

Case studies of two proposals for research funding serve as examples of how scientific texts are the products of a community of researchers. Comparisons of successive versions of the proposals show that the two biologists, in revisions of their texts, alter their personae and their relations to the literature of their fields. In writing and rewriting, they both respond to and develop a disciplinary consensus.

The Social Construction of Two Biologists' Proposals

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Two anthropologists who studied the researchers at the Salk Institute describe the scientists and technicians there as “a strange tribe who spend the greatest part of their day coding, marking, altering, correcting, reading, and writing” (Latour & Woolgar, 1979). Literary critics describe some of the texts of the scientific tribe, but only those that seem literary; they approach this tribe as collectors of primitive art. Most rhetorical and linguistic studies of scientific writing focus on the individual and treat the process as another variant of a general writing process; it is as if an anthropologist treated every tribe’s religious rituals as a debased form of Episcopalianism. Sociologists of science, on the other hand, give us ways of talking about the structure of the discipline as a whole, but they seldom discuss texts; it is as if an anthropologist ignored the artifacts of the tribe as of little interest. I would like to read two scientific texts as products of a community of researchers, to see what they can tell us about composing as a process of social construction (Knorr-Cetina, 1981; Gilbert & Mulkay, 1984).

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My examples will be grant proposals, the most obviously rhetorical writing scientists do, and the writing that has the most immediate effects on the structure of the discipline.

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. 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" (Cole, Rubin, & Cole, 1977), and Michael Kenward quotes an observer who says it is like a murder trial with "a jury of axe murderers from the same gang" (Kenward, 1984). 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 (Cole et al., 1977; Cole and Cole, 1979; Cole, Cole, and Simon, 1981; Van den Beemt and Le Pair, 1983). Both the criticisms and the studies have focused on what happens in the funding after the proposals are submitted and have asked what role factors besides "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 (NIH, 1983). 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 process essential to the development of scientific knowledge. To use the metaphor that the critic Kenward quotes, it brings the axe-murderers into the gang.

Most researchers would grant that proposal writing is a rhetorical activity in which the writer seeks a strategy for persuading a reluctant audience. There is a paradox here because the proposal format, with its standard questions about background and goals and budget, and the scientific report style, with its passives and impersonality, do not allow for most types of rhetorical appeals; one must persuade without seeming to persuade. 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. In textual terms, one describes the work so as to create a persona and insert the work into the existing body of litera-

ture. 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 context. The process of writing a proposal is largely a process of 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.

METHODS

I have collected all major drafts of proposals by two biologists working on two controversial research programs. 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. The proposals were part of a web of other writing, so I also collected drafts of articles on the research proposed, and comments on them. 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, and so on), sometimes following handwritten changes on the drafts, but usually comparing one printout to the next. I categorized changes by what seemed to motivate them and noted especially those 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 will see, there were few such changes. In addition to these revisions, they 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 commenters to interpret them. 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.¹ After assigning categories, I interviewed the writers about my inter-

pretation of selected revisions, and also had them check, at various stages of my writing, the views in the essay as a whole.²

THE WRITERS

The researchers I studied are, in some respects, representative of biologists at large research universities: They both supervise laboratories and have published many articles, and they both have received grants in the past and have reviewed grant applications themselves. But I chose to work with them partly because they were atypical: They were presenting work based on new models that put them between two well-established subspecialties of research. Judging by the response to articles presenting these models, their work is controversial. The fact that they assumed resistance to their proposals may have made them more self-conscious about their writing processes, and certainly made the rhetorical features of their proposals more apparent to a nonspecialist. In the proposals I am studying, they faced quite different rhetorical problems: One was taking his well-known and respected research in a new theoretical direction; the other was entering a new field in which he was unknown.

For David Crews, an Associate Professor of Zoology and Psychology at the University of Texas at Austin, the problem was getting continuing support for his large and productive laboratory during a period of very selective funding. Dr. Crews came to the University of Texas two years ago after receiving a Ph.D. at Rutgers, a post-doc at Berkeley, and teaching at Harvard. He does lab and field studies of the reproductive physiology of reptiles, and is author or coauthor of about 15 papers a year. His lab, with two post-docs, several graduate students, and half a floor of a building, is now funded by grants from NSF (for one species), NIH/NICHHD (for two other species), and an NIMH Research Scientist Development Award (for him). I studied his proposal for a competitive renewal, after five years, of his current NIH grant.

Although Dr. Crews 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. While he was writing his proposal (September 1982 to July 1983), he was also giving talks and he submitted an article on the theoretical model of the relation between hormones and behavior that guides his research (Myers, 1984). Thus

he knew that the model was controversial, at least in some quarters. Reviewers of the article were either very enthusiastic or very critical, and he could not afford even one critical review of the proposal. He would either have to face or to sidestep this resistance. Also, the increased competition for federal funds (Mandel, 1983; NIH, 1983) 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. Because heavily funded researchers are 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.

Dr. Crews's proposal is necessarily a long one, with more than ninety pages of text and detailed lists of experiments and procedures. He took about four months, spending two nights a week, writing 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, because he considered the proposal-writing process part of the education of the post-docs and graduate students working with him. He explained the competitive situation: "This has got to be an orgasmic experience for a reproductive biologist."

For David Bloch, a Professor of Botany, the main problem of the proposal was to demonstrate his competence in a specialty different from that for which he had been funded in the past. Dr. Bloch studied at Wisconsin and Columbia and taught at UCLA before coming to the University of Texas in 1961. Most of his published articles are on cell biology, most recently on flow cytometric studies. His lab is supported

by an NIGMS grant and an NSF grant for these studies. But since 1981, he has also been working in a quite different field, identifying homologies in rRNA and tRNA for indications of the evolution of nucleic acids. As these studies developed, he applied to NSF (twice) and to NASA for support, not getting funded but apparently getting much closer. Now he is applying to the Public Health Service and reapplying to NSF.

To some reviewers, Dr. Bloch seemed to be a latecomer to the biology of nucleic acids, but his interest in them has a long history. As a graduate student, he, like so many other molecular biologists, was fascinated by Erwin Schrodinger's *What is Life?* A back injury thirty years later gave him time for reflection and reading in this area. He offered a graduate course in what he calls "The Evolution of Evolution," in which he and his students devised what seemed to them to be a plausible model for the origins of nucleic acids. He set out to test this model, using published data and a computer program written by one of his graduate students, and worked without outside funding except university grants for computer time and a semester off from teaching.

Dr. 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 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 recently, his articles in this area could be listed only as "submitted" (Bloch, 1983). He had a clear enthusiasm for one model, which left him open to charges of a premature drawing of conclusions from insufficient data. Dr. Bloch was well aware of his rhetorical strengths and vulnerabilities from the responses he had gotten to his papers at conference, to the articles he had submitted for publication, and to his proposals to various agencies. Like Dr. Crews, he was acutely conscious of increased competition for research funds. Dr. Bloch's proposals have the same sections as Dr. Crews's, but because he does 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 contribute comments and criticism, but he is entirely responsible for the writing. He has now

been working on the project for more than two years, with countless drafts (on a text processor) of four articles, and five submitted proposals, in addition to applications to the university. Later versions of the proposal did not go through as many drafts, but sections within them changed as he accumulated more and more data in his sequence searches and as he responded to or adapted to criticism. The proposals did not grow larger. As we will see, the increasingly detailed discussions of data that he had gathered substitute for passages that described the model.

THE WRITER'S PERSONA AND THE DISCIPLINE'S LITERATURE

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? (NIH, 1984)

Dr. Crews and Dr. Bloch are mainly concerned with questions (C) and (B), defining their persona and their relation 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. 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 will 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 Dr. Crews's proposal and the four of Dr. 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. Many of the changes 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 Dr. Bloch's words, they have to "get the idea across efficiently," and in Dr. Crews' words, "they have to be made exciting." For instance, Dr. Crews wants his sentences to "flow" (see Cooper and Odell, 1976), so he deletes such unnecessary words as "causal agents" in the phrase, "Disorders of human sexuality are causal agents responsible for . . ." Both authors cut jargon wherever they recognize it, so Dr. Crews changes "low temperature dormancy" to "hibernation." A reader criticizes his use of the term "therapy," which implies he is doing the lizard a favor with these injections of hormones; Dr. Crews substitutes the more neutral term "treatment." Both authors are cautious with neologisms, so Dr. Bloch, having apparently coined the term "forward 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 they do not identify these errors by these names.

Another category of revision we might expect to see, besides revisions affecting persona, relation to the discipline, and readability, is that of substantive changes in the research proposed. But neither of these authors significantly alters 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 (the summaries of the study section's evaluation of the proposal, sent out after the decision). That Dr. Bloch and Dr. Crews do not may indicate their relations to the specialties of the study section members, relations in which they are quite different from each other. Dr. Bloch's research is so unusual and so isolated from the mainstream that he gets little detailed criticism. Reviewers suggest some statistical tests he uses, but their own work in microbial genetics seems to give them little to help Dr. Bloch with his broad evolutionary questions. Dr. Crews, on the other hand, has been working for five years on the project for which he is requesting continuing funding, and has fifty or so pages of detailed descriptions of experimental work in progress; his specific methods have already proven themselves to the study group members, and if questions are raised, they are likely to be questions of logistics and management rather than experimental design.

The important studies of funding decisions by Cole, Cole, and Rubin take as given the applicant's status in the research community, as determined by institution, publications, citations, and previous

funding. Additionally, 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 self-presentation is understatement, toning down—not one's claims for one's research, but one's language. When Dr. Crews questions the received idea that "courtship behavior . . . is dependent on androgens," he rephrases it 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" to "have not been fully appreciated." 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, even in apparently innocent comments on lizards (see Crews, 1975). 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."

Dr. 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. Dr. 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, Dr. 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, which has not been funded to do its own sequencing (the experimental determination of the order of bases on the nucleic acid), has contributed. As part of this self-assertion, the writers sometimes go out on a limb. Dr. 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; Dr. 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. Tone is not easily traced in textual terms, but clearly both authors are concerned with sounding scientific as well as being scientific. For example, Dr. 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." Dr. 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," 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. Dr. Bloch ends his latest proposal with a paragraph on broadly suggested "spinoffs." Dr. Crews adds 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 of work on environment and sex hormones to humans. As he explains it, this addition, with its social and emotional weight, was made to support his technical argument. "I wasn't going to use it," he says, "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 as in defining one's persona; 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. Dr. Bloch usually demonstrates his familiarity with the latest work in the field; Dr. Crews highlights his own contributions. Dr. 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 rather to show that he is aware of parallels and contributors of data to his own work. Nor 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 the article compared Bloch's model to that of Eigen, and the editor of the journal that accepted it compared the model to that of Fitch. So Eigen and Fitch are both cited in a later version of the proposal in a way that shows the difference between their approaches and Dr. Bloch's. 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 Dr. 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 his successful line of previous work. This sort of change is not done just to display his productivity; that is obvious enough from the section required by the proposal format using five pages to list four years of publications related to the grant by his group. 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 on 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. Dr. Crews's problem as an established researcher is, then, the opposite of Dr. Bloch's as a new researcher; he must interpret his empirical work to associate himself with a new theoretical line, whereas Dr. 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 they cut jargon wherever they recognize it, but they also add or change some loaded terms. A reviewer of Dr. 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 Dr. 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, Dr. Bloch is careful to relate his homology work to the "main business" of sequencing research. He is also careful 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, Dr. 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. He had 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 Dr. Bloch used the word in this sense while trying to prove common origins, he would be begging the question. The editor of the journal that published the article pointed out the ambiguity, and Dr. Bloch changed his terminology willingly, in the article and in later proposals, glad to restore some precision of meaning to the word. But precision is not all that is at issue here; the change is part

of the consensus-making process of proposal writing. The editor says, in asking for the change in usage, "Molecular biologists must also be biologists, too." To use the word in this more restricted sense is to acknowledge, or to assert, that one belongs to both the specialty of molecular biology and the broader discipline.

Dr. 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; Dr. Crews responds by changing his terminology throughout. She also points out the vagueness, to an ethologist who must observe these activities, of the phrases "behaviorally inactive" and "sexual behavior." Dr. Crews substitutes phrases that have more specific meanings to an ethologist: "non-courting" and "courtship and copulatory behavior." One of his changes shows, like Dr. 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—Dr. Bloch's "Abstract" and Dr. 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. These carefully composed sections are the writers' chance 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 Dr. Crews's. We will see that, late in the revision process, Dr. Bloch and Dr. Crews come to opposite strategies of self-presentation. Dr. Bloch tries to play down the more "speculative" theoretical aspects of his program and emphasize the data he has collected so far, whereas Dr. 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 Dr. Bloch is 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 (see Appendix), 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."

Dr. Bloch's accommodation of the discipline and his presentation of his work in terms of its consensus is 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, of course, on existing rRNA and tRNA, but he applies them to what he calls primordial RNA. That his data and hypothesis remain in separate sentences suggests that he has not yet found the syntax to make the connection. This gap will prove to be important to reviewers.

The second version gives a longer discussion of the homologies (sentences 4-7) before presenting the model he uses 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, 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. Dr. 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 Dr. 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 cautious 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." That is, he emphasizes the possible health applications, which were not mentioned in the earlier NSF versions.

Dr. Crews has 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 Dr. 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 labeled "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, Dr. 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. Dr. Bloch saw that his proposals and articles were getting more favorable reviews 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. Because 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.

Dr. Crew'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% 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 would not 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, because 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 Dr. Bloch's application shows that the study section members were intrigued by the homologies he pointed out, but were 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. Thus 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 a lack of 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 Dr. Bloch still stands outside the consensus of the subspecialty; he is being told that 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. Although Dr. 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. Dr. 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.

Before Dr. Crews's decision 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 Dr. Crews's interpretation is that the administrator thought that some members of the panel were just unenthusiastic about the proposal, so it might not get the good priority score necessary for funding by NIH under current budget conditions. If this is 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 turns 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. Dr. 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 knew that one of the visitors would be a well-known researcher on mammals who would be a critic of his theory. He said that he did not 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 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 its capacity to handle so many projects. There was no arguing over controversial theories; he wasn't even sure afterward which of the visitors had been his critic. In the visit, as in the written proposal, persona and relation to the literature and the discipline are crucial. Dr. Crews consults with colleagues, adjusts his tone, prepares 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 due to 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.

THE USES AND LIMITS OF RHETORIC

I have argued that the proposal-writing process shapes both the writers and, to a lesser degree, the discipline. The writers, who are doing work they see as being on the boundary of two fields, move toward a presentation of themselves as good members of those fields, and present 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 arrived at by studying some generalized portrait of the good scientist. The image seems to depend partly on the type of research proposed. Both these researchers decide 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 their work is original and

yet show that it is 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 have written letters defending their work against the criticisms of hostile reviewers; comparing these to their proposals, one sees how careful they have been with tone. When decorum is no longer demanded by the proposal format and the evaluating audience, they are unabashedly enthusiastic about their projects. They have not lost the sense they had at the beginning that they are 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 are changed by the process, even if their enthusiasm is not. Dr. Bloch, as I have pointed out, reorients his studies to provide mathematical analyses of the possibility of function accounting for the homologies he observes. His research is no longer just a proof for his ideas on the origin of life; it is now also a fairly elaborate method for comparing structures of molecules. Dr. Crews makes 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. Dr. Bloch continues to write articles about tRNA-rRNA homologies, but he is following a line of work that requires less money. He has no funding for laboratory work of his own in this area, so he continues to analyze the published data for significant patterns. He did not get a post-doc with whom to develop the theory, but he found a collaborator in, of all areas, statistical mechanics, who is 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. If Dr. Crews were not funded, it would mean cutbacks in the lab, but he has other grants for other projects to work on.

The proposal process also changes the field in a more fundamental way, by challenging the terms in which the subspecialty defines itself (Callon, 1980). Both these researchers see themselves as working at the edge of a specialty or on the border between two subspecialties; Dr. Bloch talks about "the establishment" in molecular biology, and Dr. Crews refers 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 it approves such research, it redefines that line. To a large degree, both researchers accept the assumptions and criteria by which this decision is made; they disagree with the panel only about how these criteria apply to their own work. Because they are 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 Dr. Bloch's proposals concludes, "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 Dr. Bloch's proposal or Dr. 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 to be important someday. 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.

If the rhetoric of the proposal is not given by some ideal list of persuasive or communicative techniques, or by an ideal scientific persona, or by the characteristics of the project itself, but instead depends on a complex process involving both the researcher and the discipline, then it will vary with each discipline and with the writer's relation to the discipline. Thus the cautious tone adopted by Dr. Bloch, appropriate for his situation as a newcomer, would be disastrous for Dr. Crews, who is well established in his specialized field. They learn the rhetoric of their discipline in their training as graduate students and post-docs, but they relearn it every time they get the referees' reports on an article or the pink sheets on a proposal. Dr. Bloch learns where his data receive a good response; Dr. Crews finds how his assertions affect a researcher who works on mammals. Finally, they 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.

To do this, I think we could borrow from Dr. Crews's work, observing the behavior of reptiles. He noted in the margin of my "Methods" section, "You're an ethologist." Like ethologists, we should not only observe and categorize the behavior of individuals, we should also consider the evolution of this behavior in its ecological context, and comparisons to behavior of other species in other environments. I agree with Dr. Crews that atypical subjects may provide the best means of reexamining received ideas about behavior, but I do not think we are ready yet for what Dr. Crews sees as "A Natural History of Grant Writing." This is not just because scientists are more complicated than lizards or garter snakes, or because they will not stay in glass cages until we can perform an assay, but because our language, institutions, and authority are intertwined with theirs. When Dr. Crews saw how I interpreted my data, he decided I was more of a "seat-of-the-pants ethologist." That, I think, is all one can be in the study of scientific writing now.

APPENDIX: TEXTS OF SUMMARIES

Dr. Bloch's Abstract

Version 1

(1) A search is being conducted for sequence homologies and for homologies of the reverse complementary sequences among tRNAs and rRNAs. (2) The

results of these searches are being compiled in order to compare the distributions of the sequences among different regions of the molecules.

(3) The purpose is to search for evidence of common origins of these classes of RNA.

(4) A model is proposed for the evolutionary origin of the protein synthetic mechanism that predicts a common origin of the different classes of RNA. (5) The model is based on a synthesis of a multifunctional RNA through a series of alternating syntheses: elongation through looping back, replication via templating, then repetition of the this cycle [*sic*], starting with a primordial tRNA with a simple anticodon region. (6) The result would be a molecule with extensive internal complementarity, dotted with codons and anticodons, capable of assuming configurations that would permit it to serve as message and structural RNA, and alternatively as gene.

(7) The model predicts extensive homologies between the primordial tRNA and rRNAs. (8) Homologous sequences in present day tRNAs and rRNAs are being found.

(9) An attempt is being made to sort out the relative importance of function and common descent as explanations for the homologies, by studies of the commonality of the homologies and their placements within and among the RNAs of the different classes.

Version 2

(1) Ribosomal RNA is peppered with tracts that are homologous with regions found among the different transfer RNAs. (2) The matches are too frequent and extensive to be attributed to coincidence. (3) Their distributions and patterns suggest a common evolutionary origin for the two classes of molecules. (4) Function as an explanation for their existence appears unlikely but cannot be ruled out. (5) Different domains have been conserved in different classes of organisms. (6) Our work will continue to identify examples of these homologies by searching for them among a variety of organisms. (7) The search was prompted by a model for the origin of a primitive multifunctional RNA molecule. (8) In the model, a short RNA with a codon or anticodon near the 3' end undergoes successive rounds of elongation by self-priming (looping back) and self-templating, giving rise to an RNA in which codons are held in contiguous configurations by secondary folding. (9) The subsequent split of message, transfer, and ribosome functions is thought to follow acquisition of the cellular habit. (10) The model suggested the existence of homologies among present day t- and r-RNAs and this prediction is being realized. (11) The interpretation of the homologies is of importance. (12) A multidimensional test for evolutionary convergence has been designed and is being used to determine whether the homologies do indeed reflect common origin rather than function. (13) Filling out the rRNA map, through continued accumula-

tion of homologies, should permit the reconstruction of a primordial RNA molecule.

Version 3

(1) 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. (2) They are too frequent and too extensive to be attributed to coincidence. (3) Factors contributing to these matches might be shared functions at the RNA or DNA levels, or common origins. (4) The latter might be of recent derivation through recombination and transfection, or relics of ancient origin. (5) The matching sequences are distributed without discernable pattern among the molecules and among species. (6) Their frequent appearance, often unique to interspecies comparisons, indicates that they need not result from selection for interaction in a common cellular environment. (7) They are also thought to be conserved vestiges of ancient origin. (8) The occurrence of overlapping sets of homologies within species, and confirming overlays among species (homologies found in independent searches in different organisms that occupy equivalent positions on the rRNAs, and assign similar base sequences) suggest that their continued identification should permit the reconstruction of an RNA that is ancestral to both tRNAs and rRNAs. Such a "synthesis" should help to provide an understanding of the early evolution and current functions of the transcription-translation mechanisms.

Dr. Crews's "Specific Aims"

Version 1

I propose to work in the area of reproductive biology, concentrating on the regulation of reproduction by internal and external stimuli in seasonally breeding vertebrates. Specifically, I will continue my studies of two reptile species that differ markedly in their reproductive physiology.

The green anole lizard is similar to many laboratory and domesticated mammals and birds in that the peak gonadal activity (gamete maturation accompanied by a substantial increase in the circulating level of sex steroid hormones) is *associated* with mating. Species that exhibit such a reproductive tactic frequently have a functional association between sex hormones and sexual behavior. Previous research with the green anole lizard has shed new light on ecological and evolutionary adaptations of the neuroendocrine mechanisms controlling sexual behavior and reproductive physiology. In contrast, the red-sided garter snake, as well as many other vertebrates including some mammals, exhibits a *dissociated* reproductive tactic. In these

species, production of gametes and maximal sex hormone secretion are temporally dissociated from mating behavior. In the garter snake, gametes are produced in late summer only after the breeding season is ended; the gametes are then stored until the next mating period. Thus, unlike those species with associated reproductive tactics, mating in the red-sided garter snake occurs when the gonads are completely regressed and circulating levels of sex hormones are low. This implies that the causal mechanisms of mating behavior, at least at the physiological level, must be fundamentally different in species with dissociated reproductive tactics. Recent studies of the red-sided garter snake indicate that this is the case.

The observation that gonadal and behavioral cycles can be dissociated is itself not new, but the implications of this observation have not been fully appreciated. I present here a systematic and comparative series of studies that will focus on specific questions involving the causal mechanisms and functional outcomes of sexual behavior in these two species. From this comparison will emerge a new perspective on the many species, life histories, and sex differences observed in vertebrates. In addition to contributing to our understanding of related areas of reproductive biology, including gamete storage and animal husbandry, this research will yield insight into fundamental reproductive processes.

Version 2

In general I am interested in the biopsychology of reproduction, or more precisely the regulation of reproduction by internal and external stimuli in seasonally breeding invertebrates. The general objectives of my research are to (i) investigate how the environment regulates reproduction, (ii) determine how reproductively relevant stimuli are perceived and integrated in the central nervous system, (iii) demonstrate how the information regulates internal reproductive state, and (iv) examine how changes in internal state influence the expression of behavior. To this end, I use a comparative approach that combines and integrates the physiological, morphological, organismal, and ecological levels of analysis. The emphasis on laboratory and field experiments reveals the causal mechanisms and functional outcomes of reproductive behavior on each level without obscuring the relations among the levels. Moreover, the laboratory and field studies are complementary. The field has proven to be a valuable testing ground for hypotheses; similarly, the laboratory is the only possible arena for determining the physiological bases of phenomena observed in the field.

The specific objective is to examine the causal mechanisms and functional outcomes of the two major annual reproductive tactics—associated and dissociated—exhibited by higher vertebrates. In many seasonally breeding vertebrates, gamete production and maximum secretion of sex steroid hormones precedes immediately or coincides with courtship and copulatory (mating) behavior. This annual pattern may be termed the *associated* repro-

ductive tactic, or prenuptial gametogenesis (Figure 1). A markedly different annual pattern is exhibited in many vertebrates, including some mammals, in which the gametes are produced only after the breeding season has ended; the gametes are then stored until the next breeding period. In these species, mating occurs when the gonads are not producing gametes and blood levels of sex steroid hormones are basal. This pattern may be referred to as a *dissociated* reproductive tactic, or postnuptial gametogenesis (Figure 1).

I will focus on one representative species of each reproductive tactic. The green anole lizard is similar to many laboratory and domesticated mammals and birds in showing the associated tactic. In contrast, the red-sided garter snake shows the dissociated pattern. In many instances a direct comparison of these two species will be made, whereas in other instances gaps in our knowledge must be filled before conceptually valid comparisons can be made. Thus, some of the proposed experiments deal only with one species or tactic. Ultimately, however, my goal is to compare the two tactics at as many levels of organization as are feasible and reasonable. Such a broad approach is crucial if important generalities underlying vertebrate reproductive processes are ever to emerge. My proposed studies will contribute directly to our understanding of related areas of reproductive biology, including gamete storage and animal husbandry.

NOTES

1. I noticed, for instance, that in my earlier notes, 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 relation to the community. This shift may reflect a real difference between earlier and later drafts, as I show in discussing the authors' changes in strategy. They 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 aware of the context in which these changes were made.

2. Often the biologists gave a different interpretation of the change than mine. 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 in the English teachers' writing.

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