless. Instead we see still more mechanisms to allow the researcher to shape his or her persona and to make the decision representative of the subspecialty as a group.

In the end, the whole evaluation process was caught up in much larger fiscal decisions. According to Crews, a modification of accounting procedures, part of David Stockman's new budget in 1985, meant that the whole cost of a three-year project like the one proposed by Crews would have to be assigned to the first year of the project, instead of being spread over the accounts for three years as in the past. This effectively cut by two-thirds the already small number of proposals that could be funded that year, and eliminated most long-term proposals like Crews's in that round. But his lab continued to operate with its NSF funding and his own funding from a Career Development award. The grant was finally renewed, for three years, and it continues to fund part of Crews's research. He applied for a five-year competitive renewal in October 1987.

In my textual analysis, I have been showing the politics involved in the smallest details of the writing of funding proposals, but the result, based as it was on funding constraints rather than on the decision of the panel, shows how the effects of big political changes reverberate through science. There may be a lesson in this for those of us analyzing these detailed case studies, doing what is called, in social science jargon, "microsociology," reminding us that the larger institutional and policy processes analyzed by earlier students of science and society continue to operate. We have conducted a polemic to show the social in the scientific, but the social and political aspects of science are all too apparent at a time of funding cuts. There is much to be learned in studies of laboratories, but at some point, as Bruno Latour reminds us, we must break out of methodologies that assume that "science stops or begins at the laboratory walls" ("Give Me A Laboratory," p. 168).

The Uses and Limits of Rhetoric in Proposals

I have argued that the proposal-writing process shapes both the writers and, to a lesser degree, the discipline. The writers, who were doing work they saw as being on the boundary of two fields, moved toward a presentation of themselves as good members of those fields, and presented their work in terms of its interest to other researchers who might tend to reject it. There is a tension in both lines of argument. As we have seen, self-presentation requires a difficult balance—not too meek, not too assertive—that cannot always be reached by studying some generalized portrait of the good scientist. The image seems to depend partly on the type of research proposed. Both these researchers decided to present themselves in ways we might not expect. The researcher who wants to verify his model of the origin of life presents himself as the skeptical servant of the mountains of data printed out by his computer program. The researcher who wants to spend five more years in painstaking studies of thousands of snakes and lizards presents himself as a theoretician studying a new conceptual framework. There is a similar tension in their attempts to present their work as interesting, for they must show that it is original and yet entirely in accordance with the existing discipline. So they use citations, or significant vocabulary, or on occasion directly claim they can make a contribution. But here too they are limited; for instance, words like *new, fundamental,* and *important* are all but forbidden, and even *interesting* seems to provoke some readers. Claims of originality are risky, and criticisms of opposing views can seldom be explicit. Both authors wrote letters defending their work against the criticisms of hostile reviewers; comparing these to their proposals one sees how careful they were with tone in the formal proposal. When decorum is no longer demanded by the proposal format and the evaluating audience, they are unabashedly enthusiastic about their projects. They did not lose the sense they had at the beginning of being in hot pursuit of the secrets of life, though in their proposals they conceal their excitement.

But I have argued that the contexts of their projects were changed by the process, even if their enthusiasm was not. Bloch, as I have pointed out, reoriented his studies to provide mathematical analyses of the possibility of function accounting for the homologies he observes. His research was no longer just a proof for his ideas on the origin of life; it was now also a fairly elaborate method for comparing structures of molecules. Crews made no such methodological changes, but he has to think, whenever he writes a proposal, about what his work can contribute to fairly distant lines of research on other species and about how his theoretical models relate to those used by most researchers. Finding conventional terms for unconventional research is not just an exercise in rhetoric—it changes the research.

Of course the success or failure of the proposal also changes the research. But funding does not always determine if a research program continues. While Bloch searched for funds, he continued to write articles about tRNA-rRNA homologies, but followed a line of work that required less money, analyzing the published data for significant patterns. He did not get a postdoctoral student with whom to develop the theory, but as we shall see he found a collaborator in, of all areas, statistical mechanics, who was interested in developing the mathematical description of these homologies. A number of researchers have responded this way to cutbacks, moving into less expensive lines of research, but not abandoning the research program altogether. When Crews was not funded by the NIH in this round, it meant some cutbacks in the lab, but he had other grants for other projects, so it was not a question of sending the postdoctoral and graduate students, and the snakes and lizards, home.

The proposal process also changes the field in a more fundamental way, by challenging the terms in which the subspecialty defines itself.[4] Both these researchers saw themselves as working at the edge of a specialty or on the border between two subspecialties; Bloch talked about "the establishment" in molecular biology, and Crews referred to the "prevailing paradigm." When the study section gives a proposal like one of these a priority number below the payline, they draw the line that marks the edge of their specialized field. When the study section approves such research, it redefines that line. To a large degree, both researchers accepted the assumptions and criteria by which

4. See Michael Callon, "Struggles and Negotiations to Define What Is Problematic and What Is Not: The Socio-Logic of Translation," in *The Social Process of Scientific Investigation*, eds. K. Knorr, R. Krohn, and R. Whitley (Dordrecht: Reidel, 1980), pp. 197–219.

this decision is made; they disagreed with the panel only about how these criteria applied to their own work. Since they were part of the system, we should ask, not whether the system is "fair" to individuals, but how it serves the scientific discipline.

Representative Conlan, whom I quoted earlier, is not alone in asking whether the peer review process "stifles new ideas." A favorable reviewer of one of Bloch's proposals concluded, "Provocative ideas are always in short supply, and there is truth to the criticism that the present funding system often fails to nourish them." But the funding system exists to select as well as to nourish, and here the powerful consensus that Representative Conlan calls "incestuous" may serve to stabilize the economy of the discipline. For example, to approve either Bloch's proposal or Crews's would be to define large new research programs, beyond what these individuals propose, to study the origin of primitive RNA through homologies in present-day tRNAs and rRNAs, or to look for relations between hormonal cycles, mating behavior, and ecological factors in a wide variety of species. Such redefinitions of a field require changes in careers and institutions, and are enormously costly in time and money. In some cases, such as the line of neuroendocrinology Latour and Woolgar have studied, such costs may prove to be worthwhile. In this case, some reviewers might argue that there is too much left to be done on conventional structure-function studies, or on hormonal studies based on the simpler paradigm, for attention to be diverted to other lines of work, even if these other lines of work turn out someday to be important. If husbanding of resources for a consistent line of work is a function of funding decisions, it is not surprising that the proposals focus on who the writers are, whether they can do what they say, and whether, if they do it, they will have much effect on other researchers, or on problems that are important to a wider public audience.

If the rhetoric of the proposal will vary with each discipline and with the writer's relation to the discipline, it is not given by some ideal list of persuasive or communicative techniques, or by an ideal scientific persona, or even by the characteristics of the project itself. Thus, the cautious tone adopted by Bloch, appropriate for his situation as a newcomer, would be disastrous for Crews, who is well established in his specialized field. Scientists learn the rhetoric of their discipline in their training as graduate and postdoctoral students, but they relearn it every time they get the referees' reports on an article or the pink sheets on a proposal. Bloch learns where his data get a good response; Crews finds how his assertions affect a researcher who works on mammals. Finally, the researchers themselves come to assume most of this knowledge of the discipline as something natural. But we need to make it explicit and conscious to open it to people outside the discipline. As I will argue, the public needs not only to understand the facts of science, but to understand the way those facts are made.

I have focused in this chapter on the most obviously rhetorical genre of scientific writing; by implication, I am saying that the rest of the process of producing knowledge can also be seen in terms of the forms of texts. The rest of this book is devoted to what might be considered later stages in the production of a knowledge claim, as scientists address various audience in various genres. To receive credit scientists must publish claims in journals where they will be read by other researchers who will cite them (chapter 3). To organize a consensus around the claim they must address the criticisms of other researchers within the core group, either informally and implicitly or, in the case of controversies that reach print, formally and explicitly (chapter 4). Scientists write popularizations to reach beyond the small circle of specialists working on related problems (chapter 5). And ultimately, these claims can become a part of the general culture, as accepted facts about nature, or may be rejected as the notions of a small group of specialists (chapter 6). It is at this last stage in the life of a fact that there can be controversy about the interpretation of biological research in other discourses, and about the significance of these interpretations for the life of the community.