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.

Chapter Three
Social Construction in Two Biologists' Articles

The writer of a grant proposal knows exactly what he or she wants to accomplish, and knows in general the sort of reader he or she is trying to persuade. The rhetorical purpose and audience of a scientific journal article that might result from the funded research are not so clear to nonscientists: the rewards of having an article in *Nature* do not seem so definite and immediate as, say, a grant of $100,000 a year for three years. But money is not the only, or the most important, resource in scientific programs and careers. The "cycle of credit" in science (to use Latour and Woolgar's phrase) involves the conversion of one kind of credit, grants of money, into another kind of credit, the recognition that comes with publication and citation of one's claims to have established a new piece of knowledge, and then the conversion of that recognition into money and other resources. As we saw with both Bloch's and Crews's proposals in chapter 2, grants depend on earlier published claims and are meant to produce later published claims.

The potential audience of a scientific article is so broad that interaction of the sort we have seen between panel and proposal writer is out of the question. But those who have looked closely at scientific articles have shown that, like proposals, they are designed to persuade. The immediate audience at which this persuasion is directed is quite definite: before any article can reach the diffuse and perhaps distant audience of journal readers, it must pass by the immediate and definite audience of a few referees.[1] In most academic fields, and certainly in

1. On the origins of refereeing, see Harriet Zuckerman and Robert Merton, "Institutionalized Patterns of Evaluation in Science." Charles Bazerman expands on this historical perspective in *Shaping Written Knowledge*.

David Hull has a fascinating study of the review processes in "Thirty-one Years of *Systematic Zoology*," *Systematic Biology* 32, no. 4 (1983): 315–42. Hull draws on the sort of editorial records that have not been available to other studies I have seen. He examines

all fields of biology, every claim that counts, however renowned the originator, must appear in a journal that makes decisions on the reports of referees.[2]

The referees of scientific articles are abused nearly as much as the referees of football games. Almost every scientific researcher I have interviewed has an anecdote about a referee who reviewed an article of his or hers unfairly, or who required alterations that, in the writer's view, diminished the value of the article. But there is no equivalent, for a scientific article, of the videotape replay that shows whether the referee's call was correct. I would like to look at the processes of review and revision, not from the perspective of the individual researcher confronting the individual reviewer, but from a broader perspective in which these processes are part of the functioning of a scientific community. I will suggest that the procedures of review and revision of the text can be seen as the negotiation of the status that the

a case in which there were widespread perceptions of biases in favor of certain approaches during certain periods of the journal's history, but he does not find any evidence to support these perceptions. He summarizes this study in a review of the broader issues raised by publication and peer review, "Openness and Secrecy in Science: Their Origins and Limitations," *Science, Technology, and Human Values* 10, no. 2 (1985): 4–13.

Douglas P. Peters and Stephen J. Ceci, "Peer-Review Practices of Psychological Journals: The Fate of Published Articles, Submitted Again," *Behavioral and Brain Sciences* 51 (1982): 187–255, presents an experiment based on the trick of submitting as manuscripts the texts of articles the same journal had already published, changing only the name and institutional affiliation of the author (the journals did not use blind refereeing). Because this article appeared in a journal that has peer commentary, there is a fascinating range of responses to this experiment, most of them from editors of other psychological journals. Peters and Ceci did not change the sex of their fictional authors. One study that does attempt to investigate gender bias is Michele A. Paludi and Lisa A. Strayer, "What's in an Author's Name? Differential Evaluations of Performance as a Function of Author's Name," *Sex Roles* 12 (1985): 353–61. But their study used the responses of college students, not those of actual referees.

2. John Ziman's work has stressed the importance of review and public consensus in the authority of scientific knowledge; for a brief summary, with basic references, see *An Introduction to Science Studies* (Cambridge: Cambridge University Press, 1984), ch. 4. Linguistics is a notable exception to the requirements of publication: key papers are often available for years only as quasipublic circulated manuscripts; see Frederick Newmeyer, *Linguistic Theory in America* (New York: Academic Press, 1980), throughout, for discussion. As Newmeyer points out, the second most cited work in syntax is the unpublished doctoral dissertation by John Ross, "Constraints on Variables in Syntax" (MIT, 1967).

Many historical studies stress the importance of informal communications; for particularly rich accounts, see Rudwick, *The Great Devonian Controversy* (on geologists in the 1830s) and Pickering, *Constructing Quarks* (on high-energy physics in the 1960s and 1970s).

scientific community will assign to the text's knowledge claim. This negotiation may not directly address the claim itself and the evidence for it, but may instead focus on the form of the text. Thus, a close study of these texts may help us see one part of what Latour and Woolgar, in the subtitle to *Laboratory Life*, call "The Social Construction of a Scientific Fact." I present two articles as cases of such negotiation, showing the range of possible claims (that is, assertions of new knowledge for which the author is to be credited). I interpret the formal changes in the manuscripts as they affect the status of the claim and account for these changes in terms of the social context, the relations between the author, the editor, the referees, and the wider scientific community. I make several kinds of comparisons: earlier articles published by the same authors serve as a background of acceptable form, successive versions of the articles show the process of negotiation, and the differences between the views of editors and reviewers on one hand and writers on the other show the kinds of tensions on which the negotiation is based.[3]

The two articles are by the authors of the proposals in chapter 2: "tRNA-rRNA Sequence Homologies: Evidence for a Common Evolutionary Origin?" by David Bloch and his colleagues, and "Gamete Production, Sex Hormone Secretion, and Mating Behavior Uncoupled," by David Crews.[4] I have chosen these articles because they show the processes of social construction with particular clarity, each having had a rather bumpy ride before appearing in print. Each article had been through four reviews before it was finally accepted, Bloch sending his to *Nature* (twice) and *Science* before it was accepted in a revised version at the *Journal of Molecular Evolution*, Crews sending his to *Science* (twice), *Nature*, and *Proceedings of the National Academy of Sciences* (*PNAS*) before it was finally accepted at *Hormones and Behavior*. The authors rewrote the articles each time, so that the published versions are hardly recognizable as related to the first submissions. These texts are atypical; they mark departures in the careers of two well-established researchers, so they show the tensions operating in the process of publication. As we have seen in studying their proposals, the two authors are different enough from each other to be compared in social terms, one entering a new field, the other well estab-

3. For my use of these terms, see Gilbert and Mulkay, *Opening Pandora's Box*. John Law and Rob Williams, "Putting Facts Together: A Study in Scientific Persuasion," *Social Studies of Science* 12 (1982): 535–58, reach conclusions similar to mine in an analysis of discussions among the coauthors of articles and relate these conclusions to the concept of a network.

4. See the Reference List, section 2, for texts discussed in chapter 3.

lished in a network of researchers. And the long, drawn-out review, though unusual, helps us see the texts in evolutionary terms: we see in detail responses and decisions that are usually compressed and unnoticed even by the participants.

I collected the various manuscripts of these articles that were submitted for publication, and a few of the many intermediate drafts, together with comments of the authors' colleagues and of the reviewers, and the authors' responses to these comments, in correspondence with the editors and in interviews with me. I marked changes, however trivial, and made some guesses about why these changes were made. Then I interviewed the authors, asking them for their own interpretations of the changes, and submitted a draft of this paper to them for comments. Thus my conclusions draw on three kinds of interpretations of the texts and revisions: my own reading, in complete ignorance of biology, the comments of the authors' colleagues and then of anonymous reviewers, and the explanations given by the authors themselves. I was interested in the differences between these readings, not in determining which one was "correct." My assumption is that none of them is privileged, so I have relied on neither the authors' claims for the importance of the articles nor the reviewers' doubts. I have tried also to avoid privileging my own outsider's perspective, but that perspective may tend to dominate, just because it is the basis for my narrative.

Why did these articles take so long to get published? The explanations for the delays depend on whether one focuses on the individual researcher or on the structure of the research community. A rhetorician might say that the authors had to invent by trial and error the arguments by which they could persuade their audience to assent to their claims. A sociologist interested in how social factors distort what he or she considered objective scientific research might say that the authors' research programs conflicted with the individual interests of reviewers, who had their own established research programs; before they could publish they had to find journals of subspecialties in which their work was not a threat. Both these approaches are useful, but they both underestimate the social nature of the publication process. The rhetorical approach, in treating the problem as a matter of strategy, accords the writer more conscious control and detachment from the audience than I can see; we must remember that Crews and Bloch frame their ideas, however unorthodox, within disciplinary assumptions. The approach through individual interests, although it points out some kinds of social influences, overlooks the way the very form and language of the article tend to create consensus. It would be a

mistake, for instance, to analyze the institutional affiliations of the authors or their age or class background as "social" elements, while ignoring such textual features as the footnotes or the tables of data or the partitioning of the article into sections, all of which are governed by textual conventions that shape the way a claim can be made.

I argue that the process of writing a scientific article is social from the beginning, because it involves compromises between opposing rhetorical demands and opposing goals. In the proposals we saw a tension between the need to show the originality of the work planned and the need to place it in the context of existing work in the field. In scientific articles there is a similar tension that makes negotiation between the writer and the potential audience essential. On the one hand the researcher tries to show that he or she deserves credit for something new, while on the other the editors and reviewers try to relate the claim to the body of knowledge produced by the community. But the claim must be both new and relevant to existing research programs to be worth publishing; the writer cannot please the audience just by being self-effacing. The result of this negotiation is that the literature of a scientific field reproduces itself even in the contributions of those who challenge some of its assumptions.

This tension is brought out in the negotiations over the published form of the text, so we can see, in arguments over the tiniest textual details, larger tensions over the claim, its appropriateness to the journal, and its form. The claims in these articles can be taken on several levels of significance. For instance, one of these claims may be allowed as a description of one species, or as an interpretation of a process applicable to all species, or as an argument for how this process evolved. The claims that are restricted to descriptions of the data are not inherently more scientific, or even more publishable; they are just one level of a hierarchy in which the place of the article is being negotiated. The same claim may be considered "speculative" or "well defined," a "highly significant" advance or a "well-known" observation, depending on the body of literature into which it is placed and the audience which is to read it. We can see in the referees' comments negotiations over how the claim is to be placed. In general, the authors start by making high-level claims for the importance of their findings, whereas the reviewers demand that they stick to the low-level claims that take their findings as part of the existing structure of knowledge.

Much of this negotiation over the status of the claim concerns the "appropriateness" of a paper to the journal to which it has been submitted. Of course the authors want to see their papers published

in prestigious journals that testify to the importance of their claims. They also have practical reasons, because of the interdisciplinary nature of their work, to want to appeal to a broad range of researchers in other specialties who might, if interested, provide data to support their claims. And, like most authors, they want speedy publication, especially because the articles would then support the related proposals for funding that we saw in chapter 2. The referees, on the other hand, see their function as that of sorting papers by levels of importance, by subspecialty, and by genre. Their use of the word "appropriate" or "inappropriate" in evaluating an article might suggest that each manuscript is unambiguously marked as one sort of article or another, but the sorting is not, in fact, automatic. Here too we see a tension finally resolved in the compromises that allow an article to appear in print.

There is a similar tension over the form of the article. These authors have some difficulty fitting their new interpretations into the form of the research report or the review article, because these forms demand that the claim fit closely into the structure created by other scientific articles. The authors try to bend the constraints of form to fit their ideas—in effect, to tell their stories from the beginning—whereas the reviewers try to use the form to make the ideas fit into the literature as a whole. Again, they are arguing, not over the writers' failure to use the correct format, but over the type of the claim and the importance to be accorded to it.

The Writers

We have already seen that the ideas in their proposals could be considered controversial by other researchers. Some readers of this chapter might tend to accept these referees' reports and assume that Bloch's and Crews's difficulties in getting their articles published must be traceable to their own eccentricities or scientific skills, rather than to their claims in relation to the literatures of their fields. But as my brief biographies in chapter 1 show, both authors have had successful careers, and they are both familiar with the literature of their fields and with the processes of publication. The *Science Citation Index* shows that most of the cited publications of both authors are reports of experimental findings or field observations published in the core journals of their disciplines; it also shows an important difference in the authors' positions in the subspecialties to which these new articles matter. David Bloch receives about forty citations a year for articles as far back as 1954; his most cited paper is a 1969 *Genetics* review article that still

gets ten to fifteen citations a year, an unusual number for a fourteen-year-old article in most fields. He seems to have written several cited articles a year through the 1960s (with fewer in the 1970s, as his interests changed), all in journals of cell biology or genetics, in general journals like *PNAS*, or in specialized handbooks. But the article considered here is his first publication in the field of nucleic acid research, a very competitive field in which most publications in the major journals come from large groups at well-established labs.

David Crews has been first author of five or six cited articles a year since 1974; with the work on which postdoctoral, graduate, and undergraduate students were first authors, his lab produces about fifteen articles a year. These papers fall into several categories: articles in journals of zoology and endocrinology, those in more general journals, such as *Science* and *PNAS*, chapters in books, and popularizations in *Scientific American* or *BioScience*. Currently, his most cited entries are articles in *Science* (1975) and *Hormones and Behavior* (1976), though a controversial report in *PNAS* (1980) received a number of citations and some news stories soon after its publication. This last article sparked off the controversy analyzed in chapter 4.

The articles I have chosen to discuss differ from these earlier publications in complex ways that I can summarize by saying that the writers each had a big idea. Most of their earlier articles, whether experimental or theoretical, had answered questions posed in the literature. In the articles I consider, though, Bloch is answering a question that had not been asked, and Crews is giving a new answer to a question that had already been answered. The articles are especially suited to illustrate the tensions I have outlined because they are interpretations of published data in new terms, rather than reports of experiments continuing an established research program. Thus, if the writers are to have any effect, they need to claim significantly new interpretations and reach a broad audience, for they cannot just contribute data to, or get additional data from, the researchers in their immediate subspecialty. The two authors have a similar problem, but their processes of publishing these claims are different, partly because of their differing positions in their research communities.

For Bloch, the big idea came when he started work in an area entirely different from the cell biology studies he had pursued for twenty-five years. As we have seen in chapter 1, he started a new line of thinking in the late 1970s after a back injury and a graduate seminar give him the opportunity to think about something other than the main work of his laboratory. In 1981 he wrote, but did not try to publish, a paper on "The Evolution of Evolution," and from then until

1986 he wrote a number of drafts of one article, and versions of several related studies. Bloch, then, is an unusual example of a writer completely new to a specialized field who is (as he must be for my purposes) quite familiar with the way biology journals in general review articles for publication.

In his acknowledgments, Crews traces his big idea to discussions with colleagues whom he thanks as "innocent bystanders" and to mentors who taught him "the value of a comparative approach." He has also pointed out in conversation how these teachers made him skeptical of "deterministic models of behavior"; he sees himself as carrying on, in his experimental studies, a form of the nature vs. nurture argument. As I noted in chapter 1, his training in several different disciplines might tend to make him receptive to unorthodox ideas. His popularizing articles and reviews require him to explain and rethink basic principles, as Bloch must do for his freshman classes. The many grant proposals needed to support his large lab, most of them to health agencies, require him constantly to justify his work with reptiles in terms of its significance for humans, so he must consider its ultimate relevance. Whatever the reason, he, like Bloch, proposed a claim that was at variance with the current literature in his field, that needed support from researchers in several fields, and that led him to try a different form from that of his earlier reports and reviews of research. He, like Bloch, based a proposal for funding on this claim, and his proposal and article evolved together. Meanwhile, he and his colleagues in the lab continued to publish other articles reporting new data from their experiments and field observations.

The differences I have suggested in the positions of the two writers may or may not affect the judgment of editors and referees. But I suggest that these differences are felt before the articles are submitted, in the writing and revision of the papers. Bloch, when he was writing this article, had no research network: few, if any of his graduate school friends, colleagues, and students worked on this aspect of nucleic acid research, and the leaders in the field did not know of his work. In this sense, he, as a full professor, was more isolated than a new graduate student, who would have, at least, a sponsor. He valued the help of students a great deal, gave them coauthor status, and remained in close contact with R. Guimares, one of his coauthors, who visited Texas from Brazil for five months in 1981. But he did not then have constant informal contact with coworkers who are expert in this area. So he did not hear arguments against his claim before he submitted a paper, and he had no day-to-day sources for new arguments to support his work, or new data that could be relevant. After

years of collaborative work in cell biology, he was acutely aware of what he was missing; he wanted to get the project funded, not primarily for the equipment and time it would give him, but to have a postdoctoral student "to bounce some ideas off." His coauthor on a more recent manuscript that grew out of this project was a physicist specializing in statistical mechanics; his quest for collaborators finally took him outside of the discipline of biology.

Crews's lab, on the other hand, is an important node in his network of researchers. His graduate school training and two postdoctoral positions doing research related to his current work, his teaching of dozens of undergraduates and graduates who are now themselves teaching at other schools, his fellowships and visiting professorships, and his dozens of conference papers and lectures have given him daily formal and informal contacts with many researchers in both zoology and psychology. A recent article on which he is first author lists five coauthors at three other universities. Any article he submits for publication has been criticized by many readers; it is already the product of a community. And to a large degree, he has internalized this community; he could easily predict the contents of negative reviews (and in some cases could guess the identity of the anonymous reviewers). Crews's position in a network does not mean his articles or proposals are always accepted, but as we will see, it enables him to be considerably more flexible in the negotiation over the status of his claim, in the revision of his article, and in his choice of outlets and audiences.

Determining the Claim

Despite these differences in the positions of the authors in their fields, their two articles go through roughly similar stages on the way to publication (Appendix 2). I shall illustrate the negotiations over the published form of each article with six texts.

1. First each author wrote a wide-ranging draft he did not submit for publication.
2. Each then wrote a more limited and conventional manuscript for submission to major interdisciplinary journals: *Nature* (in Bloch's case) and *Science* (in Crews's case). Bloch's was rejected without review, whereas Crews's was reviewed by two referees who split in their decisions, and also rejected.
3. Each author then revised and resubmitted the manuscript to the same journal, with a cover letter asking for reconsideration; both were reviewed and again rejected.

4. Still confident that their manuscripts were important, they resubmitted to other prestigious interdisciplinary journals, Bloch revising somewhat for *Science*, Crews revising drastically for *Nature*. This time Bloch's article got an ambivalent but generally favorable review, but was still rejected, whereas Crews's article was returned without review.

5. After these rejections by *Science* and *Nature*, both authors submitted to journals with more limited audiences that seemed more likely to accept the articles. Bloch sent a revised version to a journal recommended by one of the referees at *Nature*, the *Journal of Molecular Evolution*. It was accepted on the condition that certain changes suggested by the referees and editor be made. Crews submitted the unrevised *Nature* manuscript to *PNAS*, where the referees were generally favorable but still recommended rejection.

6. Finally, both articles were published. Bloch's manuscript was accepted in its revised form in the *Journal of Molecular Evolution*, where it appeared in the December 1983 issue. Crews's unrevised manuscript was accepted at *Hormones and Behavior* on the basis of its previous reviews, and appeared in the March 1984 issue. The revisions between each of these stages are extremely complex, ranging from massive cuts and additions to the shifting of an adjective or a comma. I shall focus on changes that seem to affect the scope or the form of the article, for these are the features that seem most crucial in the negotiation of the status of the claim.

A citation such as "(Watson and Crick 1953)" refers to a single knowledge claim an article makes, in this case, for instance, the claim that the structure of the DNA molecule is a double helix with chains of phosphates on the outside and particular pairs of bases connecting them. Nigel Gilbert has shown how one published article may contain a number of possible knowledge claims, from which the authors and readers select the claim relevant to the model by which they are interpreting the article. Latour and Woolgar have arranged the interpretations of the claims in an article in a five-level scale of statements from "fact-like status" to "artefact-like status." They show how statements can be transformed from one type of statement to another by addition or deletion of "modalities," statements about the statements, as in "The structure of GH.RH was *reported to be* X." Trevor Pinch also proposes a hierarchy of claims; his is arranged in terms of what he calls increasing "externality," from the lowest level, statements about the observing apparatus ("Splodges on a graph were observed") to the highest level, statements about phenomena at several removes from the observing apparatus ("Solar neutrinos were

observed").[5] He points out the similarities between these poles and the philosophers' opposition of claims with high veracity to claims with greater theoretical significance. I see a hierarchy similar to these in the two articles I am describing, but I prefer to define it in terms of the distance between the authors' claims and the claims of the particular part of scientific literature in which they are to be placed. The issue is the way the claim fits in what Pinch calls the "evidential context." The higher-level claims, in each case, involve contradiction of large bodies of the literature, of claims that underlie many research programs or claims that are particularly well entrenched. The lowest-level claims contradict nothing, but neither do they add anything to what has been accepted. Like Pinch, I see this hierarchy as determining, not just the degree of acceptance or rejection of a particular claim, but which claim is accepted or rejected. Like Latour and Woolgar, I am trying to base this hierarchy in the language of the claims rather than in some inherent reliability or unreliability of methods of observation or experiment.[6]

As Pinch points out, higher-level claims are likely to be profound but risky, whereas lower-level claims are likely to be taken as correct, but are also likely to be trivial. Both the biologists I am studying try to make the highest-level claim the editors and reviewers will allow (Appendix 2). Bloch's highest-level claim appears only in the early draft he did not submit for publication; he identifies a fundamental concept that he says links several kinds of evolution. "Transfer of control . . . given the name 'surrogation,' marks the appearance of new kinds of behavior at every level or organization and process, including evolution itself." His first manuscript submitted for publication just presents the model and makes the claim that "a primordial tRNA produces through successive rounds of elongation a molecule with multiple functions of gene, message, and scaffolding, and which serves as a source of the original tRNAs and rRNAs." Supporting this model, in the same manuscript, is a more limited claim, an interpreta-

5. Nigel Gilbert, "The Transformation of Research Findings into Scientific Knowledge," *Social Studies of Science* 6 (1976): 281–306; Latour and Woolgar, *Laboratory Life*, pp. 78–86; Trevor Pinch, "Towards an Analysis of Scientific Observation: The Externality and Evidential Significance of Observational Reports in Physics," *Social Studies of Science* 15 (1985): 3–36; Harry Collins, *Changing Order*, has a discussion of these approaches to presentation of claims in chapter 6. Bruno Latour discusses the modalities at greater length, with a variety of examples, in *Science in Action*, pp. 21–44.

6. I discuss the form of these statements in terms of Brown and Levinson's linguistic analysis of politeness in "The Pragmatics of Politeness in Scientific Articles," *Applied Linguistics*, 10 (1989): 1–35.

tion of data: "The patterns and distributions of homologies make phylogenetic relatedness a more plausible explanation than evolutionary convergence." This is the major claim that remains in his first revised version of the article after the reviewers' criticisms. A still more limited claim shows the relation on which the interpretation of a common origin for the two molecules is based: "The existence of homologous sequences among tRNAs and 16S rRNA is demonstrated." This is the claim that most interests the reviewers, who generally agree in finding his data on matching sequences intriguing.

It would be possible for Bloch to make an even more limited claim, stating the sequences of the RNAs without insisting on the homologies, but since he is using already published data rather than doing his own sequencing, such a very limited claim would not be publishable. Even the observation of homologies is trivial, in his own view, without some explanation of why this pattern should be noticed. So we cannot simply say that Bloch should avoid speculation; he has to try to make one of his higher-level claims stick. If the model for the evolution of RNA is accepted, he will have one piece of his larger argument in place. He may have selected this particular piece because he can define the claim for the homologies clearly and design a research program to support it using computers (to which he has access) but requiring no new (and expensive) equipment. This awareness of what constitutes a "well-defined" claim and a practical research design are part of what he brings with him from his cell biology work; he doesn't have the contacts, but he does know the conventions.

Crews, like Bloch, makes higher claims for the implications of his work that are supported by lower-level claims interpreting his observations and still lower claims showing what he has observed. His highest claim, the claim that relates to nature vs. nurture arguments, is that environmental factors may influence the evolution and development of three aspects of reproduction: "(i) The functional association among gamete production, sex hormone secretion, and mating behavior, (ii) The functional association between gonadal sex (= male and female individuals) and behavioral sex, (iii) the functional association among the components of sexuality."

His first submitted manuscript limits the claim somewhat by focusing on the first of these aspects, the assertion that, contrary to the assumption of what he calls the prevailing paradigm, these processes can be dissociated. Supporting this claim is the more limited claim that there exist many species in which gamete production, sex hormone secretion, and mating behavior *are* dissociated, and that these species need to be studied further. This is the claim that the more

favorable reviewers emphasize as an addition to the structure of claims in the literature. When Crews revises his claim to this level, he loses some of the argument for ecological approaches he was making in his first draft; the claim for dissociation can be made at this level without any reference to the environmental factors leading to such dissociation. Supporting this claim is the still more limited claim that these processes of reproduction are certainly dissociated in at least one species, the red-sided garter snake. This last claim is accepted even by hostile reviewers, but Crews has already published his studies of garter snakes, so to make that claim alone would be trivial. It belongs, not in *Science*, but in the popular article he and William Garstka wrote for *Scientific American* (see chapter 5). Crews, like Bloch, must limit the scope of his claims. But as we shall see, he has the advantage that his claims, however contrary to established research programs, emerged from those programs and can be related back to them.

The hierarchy of claims has some relation to the hierarchy of journals to which Bloch and Crews submit their articles, at least in the case of some of the most prestigious journals, which insist that the claims in articles they publish be of interest beyond any one subspecialty. Bloch's decision to send his article first to *Nature* and Crews's decision to send his first to *Science* indicate how important they considered their claims, since the editors of these journals say in their instructions to contributors that they select "items that seem to be of general significance" (*Science*) or "reports whose conclusions are of general interest or which represent substantial advances of understanding" (*Nature*). Neither publication is limited to biology articles, but since there is, apparently, no biological equivalent of *Physical Review Letters*, they fill the role of rapid-publication, prestigious journals. And they offer the access to the broad audience both authors need if they are to find a wide range of data to support their hypotheses.

The articles were rejected, originally, on the grounds of the "appropriateness" of their articles to these journals, rather than on the grounds of faulty interpretation of data. The editor of *Nature* returned Bloch's article without review, saying that the journal was unable to publish manuscripts that, "like yours, are very long and speculative" and suggesting he send it to *The Journal of Theoretical Biology*. The words "long" and "speculative" and the alternative journal suggested place the article in the hierarchy of claims: the editor does not accord the claim in this form the status that would justify such broad implications, so much space, or such a broad audience. Bloch's response, in a covering letter with a revised manuscript, shows he reads the editor's

criticisms as part of a negotiation, not just as a formal criticism of the length of the article. But his response also shows he is not yet willing to give much up by making major revisions. He argues that though the article seems speculative to the editor, it is in fact "an analysis of hard data" that "makes predictions" on the basis of the model, and "these predictions were fulfilled." He argues that the speculation at the beginning "is appropriate" and "would be conspicuous by its absence." If it seems to belong in a more specialized, more purely theoretical journal, then the editor is overlooking "a completely new slant on the origins of RNAs and of coding mechanisms . . . a 'Rosetta Stone' for the origin of life." He apologizes for seeming to make "extravagant" assertions of his claim, but such assertions are the only way he can press a claim not grounded in the literature.

This letter from Bloch asking for reconsideration suggests he saw the rejection by *Nature* as an oversight on the part of one editor, not as part of the community's assessment of his claim. But when his revised version was given to referees (three rather than two, suggesting the editor tried to resolve some ambivalence), they made comments similar to those given by the editor, focusing on the status of the claim rather than the evidence or argument for it. As a high-level claim about the origin of RNA it lacks rigor; as a low-level claim making some observations about homologies in RNAs it lacks general interest beyond the subspecialty concerned with molecular evolution. One referee suggests the claim cannot be supported by the literature of molecular biology, and thus belongs in what he sees as a less rigorous subspecialty: "The manuscript drifts into unsubstantiated speculation; this, however, is common in evolutionary papers." Another shares these doubts about the "highly speculative evolutionary model." Two of the reviewers suggest it be sent to "a more appropriate journal" or "a more specialized journal." The other suggests *Nature* could "publish a much briefer account of the homologies together with a brief possible interpretation of it." Although two of these readers raise statistical questions, and one refers to an earlier article, famous in the field, that puts forth a similar idea, none of them attack the evidence so much as they question the status of the claim itself. The reviews at *Science*, although much more favorable, also deal with the status of the claim, splitting the higher-level claims from the lower-level claims: "It is not clear that the empirical observations of homologies and the discussion of pmfRNA [that is, the model for common origins] can both be adequately presented in a single paper which meets *Science* page limitations." The editor apparently took this tension between claims as unresolvable, for she rejected the article without suggesting any rewriting.

The comments on the claim by referees at the more specialized journal that accepts Bloch's paper, the *Journal of Molecular Evolution*, are actually quite similar to those at *Nature* and *Science*, but here these comments do not indicate that the paper is inappropriate to the journal. "The hypothesis of this paper is of interest to evolutionists," one begins, suggesting that Bloch has found his niche in the hierarchy. Although by the *Nature* referees' standards the hypothesis is quite far from the data, here the referee finds claims that are, in Popperian terms, falsifiable: "Both the hypothesis and the data are clear-cut enough so that if the authors are wrong they will hear about it quickly from other scientists." But even the most enthusiastic reviewer is concerned that the article, while significant, does not entirely fit in the current structure of claims. If the statistics check out, the reviewer says, "This is an important finding that needs to be explained," and he or she "strongly recommends" publication. But before Bloch can make higher-level claims, his lower-level claims must be accepted by the rest of the research community of the subspecialty. "The essence of an initial paper should be to document the reality of the homologies rather than extensive studies of their origin. If such discussion is to be included at all, it should be far more balanced and less speculative." In these terms, his discussion is "speculative," not so much because it runs ahead of the data, but because it runs ahead of the literature.

The criticism of Crews's first manuscript by the referees at *Science* also focuses on the placement of his claim. One sees it as placed too low on the hierarchy, in relation to the accepted knowledge of the field: "I learned very little . . . the model is really very simple." But he can imagine a more important article on the same topic and by the same author that would be appropriate to this journal. "I am ambivalent. *Science* needs some articles in important areas such as this. I think the author, who has done some very important work, can do a better job of putting things together. . . . *Science* is read by such a wide audience that this article will certainly reach the audience that needs it most." This ambivalence makes sense if we see that the decision being made concerns not just the acceptance or rejection of the article, but also how the knowledge claim will finally be presented to the community. The status of his claim is indicated by a number of formal features that can be negotiated separately; the referee can choose to accept the author's claim without accepting some aspects of its form. The other, entirely negative review, also places the status of Crews's claims. The referee locates three claims Crews is making: one is "an accepted fact," (that is, a claim at the lowest, trivial level), another is "not a new or startling observation" (also at a low level),

and the third is "a quantum leap from faulty premises" (a higher level than the referee will grant). He sees, as does the more positive reviewer, that the negotiations here concern the status that this journal can confer upon the claim: publication of this material in a journal as prestigious as *Science* "could set the field back by providing a strawman for those that feel it necessary to refute the thesis." In effect, he doesn't say, "it's wrong," but says, "it doesn't belong in this field."

Crews's letter to the editor in response to the *Science* criticisms shows that he, like Bloch, is aware that formal points are part of a negotiation of the status of his claim. Like Bloch, he defends his claim in a language quite different from that of the article itself. But unlike Bloch, who can only point out to the editor the importance of his claim, Crews is enough a part of the network of researchers to be able to fit his claim into its structure of knowledge, or at least to try. His originality, he says in his letter, is not in the observation but in recognizing its larger implications. Even if "this observation has been around for at least forty years, its significance at the conceptual level has been unappreciated." Whereas the reviewer says his claim is "an accepted fact," he can show that the opposite view is held by the standard textbook (from which he quotes) and in two recent *Science* articles. He implies that his refutation of this view is appropriate to the same journal and audience.

But in a second review at *Science*, the referees are even further apart than before. One referee says again that the claims are either well known already ("Only the naive who had done no reading would suggest that . . . no one has ever claimed that") and out of touch with current knowledge ("it is entirely different from the well-documented findings"). Finally he or she questions whether the paper says anything definite: "I was unable to find this experimentally testable hypothesis, as were two of my colleagues who I asked to read the paper." The second referee seems to have read a different article from the first: "The author of this paper provides a valuable service . . . a needed jolt . . . another important contribution . . . a clever and reasonable hypothesis." Although the referee says "any biologist with even a passing interest" in the topic is aware of some of the specific instances Crews cites, he or she sees the usefulness both of a list showing "a large number of such exceptions" and of his evolutionary hypothesis, which "will certainly generate debate and further research." In a sense, the two referees did read different articles. Crews suggests that the first reviewer is a classical neuroendocrinologist, and the second a comparative zoologist. If that is the case, they are placing the claims of the article in different hierarchies, so different

that where one reader finds a clever and reasonable hypothesis, the other finds no hypothesis at all.

The comments on the shortened manuscript Crews submitted to *PNAS* show that it is not necessarily enough that the claim of the article be significant; it must have the right sort of significance. Although the referees grant the importance of the article, they both say that it does not make the kind of claims appropriate to this journal. *PNAS* is certainly a prestigious journal, but the checklist it sends to referees to get their comments suggests that, unlike *Nature* and *Science*, it selects claims presenting new data that fit an already established conceptual framework. So one referee says that "this article breaks new ground" but decides that "While I found the hypothesis as presented quite interesting and worthy of serious experimental attention, I do not think this idea merits a separate *PNAS* article." The other review shows clearly how the status of the claim may be separated from the question of appropriateness to a specific audience and journal, so I shall quote it at length.

> I have little problem with my recommendation regarding this paper: it does not belong in *PNAS*. I think that the points raised are of great importance, that the scholarship is genuinely profound, that the conceptualization is original, that the presentation is crystal clear and not obfuscated by unnecessary information and argumentation. I would strongly urge its publication in a more general journal (obviously, I would think first of *Science* or *Nature*) where it will receive the attention it deserves. . . . It is because I consider this survey/thesis to be highly significant that I do not think it belongs in a journal that publishes "data" papers.

Though it may have struck Crews as ironic that his paper would be rejected for being "highly significant," and that he would be referred back to the journals he had spent months trying to satisfy, this reviewer's decision makes sense. It is consistent with earlier reports in focusing on the "issue of appropriateness," and on determining just what kind of claim is being made, rather than evaluating the evidence for the claim. So for this reviewer a "highly significant" theoretical formulation is as much out of place in *PNAS* as would be an article with weak data or unimportant claims. This interpretation of the reports is confirmed by the decision at *Hormones and Behavior* (which often publishes the work of Crews's group—three articles in that issue alone), for Crews just sent that journal the reports he had gotten at other journals, and the article was accepted without further review

or revision. The editors seem to have accepted the favorable reviews I have quoted and to have discounted the unfavorable comments, which dealt with the article's relation to other specialties or its appropriateness for more general journals. So the same placement of claims that was grounds for rejection at *Science* and *PNAS* is taken, at a journal devoted just to one specialty, as grounds for acceptance.

What has changed in the course of these negotiations? Both Bloch and Crews have altered their claims, choosing a somewhat more limited claim before submitting the manuscripts for publication, and then cutting their more controversial, higher-level claims in their revisions. In exchange for publication, they accept a different level in the hierarchy of claims. They have also settled for less prestigious journals and more specialized audiences, accepting for these claims a somewhat different status than that they had first proposed.

Choices in Form and Style

So far I have described only those referees' comments concerning the statement of the claim itself and its appropriateness to a specific journal. But referees' comments about such matters as length, organization, and style are not just matters of taste; they too help define the status of the claim. As there is a tension in determining the appropriateness of the claim for a particular journal, between assertions of originality and participation in an established structure of knowledge, there is a tension in determining the form of the article, between the construction of the idea as the author tells it and the conventional formats of the report or review article, which emphasize the placement of the article within a body of literature. As Bazerman and others have shown, these formats, though flexible within limits, embody the attitudes of a subspecialty toward claims, methods, and use of the existing literature. And the conventional tone of scientific articles carries assumptions about the appropriate persona for the researcher. The author has a story which he cannot tell as his own, ignoring the literature, and yet does not want to fit completely into the format, distorting the shape of his idea.

A number of writers (including most notably Peter Medawar) have commented on the differences between narratives of the actual experience of science, with all their odd sources of ideas, wrong turnings, and unexpected discoveries, and the presentation of science in journal articles, the form of which suggests a method of pure inductive logic. Thus it may seem strange for me to speak of the author's "story" in describing the forms of these articles, as if they had presented their ideas in autobiographical fashion. But Latour and Bastide have shown how some narratives can remain within the form of a research report, latent in the methods section or in descriptions of physiological processes. And Mayr and others have commented on the particular importance of narratives to biological argument, which, unlike the physical sciences, must often deal with unique events in time: "Explanations in biology are not provided by theories but by 'historical narratives.' "[7] These narratives need only be implied in most articles, so that, for instance, observations of successive generations of Crew's *Cnemidophorus* can make sense without a retelling of the narrative of genetics, and Bloch's observations on sequence homologies do not need to be supported by a history of the breaking of the genetic code. Perhaps Bloch and Crews use somewhat unconventional forms for these articles because they find they need to retell a whole narrative from the beginning, rather than dealing with just one incident within the narrative given by the scientific literature. In these terms, each deviation from what the editors expect may be, not an error, but an assertion of the status of the claim, of its originality. The choice of form suggests the audience that the author thinks the article deserves. In the simplest example, an editor will not allow an unusually long article unless he or she considers it unusually significant. The reviewers' comments suggest that a similar kind of evaluation is made whenever the organization or tone of an article departs from the conventions. As Bloch and Crews gradually move from the somwhat unconventional forms of their earlier manuscripts to the more conventional versions that are finally published, they are accepting the status these referees accord their claims, accepting the decision that their claims do not call for special formal treatment.

The form of the earliest draft by each author reflects the route he took to this research program. Though Bloch's early draft, "The Evolution of Control Systems: The Evolution of Evolution," apparently follows the format of a review article, with an abstract, introduction, definitions, examples, and copious citations, the style is personal and exploratory, allowing for digressions (labeled as such), asides, suggestions of possible lines of thought left unexplored, and references to a wide range of authors outside the subspecialty, from Darwin to

7. Bruno Latour and Françoise Bastide, "Writing Science—Fact and Fiction: The Analysis of the Process of Reality Construction Through the Application of Socio-Semiotic Methods to Scientific Texts," in *Mapping the Dynamics of Science and Technology*, ed. M. Callon, J. Law, and A. Rip (London: Macmillan, 1985), pp. 51–67; Ernst Mayr, *The Growth of Biological Thought: Diversity, Evolution, and Inheritance* (Cambridge, Mass.: Harvard University Press, 1982), pp. 71–73.

Delbruck to Prigogine. The evolution of RNA, the topic of the article he will submit for publication, is here just a one-page example of the genetic code. Only at this early stage do we see in the text the relation of this model to the ideas of code, information, control, and culture with which Bloch began his thinking. Only a reader of this draft, or someone who had had a chance to hear Bloch talking informally or at a poster session, would suspect that his real goal was an explanation of the origin of life.

Crews's early draft, titled "New Concepts in Behavioral Endocrinology," also shows more of the relation of this research to his larger thinking than does the first version submitted for publication. The paper seems to have been written for people who are already receptive to his ideas, terminology, and criticisms of current concepts; the eight names in the acknowledgments suggest that only a close group of colleagues had read it yet. For this audience, he can safely follow an organization that is more exploratory than argumentative, opening with broad questions, making a leisurely review of his recent work, and only presenting his alternatives in the last pages. At this stage, one can still see the relation of his research on dissociation of gamete production and hormones to his larger assertion of the importance of environmental factors in all aspects of the evolution and development of reproduction. Crews's first draft, like Bloch's, is closer to the form he uses for oral presentations than to that of his other articles; it lacks only the slides with cartoons of lizards.

The manuscript Bloch sent to *Nature* is much more conventional than his early draft. But we can see in it a tension between the form of the report on research and the more exploratory form he had given up by comparing the submitted draft to a recent article that be had written with colleagues on cell biology, "DNA and Histone Synthesis Rate Change During the S. Period in Erlich Ascites Tumor Cells." In the *Nature* submission, six pages of introduction provide the reasoning behind the model, and then just two pages of methods and results describe the work, before nine pages of discussion and two pages on "Further Evolution." So the article is about 32 percent introduction, 10 percent methods and results, and 58 percent discussion. In contrast, the cell biology article is about 11 percent introduction, 47 percent results and methods, and 42 percent discussion. If Bloch's problem is that he is answering a question that has not yet been asked, his solution here is to start in his introduction with the most fundamental questions—the conditions for the first protein synthesis—and work toward his interpretation. In cutting the methods and results section so drastically, he may be assuming that his extensive tables (with

forty of his sixty references) are striking enough in themselves to attract attention, and too straightforward to need explanation. The bulk of the paper is in a discussion titled, "Common Descent, Evolutionary Convergence, or Coincidence," in which he gives his interpretation of these results. The last section, "Further Evolution," does not correspond even roughly to a section of a conventional article, but crowds in some of the ideas from the early draft, relating all this back to his broader claims. The form still looks like a personal essay embodying the researcher's thought, rather than a research report embodying the discipline's criteria for judgment.

Although the organization of Bloch's first submitted version suggests big ideas, his tone is as cautious as he can make it. "A panoramic view of evolution offers clues that can serve as a guide in ordering the early stages." The tentativeness of the diction balances the enormous claim; he finds "clues" and a "guide," not a demonstration. When he describes the model in his introduction, the verbs are almost all conditional ("*could* provide a configuration") and the claims tentative ("*is envisioned* as a hairpin structure"). His characteristic method of argument, here and elsewhere, is to survey a broad question, suggest possible answers, and argue against the alternatives until only his own view is left. He tries to give the impression of a balanced approach, but the responses of the referees indicate that he does not successfully avoid giving the impression that he has a prior commitment to one interpretation, that of common origins for tRNA and rRNA.

The mixture of boldness and caution in Bloch's tone is apparent in his presentation of what he told me was "gratuitous but suggestive evidence," a ratio, which he saves for last, involving the information content possible with the number of RNA nucleotides. As he puts it in the earlier version, "This is a tantalizing bit of numerology that evokes no ready explanation from current views of RNA functions or relationships." On the one hand, he is claiming to introduce a new view of the evolution of life; on the other he injects his characteristically self-mocking tone with "tantalizing" and "numerology."[8] A conclusion that Bloch added in the version after this one can serve as an example of the style of much of his writing. "The scattered homologies are likened to the shards with which the archaeologist reconstructs pottery of ancient civilizations." The awkward sentence structure shows how hard it is to work this simile, which Bloch used often in his oral

8. For a comment on this term, see M. Gossler, "Numerology," *Nature* 306 (1983): 530.

presentations of his work, into the passive constructions of the scientific article. We could treat this mixed tone, like the exploratory organization and metaphorical conclusion, as a tactical error on Bloch's part, perhaps as evidence of inexperience. But since we know he could and did write straightforward research reports earlier in his career, it seems reasonable to take these departures from form as assertions that his claim is important enough to justify some background for the argument, some speculation in the conclusion, and some personal style in the presentation.

Whereas Bloch's departures from the form of the research report can be seen most clearly in the structure of his manuscript, Crews's departures from the form of the review article are largely a matter of tone and emphasis. And he is aware of the effect of these departures, again because of his immersion in a network that gives him responses before he submits a manuscript. As I have suggested, Crews was guided in his reframing of the draft for publication by the marginal comments, sometimes quite acidic, of a number of his colleagues. For instance, one reader points out, "It takes a long time (many paragraphs) before you get to the *new concepts*," and responds to an "indirectness" in the argument by proposing "a different strategy of organization" which he describes clearly, and which Crews adopts. Another reader raises potentially troublesome questions about the physiology of a particular species, giving the kind of detailed argument one seldom sees in a referee's report. A graduate student working at another lab where Crews has contacts compiles a three-page list of ambiguous phrasing and terminology. In each case, the reader defines part of the potential response of the zoological and endocrinological communities, before Crews submits the manuscript for judgment. All these different styles of handwriting in the margins of various drafts are the visible sign of the invisible college.

Despite these suggestions, Crews's first submitted version shows some tension between what he wants to say and the review article form in which he must say it. We can see these tensions by comparing the tone of some passages of this manuscript with similar passages in an earlier review article that Crews published in *Science* in 1975, when he was a postdoctoral student and was perhaps more cautious (Appendix 2). A review article typically summarizes the recent work of a research program, drawing on a broad survey of the literature, tactfully and impersonally presented. So the 1975 article has the unthreatening textbook-like title, "Psychobiology of Reptilian Reproduction." But the title of Crews's 1983 manuscript says he will give "New Concepts in Behavioral Neuroendocrinology," challenging the work of this research program. The earlier article begins with what might be considered the stereotypical opening sentence of a scientific review article: "The interaction of behavioral, endocrinological, and environmental factors regulating reproduction has been the subject of intensive investigation in recent years." The diction of the first sentence of the new article is provocative and even combative: "Much of the information on the causal mechanisms of vertebrate reproductive behavior has been gathered on *highly inbred stocks* of rodents and birds living under *artificial conditions*. . . . Some of the organismal level concepts that have emerged are *overly narrow* and *sometimes unrealistic*" (emphasis added). I assumed that the phrases I have emphasized would be red flags to other naturalists: he is saying that they are studying something unnatural. He uses a vocabulary with contrasting connotations to describe his own work; he proposes to investigate "species diversity under naturalistic conditions" and quotes comparative biologists who say such an approach leads to "new insights" and "new paradigms of thought." He is particularly bold in attacking the most commonly studied species as well as the most commonly held ideas; psychologists have money, time, prestige, and egos invested in their laboratory animals, and might respond more fiercely to attacks on their mice than to attacks on their minds.

A similar sharpness of tone is apparent in a comparison of the conclusion of the 1983 manuscript with that of the 1975 article. The earlier article ends with a concession to the competing research program in a subordinate clause and a conventional reference to the continuing advances of the field: "Thus, while the utilization of inbred species contributes greatly to our understanding of the factors regulating reproduction, the integration of these factors can only be appreciated fully in an ecological context where the adaptive significance of such interactions become apparent." The 1983 article ends with a statement of a similar idea, but frames it in terms of a call for more work on the whiptail lizard (*his* species), so that "it becomes possible to apply evolutionary theory to gain insight into the evolution of psychoneuroendocrine mechanisms." The earlier article stresses uses of *our* knowledge, whereas the later manuscript suggests that a whole new approach has been overlooked by most workers in the subspecialty.

From a rhetorical point of view, we might argue that the tone of these sentences is a strategic mistake, a miscalculation of his audience. But seeing the article in terms of a negotiation, we can see his tone as an assertion of the value of his knowledge claim. He is saying that this article differs from the views of most neuroendocrinologists, but it is still important enough for the front section of *Science*. A more

cautious article presented as a review of current knowledge, with a title like "More on the Psychobiology of Reptilian Reproduction," would be more appropriate in tone, but less important to nonherpetologists, and thus less appropriate for *Science*.

Many of the reviewers' comments are concerned with the departures from standard organization and tone I have described. For instance, all Bloch's reviewers comment on the length of his manuscript, even though he revised it, after the first rejection, to fall just within *Nature*'s word limits, pointing out that it is "about 2,930 words" (*Nature*'s limit is 3,000). As with most academic journals in which space is at a premium, appropriate length is determined, not by the limits given in the "Instructions to Authors," but by the importance granted to the article's claim; these reviewers do not think Bloch has earned 3,000 words yet. The most telling criticism of Bloch's style comes from his most enthusiastic referee at the journal that finally publishes his paper, a reader who seems to worry that Bloch's persona will endanger the reception of his work. "If the author is to have his observations seriously evaluated by others in the field, it is important that he not present himself as being overly speculative. Discussions of 'shards' and extremely speculative ideas such as Figure 5 [his original model] and those beginning at the bottom of page 7 [interspecies comparisons that form the basis for his current work] will not improve the author's chances of being taken seriously at this stage and would best be removed." This response shows that Bloch is perceived as a newcomer to the field, whose use of personal metaphors, asides, and "notions" is inappropriate "at this stage," and who may need guidance on his presentation. Perhaps a more personal and expansive style is permitted to those whose work has already been recognized.

Some of the rather vague criticisms from reviewers of the form and style of Crews's paper also seem to be directed at his departures from convention, in this case the format of the review article. For instance the more favorable referee of the first version says "the manuscript is not well-written." It is always hard to know exactly what this sort of comment means, but if we read on we find the more definite criticism that "a review paper of this nature which has pretensions to generalization should not be based on a preliminary review of the literature!" The meaning of "preliminary" here is relative; the 1983 article has more references than the review article published by the same journal in 1975, and probably has, already, more than *Science* wants to print. The problem is, perhaps, that a review article must not be so much a

review of one's own work, in which the work of others serves mainly as a background; the reviewer could be objecting, not to the number of citations, but to the emphasis implied in the organization. A favorable reviewer of the second manuscript submitted to *Science* shows, as we saw in Bloch's case, that criticisms of form—especially length—can sometimes be interpreted as attempts to redefine the claim. "A short paper will be read more often—a point briefly made is often the point well made." This may be good advice for any academic author, but the specific passages the reviewer would like to cut suggest the reviewer is more concerned Crews will alienate readers than that he will bore them. The garter snake sections can be deleted because "anyone interested in reproductive biology must have noticed the article by Crews in *Scientific American* a few months back." This change, like the comment on the "preliminary" review, can be read as an insistence that he move the emphasis from his own work. The reviewer also suggests that the whiptail lizard sections should be cut because they are controversial. "Female mating in the wild has never been observed (judging from a heated discussion by *Cnemidophorus* workers after a seminar by Crews at a recent ASZ symposium.) . . . Personally, I think Crews is on shaky ground here, and there would be great danger of a hostile reaction to an otherwise important contribution." The reviewer does not attack the *Cnemidophorus* work directly, but can rely on this vague consensus. The work is inappropriate in a review article that claims to represent the work of the specialty, not because it is wrong, but because it is the author's own claim and has not yet been accepted by others working on the same animals.

Negotiating Form and Style

The changes the authors make in various revisions in response to these reviews show they take these apparently superficial matters of organization and style as issues affecting the status of their claims: they make most of the changes suggested, but reluctantly. Bloch described his revision of the article for *Science*, after he read the *Nature* reviews, as "cutting some of the speculation and adding some new data." This he certainly did, extending his list of matches and including his recent reading in the reference list. He also changed his self-presentation radically, becoming the judge of, rather than the advocate for, the claim for the common origins of tRNA and rRNA. The title, "An Argument for a Common Evolutionary Origin of tRNAs and rRNAs" becomes "tRNA-rRNA Sequence Homologies: Evidence for a

Common Evolutionary Origin?" The new title puts the data first, changes an *argument* to *evidence*, and strikes a note of skepticism with the question mark.

The structure of Bloch's new version moves much closer to the structure of a conventional research report. Comparison of the first version submitted to *Nature* and the revised version submitted to *Science* shows a reorganization along inductive rather than deductive lines, moving toward the conclusion of common origins rather than from this assumption. The first four pages of the review of theory are cut, as are the last two pages on prospects for "further evolution." The article now begins with the homologies, lists some of them, and gives his methods. The formula for determining the significance of these homologies, his link with a recognizable line of previous research on sequences, is now on page 2 instead of page 6. In general the exposition is tightened, introductory sentences are added, a few asides cut, and some sections are moved from "Results" to "Methods," where they flesh out that previously rather skimpy section. Bloch split the section on whether the homologies result from convergence to take into account both convergence and function, showing a new refinement in his argument and recognition of a body of literature on sequences. The section on convergence, his refutation of a counter-interpretation, is shorter, and ends cautiously saying that a larger data base is needed. The model that was first is now last; it occupies only a paragraph, and comes with no elaborate explanation of the conditions it satisfies. The article is six pages long instead of twelve, with ten notes instead of sixty (*Science* discourages "exhaustive" reference lists). But only notes indicating the sources of sequences are cut; all the substantive references to related work by others are retained.

Bloch's revisions for the *Journal of Molecular Evolution* continue the reorganization into more conventional format, with an emphasis on the data, and into a less personal and less assertive style. The introduction emphasizing the significance of his findings is cut, and a short summary of his method is put in its place. In the first sentence, where before rRNAs were "peppered with stretches" homologous to tRNAs, now they "were found to contain stretches. . . ." In a gesture toward consideration of both sides of the data, suggested by a referee at *Science*, he adds a new table showing the tRNAs that *don't* have homologies. The conventional heading "Discussion" replaces the earlier, less formal heading, "Why the Homologies?" He finds more arguments against the possibility of coincidence or horizontal transmission and supporting the concept of a multifunctional molecule. He

makes the tone even more cautious: "We propose that" becomes "one interpretation would be that. . . ." Finally, the model is deleted entirely from the text and relegated to the caption of figure 5, and the sentence on how the model led to the finding of the homologies, the last relic of the narrative of his thought that Bloch gave in his first version, is deleted.

After the review at the *Journal of Molecular Evolution*, Bloch makes nearly all the changes the reviewers and editor suggest. He adds a numerical example, some figures on the quality of the matches, an example of his calculation of the possibility of coincidence, and references to possible DNA-level transfections. The tone becomes still more cautious; an "acceptable" level for excluding homologies as coincidental has now become a "provisionally acceptable" level, and where he had said that these coincidental matches "will be revealed" by further comparisons, now he says they "should be revealed." He admits a possible weak point of his method of argument, that "the evidence so far has supported homology only by eliminating or weakening arguments favoring alternative explanations." And finally he deletes figure 5, which was criticized by the reviewers, and with it all trace of his model.

In addition to the changes suggested by reviewers, Bloch makes an apparently minor formal change suggested by the editor that is relevant to the position of the article in the literature, the same change from *homology* to *matching sequence* that we saw in the proposal. The editor had commented on Bloch's use of the term "homology," a complex term that usually means a *common sequence* in molecular biology, but means a feature resulting from *common origins* in evolutionary biology.[9] The editor said he had long had a policy of trying to keep the word univocal, and argued that use of the word in its more general sense marks an unnecessary division within the discipline: "molecular biologists have to be biologists too." Bloch was happy to agree and change his use of the term; otherwise he would be begging the question in arguing that the homologies showed common origins. However, he also adds a note saying, "Their distributions suggest . . . that they represent true homologies."

The concluding metaphor of the shards is gone, alas. Instead Bloch ends the published paper with another metaphor, that of "filling in the map." But this, he tells me, refers to "a phrase used back in '55 by Benzer, describing filling in the genetic (linkage) map with mutants,"

9. Mayr, *Growth of Biological Thought*, p. 465; M. Norell, "Homology Defined," *Nature* 306 (1983): 530.

so it is an allusion to a tradition in the field, not an assertion of a personal style. We may, however, see a personal style in another sort of figure added by Bloch to the final version of the paper, a diagram related to his current work on species comparisons. The version in print is full of tables and graphs; as a colleague said, "every day he thinks of some new way to illustrate it." His graphic figures may in some way replace the figures of speech he has had to cut; in fact, his figure 2a, showing where various matching sequences are located on a conventional diagram of tRNA, is the equivalent of his shards metaphor. Both kinds of figures provide visual images that represent selected features of highly complex data. In these terms, Bloch gradually changes his figures to the kind more acceptable to the *Journal of Molecular Evolution*.

Crews's revisions of the first version submitted to *Science* also show some concessions to the views of the referees and the conventional form, with its implied placement of the claim in the context of the literature, in order to get his claim in print. In his first revision for *Science*, the accounts of his own studies are shortened and subordinated to the work of others. This version is more readable: digressions are deleted, especially near the beginning, transitions are added, some supporting but complicating details are moved to the notes (he now has eleven explanatory notes instead of two), and a concluding restatement of the argument replaces the appendix-like anticlimax of the earlier draft. He says in his letter asking for a second review that these changes make this version more "straightforward," but these changes affect the persona of the article as well as its readability, for the article now makes a sharper claim and makes fewer demands on the reader.

I can draw no line of demarcation dividing the changes Crews is willing to make from those he is not. But in general he is acutely aware of how his tone defines his relation to the work of others, and he is willing to change this tone wherever necessary. He is unwilling to modify his inferences from his evidence, preferring even to cut sections and use them in other articles rather than compromise his argument. The change in tone at this stage is suggested by the title; the assertive "New Concepts in Behavioral Endocrinology" becomes the descriptive "Functional Associations in Behavioral Endocrinology: Gamete Production, Sex Hormone Secretion, and Mating Behavior." The provocative opening remark about other researchers' "highly inbred stocks of rodents and birds" becomes a milder comment on "laboratory and domestic species." Where before, in the summary attacked by one reviewer, he said "this survey makes several points", now it

"raises several questions." He adds a cautious note in saying that the lack of dependence of mating behavior on hormones "may be more common in vertebrates" than previously thought. Other changes in tone are apparent in his changing "my laboratory has been investigating" to "the most thoroughly investigated species . . ." and in his phrasing of an assertion in the form, "it is important to restate the obvious." He responds to a reviewer's criticism of his "really very simple model" by pointing out that "the four reproductive tactics . . . represent extremes." He is careful to incorporate the "existing body of knowledge" referred to by the other reviewer, reminding the reader that many species *do* follow the conventionally accepted pattern. An example of his avoidance of confrontations (and witty understatement) is his mention of his most controversial point, the relevance of all this to humans, only in a note. By softening the confrontational tone of the earlier version, Crews includes his readers on his side of the argument, whether they belong there or not.

Although Crews backs off in matters of tone in this resubmission to *Science*, he mounts a counterattack in the form of his argument, adding a great deal of material. First he establishes the paradigmatic status of the concept he is attacking in a new transition: "the concept . . . has persisted despite an increasing number of studies revealing variations to this rule." Here he adds a number of counterexamples and then asserts, cautiously, that "It is possible that the rule . . . may be due to a bias in the species most studied." Since his associated/dissociated dichotomy was considered too simple, he adds more examples to develop it in detail. The brief comment that was called "a quantum leap from faulty premises" is expanded into four paragraphs. Another comment that had been criticized, on explosive or opportunistic breeders, is moved from the beginning, where it seemed an aside, to the end, where it is introduced as "a classic example," well known to all.

The article can no longer be called "a preliminary review of the literature," and if it is to become, in the words of the reviewer, "a straw man," it is well stuffed. The revision is only one page longer, but whereas the earlier version had 57 references, the new one has 195, far more than is usual in a *Science* review article. The considerable changes show again how Crews's place in a research network of zoologists, psychologists, and endocrinologists allows him to respond to critics in his revision. The earlier version had listed twelve readers, mostly colleagues in the zoology department; the second lists thirty-one, mostly from other departments and schools. And this list includes only the actual readers, not those who raised questions or

made criticisms and suggestions after the many lectures he gave while he was revising, or those who talked to him in the halls, on the phone, and after work at a Mexican restaurant. Any writer can cut the parts of an article criticized by reviewers, but perhaps only a writer who argues his claim every day can rebuild the article on broader foundations of evidence in a period of a month.

This remarkable flexibility continues in the five major revisions Crews made between the *Science* version and the one for *Nature*. At first, like most authors, he tried to make a minimum number of changes, using his word processor to change all occurrences of *behavior* to *behaviour* for the British journal, cutting the subtitle, which exceeded *Nature*'s limit of eighty characters, and adapting his references to its style sheet. But in later drafts, after many more readings by colleagues, he made what he considers "wholesale cuts." What finally emerged is a very concentrated version of six and a half pages (of which three and a half are devoted to a figure, a table, acknowledgments, and references), with the new, catchy, headline-style title, "Gamete Production, Sex Hormone Secretion, and Mating Behaviour Uncoupled." The introduction is gone, and the paper begins immediately with the argument. Following a favorable reviewer's suggestion, almost all examples are relegated to the table, only two sentences are left on Crews's garter snakes, and his controversial *Cnemidophorus* studies are deleted entirely. One important sentence is added, making the current view that he is attacking seem one-sided: "Indeed, all of the data supporting this paradigm have been obtained from species in which both sexes exhibit an associated reproductive tactic." Now there are just fifty-two notes; significantly, only five of them refer to work done in his lab, and the first of these is carefully placed far down the reference list. The evolutionary argument the *Science* reviewer had called "a clever and reasonable hypothesis" is now apparent only to the reader who compares a statement on the second page (saying that the old view had supported phylogenetic conservatism of these relations) to the last sentence ("The possibility that similarities in the mating behavior in different vertebrate species [are] the result of convergent, rather than divergent, evolution, adds another perpective to our understanding"). As the form of the article has approached the conventional format, and the tone has become more cautious, the article has changed subtly from an attack on a paradigm by one scientist to an outline of the logical implications from the collective work of all the researchers in the field.

How do "*J Mol Evol* (1983) 19:420–28" and "*Hormones and Behavior* (1984) 18:22–28" differ from the authors' first manuscripts, "The Evolution of Evolution" and "New Concepts in Behavioral Endocrinology"? The claims in the published versions are at a lower level of the hierarchy, Bloch claiming only that matching sequences *may* indicate common origins, Crews claiming only that his comparisons show that gamete production, sex hormone secretion, and mating behavior may be dissociated in some species. In Latour and Woolgar's terms, they have had to add modalities and move their claims away from fact-like status. In Pinch's terms, the authors, in this evidential context, have to settle for claims of somewhat less externality than those they had first proposed. They have to leave out their models, and this could be a loss for them, because whatever words have been excluded at this point, as the article goes into print, cannot be part of the authors' claims. So if, for instance, molecular biologists not only accept Bloch's claim of common origins for these two molecules, but follow this claim to something like his model as well, this article would give him no way to assert his priority. (For this reason, he described the model in an abstract published separately.) We see this limitation of claim as well in the more conventional personae and forms the authors use in the published versions. These are, as one might have guessed, not so much fun to read as the earlier drafts, and not so clear to a nonspecialist, since they are highly compressed, are allusive in their references, and give none of the background or history of the claims.

Perhaps the most serious change in the articles, in practical terms, is that they now reach much more limited audiences than those the authors had hoped to address when they submitted their manuscripts to *Nature* and *Science*. This means not only that the articles miss whatever prestige an article acquires by appearing in those journals, but also that they are less likely to be seen, in Bloch's case, by the molecular biologists doing sequencing, and in Crews's case, by the wide range of zoologists. These are the researchers who, if they reoriented their research programs to pursue these new interpretations of published data, might provide more data to strengthen these claims: more sequences to check for matching tRNAs and rRNAs, or more animals for which the patterns of hormone levels, gamete production, and mating behavior are reliably known. But we should remember that Bloch and Crews are asking for a great deal. As with grant proposals, the selection process serves a social function. If we wanted to explain all aspects of the scientific community in functional terms, we could see in the relegation of their articles to more specialized journals an example of how the publication process works, protecting these researchers in other fields from just this kind of claim from outside their own research programs, and thereby preventing the capricious redi-

rection of goals, the proliferation of research programs, and the scattering of resources.

The process of publication of a claim does not stop with the acceptance of one article; both writers have other outlets for their ideas, at other levels of the hierarchy of journals. Here again we see a sharp difference between Bloch's opportunities and those of Crews, with their differing positions in their fields. Bloch tirelessly presented his papers on RNA sequence matches at conferences, and argued his views with important speakers visiting his department. In one poster session at a huge cell biology convention, he would repeat to anyone who was interested his whole case for common origins, drawing the listener along from figure to figure. If the listener seemed interested, Bloch might go on to his larger ideas about surrogation. Thus, in this forum he could have his own form and choose his own level of claims, according to the responses of the individuals who made up his audience. But his audience on this occasion consisted largely of friends and students, nearly all still working in his old field, and a few passers-by, perhaps attracted first by his lively illustrated bulletin board, many of them apparently graduate students with time for an intriguing, if odd, idea. Bloch put a great deal of preparation and energy into these presentations, and was happy with the chance to persuade anyone, but it seemed unlikely that he would persuade in this way the powerful molecular biologists whose interest he needed.

Bloch found another outlet for his model in a very short version of a paper delivered at a European conference on the Origins of Life, the proceedings of which were then published. In this unrefereed outlet he was freer to speculate, as the less cautious title suggests: "tRNA-rRNA Sequence Homologies: A Model for the Origin of a Common Ancestral Molecule, and Prospects for Its Reconstruction." This gave him a citation he could use in proposals and in other manuscripts to refer to his model, and a priority claim for the idea of primitive multifunctional RNA, should the idea be widely accepted. But even he found it rather too compressed to be easy reading. And he discounted the authority this publication would have for the experimentalists he needs to reach; with some praise and self-irony he called the origin-of-life people (among whom he counted himself), "a bunch of nuts." He said that later when a more detailed paper that included the model was published, the early paper would be superseded.

So Bloch continued to try to find outlets for the parts of his work cut from the published article and for the data and theoretical refinements that emerged after the final version of the *JME* manuscript. He submitted to *JME* a second article arguing that interspecies comparisons would show evolutionary convergence, and a third article with the details of the model, but these were both rejected. Then his collaboration with the physicist at the Center for Statistical Mechanics, Apolinario Nazarea, led to a manuscript in which "second order spectral analysis is used to depict rigorously and to characterize principal periodicities in the positions of conserved sequences common to tRNAs and rRNAs." Note that the last phrase, "conserved sequences common to tRNAs and rRNAs," takes as proven, not only the existence of matching sequences, but also the explanation that they are due to common origins. But, as the earlier reviewer's comment about publishing the data first suggests, articles like this had to wait until his earlier findings were known enough to serve as the basis for new problems. The publication of the *JME* article was helpful, but it did not become a breakthrough that would open further outlets for publication; it was not immediately cited and did not immediately become a part of the literature on molecular evolution.

For Crews the question is not so much whether an article can be published as where. Even though, as we saw, he cut out the first half of his manuscript before sending it out, and finally published an article six and a half pages long, he has been able to publish most of what he wrote. The material on his own work, which he cut to place more emphasis on the field as a whole, appears in two articles in an issue of *BioScience*, a glossy but rather serious popular biology journal. He was guest editor of this issue, chose seven articles (including his own) on similar comparative research, and used the forum to make his polemical methodological point about the importance of studying atypical species. Crews has also edited a book gathering together studies that show alternative reproductive tactics in a wide variety of species in all the major categories of animals, *Psychobiology of Reproductive Behavior: An Evolutionary Perspective*. This, too, is a kind of outlet available only to researchers who are already well established in a network of other researchers, to whom they can turn for work parallel to their own.

Though the theoretical implications of Crews's claim were cut from the *Hormones and Behavior* article, or at least well hidden, he was able to present them undiluted in a paper for an unrefereed but not unprestigious forum, an invitational symposium at the Kinsey Institute (1985). For this audience of physicians, psychotherapists, and other researchers interested in sexuality, an audience that did not need to determine the status of his claims or place him in the literature of neuroendocrinological research, he could be as assertive as he was in earlier drafts. The argument had become more cautious since then, and

is supported by all the additional data gathered during his revisions. But the tone, even in the abstract, is like the tone of "New Concepts in Behavioral Endocrinology": "The great diversity in reproductive tactics . . . has been unappreciated by behavioral endocrinology," and "the deterministic paradigms in behavioral endocrinology are overly narrow." Where in the *Hormones and Behavior* article the evolutionary ideas were held until the end, here they are emphasized from the beginning. From the out-takes of the *Science* version he gets a sentence on the evolution of regulatory mechanisms, lists of exceptions to the paradigm and of animals with associated or dissociated patterns, and descriptions of his own work. Two pages on the development and evolution of functional associations at the end of the *Science* submission, which had been the focus of criticism from the more hostile referees, are here expanded into six pages. The added pages make explicit the way his claim applies to other levels of the study of reproduction, so the place of the *Cnemidophorus* in this program is now clearer.

A particularly telling difference between the Kinsey talk, for a general scientific audience, and the *Hormones and Behavior* paper, for an audience of neuroendocrinologists, is in Crews's use of the scientific literature. He begins the Kinsey talk with a motto (a practice common in reviews by elder statesmen, but not usual in a scientific paper) from an article dating to 1946, and he refers prominently in the introduction to insights from masters in the comparative field, often from texts twenty to forty years old. The quotations seem to be a part of the persona he is developing here; on the one hand he is an outspoken dissenter from the rigid paradigm of neuroendocrinology, but on the other hand he is the inheritor of a rich tradition of comparative work. Although such self-presentation is not encouraged by the review article format, it is appropriate in this oral presentation to an audience of nonbiologists, for whom he must represent biology (he is the only biologist there) and also present something lively, new, and relevant to their own work with humans.

Finally, almost all of Crews's first article appeared in print. But it appeared in five separate texts, for four separate audiences, and was inserted into the structure of scientific facts in four different ways. For Crews as for Bloch, the way his text enters the literature is crucial in determining the eventual status of its claim. Whether his claim or Bloch's becomes a fact depends on how the articles are used by other researchers. But the form of the claims has been set; even if we don't know what the response of the research community will be, we know exactly what they will be responding to. What is not printed cannot be cited.

Though it is still too early to judge the fate of Bloch's model of molecular evolution and Crews's model of diversity in reproductive systems, the first responses to the articles can be taken as an ironic postscript to my story, and as a reminder that a bumpy ride from the referees and publication in a specialized journal do not necessarily mean that an article will remain in obscurity. Two months after Crews's article appeared in *Hormones and Behavior*, an editor of *Science*, interested in the issues it raised, called and wrote to Crews to offer an official invitation to write a longer review article on this topic. Even after all his experience revising the *Hormones and Behavior* article, Crews rewrote his submission drastically several times, and had his coauthor Mike Moore (with whom he had developed many of the ideas) rewrite it again after that, before it was submitted, reviewed (by two favorable reviewers this time), and accepted. When it appeared in January 1986 as "Evolution of Mechanisms Controlling Mating Behavior," he had finally published his main claim in the form in which he wanted it, in the journal to which he had originally sent it.

Bloch had a similar ironic turn of fortune. When he began his collaboration with Nazarea, he was skeptical about submitting their statistical analysis to *PNAS;* Bloch said in a letter that sending it to such a prestigious general science journal was "his idea, not mine." Rather to his surprise, it was accepted by this prestigious general science journal. The model appeared in *BioSystems*, which had invited Bloch to submit it after the origins of life conference. And soon afterward these two articles were discussed in a page-long news article in *Science* by Roger Lewin. A passage from the article shows how Bloch's work can be placed in the literature so that it is news, and disagreements are presented as controversy, rather than as rejection.

> Although their conclusion is not universally accepted, Bloch and his colleagues consider that there is sufficient reason to argue that the sequence similarities between tRNA and rRNA between the species reflect a common origin, not a recent convergence, and is therefore homologous [cites *BioSystems* article]. A similar, but much less detailed, suggestion based on comparisons of a tRNA and a small (5S) rRNA was made more than 10 years ago by James Lacey and his colleagues at the University of Alabama (2), but it was not extended to the larger rRNA's that are the basis of the Austin study.

The way Lewin refers to "the Austin group" and "Bloch and his colleagues" must have been a satisfying recognition that he did at last

have some people to work with, "someone to bounce ideas off." Bloch was preparing further articles when he died in the autumn of 1986.

The turnaround in the fortunes of both claims, so soon after publication, reminds us that, though historians can locate classic papers in retrospect, the success or failure of a claim seldom hinges on one article. Acceptance or rejection usually comes over the course of many articles, and last year's wild speculation may become this year's plausible hypothesis and next year's basic assumption.

Conclusion

What level of claim can I persuade the readers of this book to accept? At the lowest level, I am saying that scientists sometimes revise their manuscripts considerably to get them published. To support this claim, I need only show you the stacks of manuscripts. This claim, though on a very low level of externality, is significant in some evidential contexts, for instance, the context of technical writing teachers trying to convince their students of the value of rewriting assignments, or perhaps the context of scientists displaying to nonscientists the work that goes into a seven-page article. But for the audience I hope to address here, that is arguing on the level of Pinch's "splodges on a graph," the level of uninterpreted data. I can put the claim on a higher level of externality, to continue using Pinch's terms, by using these manuscripts to show that a scientific claim is socially constructed. But this claim, though it is in terms familiar to sociologists and historians of science, tells them nothing new. A more specific claim, that is likelier to tell the readers something new, is that the comments on and revisions of these manuscripts show one of the ways in which claims are socially constructed, that is, through the negotiation of the form of the article and thus the status of the claim. It seems to me that this claim may have relevance in two different evidential contexts, telling us about science or telling us about texts. In one context, these cases suggest that the process of writing and revision of articles has an important consensus-building function. We have seen how this process maintains the homogeneity of the scientific literature. We have also seen how it shapes the research itself, Bloch, for instance, putting more and more emphasis on his data. In another context, that of literary criticism, these cases show the relations between texts, within the genre of the scientific article. The question in this context is not how reality is transformed in texts, but how it is made by texts.

Like Bloch and Crews, I need more data to support my claims, so I address in this book an audience interested in scientific texts, though perhaps interested in them in other evidential contexts. I particularly need data from other disciplines and earlier and later stages of the publication process, about how persuasion is planned before a draft is written and how an article is read after it is published. From cases in other disciplines, it would seem generally true that a number of claims are possible from one line of research, and that disagreements about the status of claims do tend to focus on matters of appropriateness to the journal, organization and length, persona, and use of the literature. But I haven't yet seen enough descriptions of the publication process in the literature to know how far this description is useful. I see from Pinch's cases that negotiation in physics and biology are rather different; the biological arguments seem to involve the usefulness of alternative concepts for organizing large bodies of data that were collected for other reasons rather than the sort of crucial experiments that characterize the history of physics. For instance, it seems to be an acceptable response to Crews's argument from the mating of garter snakes to say, "that's just one species," whereas it is not an acceptable response to an argument from the perihelion advance of Mercury to say, "that's just one planet." We may find other characteristic differences between disciplines in the process of negotiating a claim and a published text.

The earlier and later stages of the process of writing a scientific article seem to be particularly appropriate for ethnomethodological and ethnographic approaches.[10] Historians of course, do not have access to the daily phenomena of a lab or to the immediate responses of readers, but they do have access to a wealth of written texts that may be more revealing than any direct observation.[11] The tendency of contemporary history of science to massively documented biographies of key individuals, although it may lead away from crucial sociological issues, is likely to yield insights for students of texts (the

10. Lynch's *Art and Artifact* and Garfinkel, Lynch, and Livingston's study of a tape of astronomers at work are particularly good examples of detailed documentary study of events and interactions preceding writing. See H. Garfinkel, M. Lynch, and E. Livingston, "The Work of Discovering Science Construed with Materials from the Optically Discovered Pulsar," *Philosophy of the Social Sciences* 11 (1981): 131–58.

11. Martin Rudwick's *The Great Devonian Controversy* is a classic study that may remain, for quite a time, the fullest possible use of texts as documents; Frederick Holmes, "Lavoisier and Krebs," has traced the development of a text by Lavoisier, and Charles Bazerman has begun a similar study of Newton's *Optics* in *Shaping Written Knowledge*.

enormous literature on Darwin is an example). For the later stages of a knowledge claim, after publication, there have been some interesting studies of readers' interpretations.[12] But to see these stages, one has to go beyond case studies of individual scientists and laboratories to the core set (Harry Collins's term), the group of researchers concerned with one research issue. The fate of a claim is not decided when it is published, even when it is published in *Nature* or *Science;* it depends on who reads it, how it is read, and how it is used.

12. Studies of readers' interpretations include Amman and Knorr-Cetina's forthcoming study of conversational responses in the representation of a gel in a molecular genetics article, Gilbert and Mulkay's interview material on the evaluation of experiments ("Experiments Are the Key"), and Charles Bazerman's chapter on physicists' reading in *Shaping Written Knowledge,* "Physicists Reading Physics."

Chapter Four

The Cnemidophorus *File: Narrative, Interpretation, and Irony in a Scientific Controversy*

A nonbiologist might expect, after reading the exchanges between reviewers, authors, and editors quoted in chapter 3, that the publication of Bloch's and Crews's articles, and the further publicity given to their claims in *Science,* would be followed by the sort of heated controversy familiar to readers of the *New York Review of Books* or *Critical Inquiry.* But this sort of back and forth exchange in print is not common in the scientific literature. The usual method of dealing with research claims one thinks are wrong is to ignore them; if they are not picked up by anyone, they will disappear into the morass of scientific publications. Citation analysts have often noted that negative citations are rare; the lack of any citation is a much more effective way of dismissing a claim.

Though printed exchanges are rare, controversies are quite common in science, probably much more common that the nonscientist imagines. Sometimes they concern priorities, when the research is perceived as a race toward a clearly defined goal. But more often they concern the definition of the goal of research, the conceptual framework within which work is to continue. These controversies are pursued at conferences, in phone conversations, in letters, in referees' reports and in implicit comments in articles. Just as the usually unnoticed dynamics of article reviews are clearest in the rare cases (like those in chapter 3) in which an article is repeatedly revised and resubmitted, the informal and implicit exchanges of scientific controversies are clearest in the relatively rare occasions when they emerge explicitly in texts. That is why controversies have been so important to sociologists and historians, like those described in chapter 1, who