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 want to open up the black boxes of science, to show the processes of construction of what we take as facts.[1]

I have a brown manila folder that contains reprints of articles by three groups of biologists, sent to me by David Crews with a note reading, "Controversy and how I have handled it." Taken separately the articles look unremarkable enough: reports of results in various studies of the behavior of lizards. But taken together they tell a story, at least to the biologist who labels them a "controversy." There is a crucial rhetorical difference between the strategies in such cases and those considered in chapter 3. The immediate audience in a controversy is not the editor or the referee who controls access to a journal, but the broader group of researchers working on whatever is defined as the problem, especially those researchers not already committed to one view or the other of the work at issue. They raise questions about how a writer can open up discussion of a scientific disagreement, how one text relates to another, how disagreement is finally resolved or ended, and how the fact that results is codified.

One way I see of approaching the questions raised by this file is by looking at the articles as showing the contruction, interpretation, and reinterpretation of narratives. They are not just evidence of a controversy; they are presentations of that controversy, of where it comes from, what it is about, and how it should end. (Even Crews's short note labeling these texts a controversy, and his selection of these texts, might be disputed by the other participants.) What interests me about this process is what happens before one story wins out and becomes *the* story.

By *narrative,* I mean the selection and sequencing of events so that they have a subject, they form a coherent whole with a beginning and an end, and they have a meaning that is conveyed by the sequence as a whole. If this seems an odd activity for scientists, it may be because we associated narrative with storytelling, fictions, and falsehoods. Even some critics who give narrative a more general definition insist on a distinction between narrative and the information that makes up scientific knowledge. For instance, Jean-François Lyotard refers to "the preeminence of the narrative form in the formulation of traditional knowledge" when distinguishing this traditional knowledge

1. The most detailed, and best written, of accounts of controversy is Martin Rudwick's *The Great Devonian Controversy.* See also Andrew Pickering, *Constructing Quarks;* Harry Collins's studies in *Changing Order;* Harry Collins and Trevor Pinch, *Frames of Meaning: The Social Construction of Extraordinary Science* (London: Routledge and Kegan Paul, 1982); Steven Shapin, "The Politics of Observation," and the introduction for general readers in Latour's *Science in Action,* which cites a number of other studies.

from knowledge in "the scientific age".[2] Narrative statements are taken as true within a context and a form; scientific claims are supposed to be true regardless of context, precisely because they have been stripped of context, of actors and processes.

This distinction between narrative and scientific argument has long been respected by literary critics; we saw in chapter 1 that when Dwight Culler treats the *Origin of Species* as a narrative constructed by Darwin, he is no longer treating it as science, and when Walter Cannon treats the *Origin of Species* as science, he dismisses the form as a distraction. But recently there have been a number of studies of narrative in scientific rhetoric, by literary critics, anthropologists, and sociologists of science. These approaches are quite different from each other, but it should be pointed out that none of them is a debunking exercise showing something unscientific in storytelling.[3] They are not showing the scientists doing something scientists are not supposed to do, because they are not assuming that there is a kind of knowledge stripped of narratives, to which scientists are supposed to restrict themselves.

The scientists themselves might see the controversy as working through arguments, rather than narratives. Arguments are supposed to work by reference to evidence and to the prescriptions of inductive reason, that is, by standards external to the discourse. But the power of narrative is based on its form, and this formal power is at work much of the time in this controversy. The biologists do bring in evidence, but it is effective, or isn't, because of the way they make the whole story fit together so that it has meaning; change one part and the whole meaning changes. We shall see a frequent tension between the author's assertions that the texts are arguments, and remain open to be shaped by still unknown facts, and the functioning of these texts as narratives that are persuasive because they are complete.

2. Jean-François Lyotard, *The Post-Modern Condition: A Report on Knowledge* (Manchester: Manchester University Press, 1984), p. 18.

3. For examples, see Beer, *Darwin's Plots;* Misia Landau, "Human Evolution as Narrative," *American Scientist* 72 (1984): 262–67; Jonathan Ree, *Philosophical Tales;* Bruno Latour and C. S. Strum, "Human Social Origins: Oh Please, Tell Us Another Story," *Journal of Social and Biological Structures* 9 (1986): 169–87; Woolgar, "Discovery"; Lynch, *Art and Artifact;* Gregory Myers, "Making a Discovery: Narratives of Split Genes," in *Narrative and Cognition*, ed. Christopher Nash (forthcoming).

The title of a 1985 television documentary, "Science—Fiction" by the BBC played on the ambiguity of such comparisons between science and other narratives, connecting the argument that science was a construction with the popular sense that it must then be false.

Though I will show how each article responds to a previous article, the process of interpretation I describe is not really a dialogue. Michael Mulkay in *The Word and the World* has analyzed an exchange of letters between biochemists with rival theories in terms of conversational analysis. He finds that the authors are aware of conventions of turn-taking parallel to those of dialogue, in which there is one speaker at a time, silences are avoided, and each turn is defined by the next in the sequence. But the exchanges of articles I am studying proceed on principles different from those of conversation (as they are different from the principles of the unequal exchanges between referee and author in chapter 3). The authors do not even address each other; instead they address those who might still be persuadable: a potential audience of herpetologists, comparative zoologists, geneticists, neuroendocrinologists, evolutionists, and ethologists.

The narrative on which this controversy is based is simple enough—one lizard climbs on the back of another, grips the pelvic region of the lower lizard in its jaws, and arches its back so that its cloaca is under the cloaca of the lower lizard. As it happens, all the biologists studied agree that they have seen this sequence of events. But they disagree about its interpretation, about the context in which this narrative should be placed, how it figures in other narratives, such as the story of a career, or the story of the research field as a whole. The significance of the biologists' reinterpretations will be clearer if I give some background to the controversy. But since the background is just what is at issue, I shall present two summaries of the issues involved, both of which explain the materials I have and either of which holds together on its own terms. In order to show the possible differences of perspective the more clearly, I have imagined the accounts of two opposed narrators instead of using the words of any of the biologists I am studying.

One Overview of the Controversy

The *Cnemidophorus* is a genus of lizard that is of special interest because it includes some parthenogenetic species, animals that reproduce from the eggs of the female without any males. Thus they provide an opportunity to study aspects of the control and evolution of sexuality that cannot be separated and analyzed in sexual species. Through the 1970s, researchers carried on increasingly large and sophisticated studies of a number of aspects of the physiology—that is, the bodily processes—of these species. But the researchers were not interested in the ethology—that is, the behavior—of these species. So, in their observations, they did not see anything odd about this behavior. David Crews, a young researcher then at Harvard, had been working on the reproductive processes of another genus of lizard, and became interested in the *Cnemidophorus*. So he brought a comparative approach to the field, and in particular an interest in how behavior is related to hormonal controls. He saw immediately what other researchers had ignored,[4] that these nonsexual lizards, who did not need to mate, sometimes mounted each other, behaving just like the sexual species of the same genus when they mated. And he set out to explain the significance of this paradox. But when he and his coworker Kevin Fitzgerald tried to publish these findings, they ran into personal opposition from established figures in the field who had, after all, been scooped. Their critics were in a position to block publication at the first journal to which the report was submitted. But it was finally published in the *Proceedings of the National Academy of Sciences*, a prestigious outlet. The article got an unusual amount of publicity, because of its implications for the nature of sexual behavior. Crews and his team of graduate students and postdoctoral fellows then continued with larger-scale studies that, in essentials, confirmed his earlier findings and opened up new perspectives for the comparative approach to the evolution of sexual behavior. What it all shows is how researchers get an investment in one approach to a research area, and then can't see another approach even when there is clear evidence for it. All one can do is keep adding to one's evidence and refining one's methods until all but the diehards accept the new paradigm.

Another Overview of the Controversy

Cnemidophorus are unusually interesting lizards, because they are among the few vertebrates that reproduce parthenogenetically. But they were hard to study at first. The key to *Cnemidophorus* research was the long and difficult process of learning how to maintain them in

4. These two accounts are intentionally slanted. Orlando Cuellar commented on the phrase saying that other researchers had ignored the behavior, suggesting that one could alternatively say "Crews recorded what other researchers had elected to ignore." He also pointed out some particularly slanted phrases in an earlier version, noting that, for instance, the first account, in describing Crews as "a bright young researcher from Harvard," implied "the other guys are old and dumb." But even where the account was not intentionally slanted, there were phrases that either Cuellar or Crews took exception to, showing how loaded an account of a controversy is likely to be. My vagueness about who criticized Crews could create the impression that all the critics I later mention opposed publication of the article; Cuellar points out that he did not review the paper and in fact did not see the paper until after it was published.

captivity. Over the course of about ten years, several research groups contributed findings to this procedure. Those workers who spent years going to the field and then trying to duplicate it in the lab gained a subtle understanding of these lizards—they could, for instance, tell from a photograph whether a lizard was diseased or abnormal in some way, and in a larger sense, they were always aware that a terrarium in Manhattan or Salt Lake City is not the Arizona desert. This work then began to pay off in a number of areas of biology, including genetics, evolution, and physiology, as researchers revealed the mechanisms of parthenogenesis.

At this rather late stage, a young researcher from Harvard, who had worked with a sexual species of lizard (one that is so easy to keep that it is a common pet), and who had just started work on the *Cnemidophorus,* asked an experienced researcher for some additional animals and some advice on maintaining them. The established researcher collected some animals for him and, more important, let the newcomer visit his lab and tap his expertise. But almost as soon as the newcomer got the lizards, and long before he could have gotten publishable results, he seized on a peculiar bit of behavior, noticed in a very few animals, and blew it up into a sensational claim. He saw some lizards mounting others and concluded from this that even unisexual lizards need to mate. Established researchers explained that they had seen such behavior too, but that they could recognize it as unnatural, an artifact of captivity, and so disregard it. The newcomer was encouraged to continue his work, but to wait until he had something more substantial to report. If everything the lizards did was blown up this way, there would be hundreds of articles published, but no progress on the important lines of research. The newcomer pushed his theory even after it was rejected by two reputable journals, and he finally got it published through the influence of a famous biologist who is best known for his work on insects. That would have been the end of it, but because it was about sex, and this newcomer has a genius for publicity, the article was picked up by *Time* as a sort of joke. So some of the established researchers went to the trouble of explaining in print why his theory was ridiculous, drawing on their own extensive records of the lizard's behavior. The newcomer still comes out with articles saying the same thing, but other *Cnemidophorus* workers have better things to do than to refute him again. What it shows is the effect on young researchers of the pressure to publish. This new *Cnemidophorus* researcher has certainly advanced his career. But in the end, this kind of sensationalism and haste doesn't advance science.

These contrasting accounts show what I mean by placing a narrative in context. First, narratives have meaning within other narratives, so that one's interpretation of the lizards' behavior may be linked to one's interpretation of a scientist's behavior, and that interpretation may be linked to one's interpretation of changes in the discipline. On the broadest level the story of a research *field*—in this case, the work by all scientists working on one genus of lizard—is made up of the planned efforts of separate laboratories, each of which can be seen as a separate *project*. The project is made up of individual *studies*, each of which is seen as a sequence of actions leading to a single claim that might be the basis for a published article. And these actions by the researchers are all focused on defining a sequence of actions by the *animals* themselves.

Each of the two overviews I have given moves from the field to the project to the study to the animals, and then back to the study and the project and the field to show the significance, or lack of it, of this narrative of the animal. In the first account, the observation of this behavior by the lizard leads to a study which is part of a larger project on the evolution of sexuality which, if it were successful, would reorient the whole field. In the second account, the lizards are performing the same sequence of actions, but these actions tell about the competence of the researcher, not about nature. The narrative of the study—the discovery or nondiscovery of a behavior—does not follow from that of the lizards, but is explained in terms of the project, which is based on the career goals of the researcher. In either story, the persuasiveness of the interpretation of the animals' actions depends on its place in a larger narrative.

The controversy also makes visible a context that includes all the levels of narrative: the world of texts. This context is usually not acknowledged explicitly: in scientific texts it is assumed that other scientific texts are transparent carriers of a meaning that can be concentrated in the abbreviations of citations. Though scientific texts always cite earlier work, they rarely quote the exact words of another text. As we will see, the participants in this controversy often quote phrases, both reinterpreting the words of their opponents and defining the meaning of their own. Thus the controversy is an arena in which we can see the biologists' own textual criticism.

A participant's reinterpretation of the narrative on one level can lead to an entirely different story on the other levels. There are, of course, many possible interpretations of these narratives, but in this controversy the interpretations come down to just two. When one researcher puts the narrative of another in a new context, its significance is not just altered, it is reversed. This is what I am calling ironic reinterpretation—the perception of a "true" narrative underlying the narrative given. For instance, if one can put a narrative of "normal" behavior in a context in which it is seen as "abnormal," the observation of the behavior is no longer a basis for meaningful statements about the species, but can only be used to raise questions about the correct procedures for maintaining colonies of laboratory animals. Similar reversals are possible with narratives representing the poles of experience or naïveté, open-mindedness or prejudice, discovery or artifact, data or hypothesis.

I shall analyze five published articles.[5] Crews and Fitzgerald published their first article on *Cnemidophorus* in 1980 in *PNAS*. Crews's two major critics, the best-known researchers working with this genus, took the unusual step of rebutting his arguments in print, and Crews and his colleagues responded in print to these criticisms. In 1981 Orlando Cuellar of the University of Utah, who in the early 1970s had shown the chromosomal mechanisms of parthenogenesis, criticized Crews and Fitzgerald in a postscript to a report of a long-term study of the reproductive rhythms of *Cnemidophorus*. Then two years later C. J. Cole of the American Museum of Natural History, who pioneered the physiological study of the genus, and his colleague Carol Townsend published a study of the mounting behavior intended to refute any claim for its reproductive significance, reporting cases and data from their extensive records. The conclusion to their article makes unusually explicit criticism of Crews's group's interpretations of *Cnemidophorus* behavior.

The tone of the postscripts of Cuellar and of Cole and Townsend suggest that the argument cannot be resolved simply by one side producing more data and convincing the other side, though both sides act as if it can. The controversy is over interpretive issues, not over the data. The Crews group did not think it enough just to publish more articles giving more evidence; they also responded to the criticisms directly on two occasions. The response to Cuellar is "Psychobiology of Parthenogenesis," by Crews, Jill Gustafson, and Richard Tokarz (the list of authors gets longer here, so I'll abbreviate this

5. The articles are cited in section 3 of the Reference list. An earlier controversy among *Cnemidophorus* workers can be seen in a review of the ecology and evolution by Cuellar in *Science* in 1977, and criticisms (or "Technical Comments," as *Science* classifies them) published in *Science*, with Cuellar's response, in 1978. This exchange confirms many of the textual features I have described in the controversy over pseudosexual behavior; one can trace in them a hierarchy of narratives, and see ironic reversals, and use of quotations, and the focus on the texts.

as CGT), published in an edited volume, *Lizard Ecology*. The main purpose of the CGT chapter is to present the behavioral inventory that was one of the stated goals of their research when they first began collecting *Cnemidophorus*. This would seem to be a purely descriptive and uncontentious project, but in this controversial context, the inventory that includes a category for "Sexual behaviors"is a challenge to his critics who question the relevance of the behavior Crews describes. CGT acknowledge the their work is subject to controversy only in their concluding section, "Is Pseudocopulatory Behavior in an All-Female Species 'Normal'?"

The response to Cole and Townsend by Michael Moore (then a postdoctoral fellow in Crews's laboratory, and now an assistant professor at Arizona State University) and Joan Whittier, Allan Billy, and Crews (MWBC), combines a report of new findings with a rebuttal of critics' arguments. It was published in 1985 in the same journal that published Cole and Townsend's article (not a usual journal for articles by Crews's group), and is recognizably part of a controversy, not only in its explicit references to Cole, and in its ironic reinterpretation of critics' articles, but in the intensity of its attention to methods and to theoretical assumptions. The sequence of articles in the file is:

1) Crews and Fitzgerald (1980)
2) Cuellar (1981)
3) Cole and Townsend (1983)
4) Crews, Gustafson, and Tokarz [CGT] (1983)
5) Moore, Whittier, Billy, and Crews [MWBC] (1985)

I shall try to show how the narrative of the lizard is constructed in the apparently nonnarrative account of Crews and Fitzgerald. Then I shall trace some of the interpretations and reinterpretations in later articles by focusing on five areas of disagreement, each linked to one part of the standard research article: the claim, the introductory review, the methods, the results and discussion, and the closing. These disagreements involve interpreting what the article said, placing the work in context in the research field, determining whether the study was competent, selecting and interpreting the evidence, and settling the controversy:

1. Each article gives some version of what the Crews and Fitzgerald article was saying, and there is disagreement about the selection and interpretation of the language of the claim.

2. The articles differ, usually in their introductions, in their accounts of the history of the field, and over the place of the Crews and Fitzgerald article in it.

3. The methods sections of the articles expand as previously insignificant details become significant.

4. The interpretation of results often turns on negative results: the lack of evidence that is assumed to indicate the narrative. So the two sides do not meet head on, but through arguments about auxiliary hypotheses.

5. Each article ends by trying to close the controversy—either by declaring the behavior meaningless, or by marking it as an accepted discovery and calling for further study.

Constructing a Narrative of Cnemidophorus *Behavior*

Before we can follow these reinterpretations of the narrative of the lizards, we need to analyze the construction of this underlying narrative itself. We cannot assume that Crews and Fitzgerald simply record a narrative existing in nature—that they are like traditional storytellers, retelling a tale told to them or evident to all observers. If this were the case, other researchers could have seen it immediately, and would have accepted it when it was described to them. The narrative is constructed in the writing of the text. So we need to see how the apparently static form of the scientific article can be used to say, "Once upon a time there were two lizards. . . . " It may seem that narratives like this one would occur only in biology, perhaps only in ethology, the study of animal behavior, and not in other sciences. But it is likely that other sciences deal with other actors, sequences and contexts that are less easily seen by the analyst because they are less easily anthropomorphized.[6]

The narrative on which Crews and Fitzgerald's article and the ensuing controversy is based is a sequence of actions shown in the caption and the photographs in the article's figure 1 (my Appendix figure A3.1). The four photographs combine four positions of lizards into a narrative. To see the photographs as a sequence, one must follow some unstated conventions of interpretation: reading the pictures in the order one would read words on a page, taking the two lizards shown in each of the photographs as the same two lizards, ignoring the third lizard in C, and ignoring the apparent difference in back-

6. For instance, Françoise Bastide of the École des Mines, Paris, presents a clever Greimasian analysis of a *Nature* article in which clay bowls of pipes are actants, in her unpublished paper on "The Semiotic Analysis of Discourse," and in "Une Nuit avec Saturne" she offers an analysis of science reporting in which a satellite is an actant. Bruno Latour treats chemistry texts as trials of strength in *Science in Action* pp. 88–89.

ground as the lizards move around the cage.[7] The caption is essential to our seeing this sequence as a meaningful narrative. First it describes the behavior we are to look for as "sexual" (in quotes). It adds action we cannot see here, such as "lunging attack." It focuses our attention on one aspect of each picture (such as the jaws and foreleg in A), and makes the position in the picture into an action (gripping). And it translates actions into the strictly limited vocabulary of terms used to describe behavior, such as "mounting and riding behavior" or "copulatory posture." This device of attributing terms will be important later in the controversy. Ethologists define a closed set of behaviors for each species; individual lizards may do all sorts of things but a narrative of animal actions, to have ethological meaning, must fit in this repertoire. It is important to recognize it takes work to make this behavior evident, because part of the controversy is over the conditions under which the behavior will or will not be seen.

As it turns out, no one will deny that the lizards can be observed in the positions shown in the photographs in figure A3.1; what they deny is that these actions fit together into a narrative of mating behavior, a meaningfully related sequence of actions. Crews and Fitzgerald must interpret the sequence if it is to be anything more than the random movements of caged animals. The abstract of the article tries to make the narrative meaningful by putting it in a larger context.

> ABSTRACT All-female, parthenogenetic species afford a unique test of hypotheses regarding the nature and evolution of sexuality.

7. Crews comments on the passage in which I analyze the photographs, "These are the same lizards, but as they move about in the cage, the background changes. You imply that I have manufactured the sequence." And he comments, where I say they assembled many different narratives of lizards, that it "implies we never saw the entire thing." I do not mean to say or imply that they fabricated evidence—that these were not the same lizards, or that they only saw part of the behavior at any one time. I use this way of analyzing their evidence to argue that all scientific evidence, even the most apparently straightforward, such as photographs of behavior, requires some interpretive work to make it into a narrative.

Some people who have heard my papers have pointed out that my vocabulary—*construction, narrative, negotiation*—might have connotations of fraud, and Crews's comment shows that scientific readers respond to these connotations. But this danger arises because traditional views of science contrast scientific objectivity, in which the researcher is totally transparent and passive, with fraud or incompetence, in which the researcher is active. I want to show that the production of any scientific knowledge involves social processes that do not fit in the traditional view of scientific knowledge; the traditional view provides no vocabulary for such processes. This only sounds like fabrication or fraud if one assumes that there is some scientific knowledge somewhere that does not involve social construction.

> Basic data on the behavior of parthenogens are lacking, however. We have discovered, from observations of captive *Cnemidophorus uniparens, C. velox,* and *C. tesselatus,* behavior patterns remarkably similar to the courtship and copulatory behavior of closely related sexual species. Briefly, in separately housed pairs, one lizard was repeatedly seen to mount and ride its cagemate and appose the cloacal regions. Dissection and palpation revealed that, in each instance, the courted animal was reproductively active, having ovaries containing large, preovulatory follicles, while the courting animal was either reproductively inactive or postovulatory, having ovaries containing only small, undeveloped follicles. These observations are significant for the questions they raise. For example, is this behavior a nonfunctional vestige of the species' ancestry, or is this behavior necessary for successful reproduction in the species (e.g., by priming reproductive neuro-endocrine mechanisms as has been demonstrated in sexual species)?

This abstract illustrates all the levels of narrative I have outlined: the narrative of the field at the beginning, of the study in the middle, and of the project at the end. Crews and Fitzgerald outline the basic narrative of the lizards in the sentence beginning, "Briefly, in separately housed pairs . . .". The key word in this sentence is *repeatedly;* to construct a narrative, Crews and Fitzgerald had to see many activities over the course of two years as one repeated behavior. A close look at the file shows that the individual animals they chose to exemplify this behavior in any text changed through the course of the study, as they got better examples. The article just notes the first observations of the behavior without details: "In late November 1978, intense social activity was noted in the cages, and daily observations were initiated." The correspondence suggests that this activity was the basis for the report in the first manuscript, sent out in March 1979. Although the published article makes the same claim as the earlier manuscript, the claim is now based, not on whatever was observed in November, but on a much larger number of animals collected in June 1979 and observed in July and August 1979. Observations of two other species are also described, without any dates given in the article. Their numbers are small, so the observations might not have been publishable in themselves, but after the *C. uniparens* narrative, they can serve as supporting data.

Crews and Fitzgerald make their interpretation of the basic narrative of the lizards' behavior by juxtaposing it in their study with two other narratives: the normal mating behavior of a pair of sexual lizards ("behavior patterns remarkably similar to . . . ") and the unisexual lizards' own reproductive cycles ("in each instance, the courted animal was reproductively active . . . "). In the article, the parallel with sexual species is made by the photographs in figure A3.1, showing unisexual lizards in what the captions interpret as the same positions as those of sexual lizards. (A later article puts three photographs of a pair of sexual lizards mating alongside three photographs of *C. uniparens* to make the point more explicitly.)

The second juxtaposition that turns actions into a meaningful narrative is made by a table that relates the behavior of animals to their reproductive state (my figure A3.2). This may not seem to be a narrative, since all elements of time are removed to make the various observations of lizards simultaneous. But by linking the reproductive state of each lizard to its behavior, Crews and Fitzgerald place the behavior, over the course of a few minutes, in the larger narrative of the reproductive cycle over the course of months. This narrative of the study depends on the narrative of the animal that was earlier created, for the malelike or femalelike behavior must be established before it can be related to reproductive state. The activity of Crews and Fitzgerald and their coworkers is confined to the notes to this table, which tell us, for instance, that they determined the reproductive state through three different procedures; dissection and observation of the size of follicles, or recording of the laying of eggs, or the palpation of the undissected animal. For the table to be coherent, these quite different procedures must be assumed to describe the same condition. The table creates a narrative by selecting details, as well as by organizing them. For instance, it shows body length (to demonstrate that it is not important) and the method of determining the reproductive state for each animal. The table is also significant for what it does not include. A critic of Crews's findings had asked about length of time the animals were in captivity, and about egg-laying records, and had asked for comparisons to lizards who had not engaged in this behavior, with the implication that the inclusion of these data might lead to a quite different story.

Each of these devices, the abstract, the figure, and the table, compiles a series of momentary observations of lizards into a narrative of the study in which the events take on a larger meaning. The narratives of the lizards and the narrative of the study are framed in the text by a narrative on the level of the research project: the creation of a discovery.[8] This narrative, too, requires selection and ordering. The

8. Woolgar, "Discovery"; Myers, "Making a Discovery."

first sentence of the abstract places previous work in the field in the context of Crews and Fitzgerald's own questions about "the nature and evolution of sexuality," rather than in the context of other research projects on the lizards' cytogenetics and ecology. The second sentence of the abstract identifies a significant lacuna in the literature, which Crews can fill: "Basic data on the behavior of parthenogens are lacking". They end the abstract with new research questions raised by his work. The discovery is presented as creating a new research project, one that will pursue the questions raised by the observation of this behavior.[9] Crews treats his work as if it were parallel to earlier studies by other researchers, citing their work prominently (as in the caption to figure A3.1) to show his procedures are unexceptionable. The irony of this reinterpretation is that behavior that other researchers had treated as insignificant—apparent mounting in a unisexual species—is now reinterpreted as significant. As one might expect, other researchers are not happy to have their studies reinterpreted in this way.

Reinterpreting the Narrative of Cnemidophorus *Behavior*

The Claim

The claim of an article, its main contribution to knowledge, is usually taken to be unambiguous, so that an unqualified reference like "Crews and Fitzgerald, Proc. Natl. Acad. Sci. USA 77 (1980) 499–502" can convey it. As I have noted in the introduction, a large body of sociological scholarship is based on the links such references make. But Nigel Gilbert has shown how a number of different sentences in an article could be taken as the claim. John Swales has summarized the research on citation context analysis and shown how these references serve a number of textual functions besides simply providing a structure of knowledge on which to base further work.[10] And I show in chapter 2 that the claim of an article can change in the course of revision and review. What we see in these texts is that the readings of a claim can vary widely among participants in a controversy. It is difficult to see this variance in most citations, because they do not quote the words they take as a claim; the particular words of a scientific article, unlike those of, say a literary critic, are not important in

9. See John Swales, *Aspects of Article Introductions* (Birmingham: Aston University, 1981), for discussion of these moves.

10. Gilbert, "The Transformation of Research Findings into Scientific Knowledge": Swales, "Citation Analysis and Discourse Analysis."

later references. In fact, quotations might nearly always be taken as a sign of trouble; something must be focusing attention on the text itself, which usually vanishes from sight in the accumulation of claims.

Cuellar's citation of Crews and Fitzgerald seems straightforward enough:

> Recently, Crew and Fitzgerald (1980) reported the discovery of copulations among several all-female species of *Cnemidophorus*, including *uniparens*, and proposed that such pseudocopulations may be necessary for successful reproduction in the species. ("Long-term Analysis," p.99)

Latour and Woolgar discuss modal shifts in presentation of claims in *Laboratory Life;* their point is that verb phrases like "reported the discovery of copulations" and "proposed that such psuedocopulations may be necessary" imply less of an attribution of fact than would phrases saying Crews and Fitzgerald "discovered copulations" and "showed they were necessary." To remind us of the agency of the researcher, the author, is to weaken the claim.

Similarly, Cole and Townsend's citation signals that they will question the claim by focusing on the exact words used by Crews and Fitzgerald. Cole and Townsend's use of the exact words is an example of what Dan Sperber and Dierdre Wilson call "echoic speech." Sperber and Wilson give this example: "*He:* 'It's a lovely day for a picnic.' [They go for a picnic and it rains.] *She* (sarcastically):'It's a lovely day for a picnic, indeed.' "[11] Cole and Townsend's use of Crews and Fitzgerald's words has a similar, if less obvious, effect of irony.

> Recently, Crews and Fitzgerald (1980) reported that captive females of unisexual species of lizards exhibit 'behavior patterns remarkably similar to the courtship and copulatory behavior of sexual congenerics'. Although other investigators had observed this also (Schall 1976, Werner 1980; Cuellar 1981; personal observations), only Crews and Fitzgerald (1980) suggested that homosexual behavior is normal in the reproductive biology of unisexual lizards. For whiptail lizards, they stated that: (a) 'In each instance', in *Cnemidophorus uniparens*, the female exhibiting malelike courting and mounting was 'reproductively inactive', (b) the female being courted was

11. Dan Sperber and Deirdre Wilson, *Relevance: Communication and Cognition* (Oxford: Basil Blackwell, 1986), p. 239.

> 'reproductively active', (c) such behavioural interaction was not seen among 'any females of sexual species', and (d) such interaction among unisexual species of *Cnemidiphorus* 'may be required for successful reproduction'.

Cole and Townsend select words of the Crews and Fitzgerald article to define the claims they will refute; they end this passage with the statement "Our own observations . . . contradict these suggestions." The implication of the quoted words is that Crews and Fitzgerald have stated their case much too strongly ("in each instance" "any females of sexual species") or that their terminology is vague ("reproductively active") or that they have gone out on a limb of hypothesis ("may be required for successful reproduction") and now they are going to get their comeuppance. Their claim is paraphrased as well as quoted; only Crews and Fitzgerald have "suggested that homosexual behavior is normal." The use of the word *homosexual*, which does not occur in Crews' articles, implies a sensationalism on his part.

As in some controversies in literary criticism, both sides treat the original text as unambiguous and the interpretations put on it by the other side as selective, overingenious, and transparently motivated. CGT respond to Cuellar's interpretation of Crews and Fitzgerald's language with some reinterpretations of their own.

> Cuellar (1981) has stated that Crews and Fitzgerald (1980) "proposed that such pseudocopulations may be necessary for successful reproduction," and others have echoed this statement. But this is a misinterpretation of that paper.

The disagreement shows that the reduction of a four-page article to a one sentence statement is not all trivial or automatic. CGT themselves reinterpret the context, the significance, and the strength of the claim in the earlier paper. First, they define it as an "initial report".

> The purpose of that initial report was to document the alternation of male-like and female-like sexual behaviors during specific stages of the follicular cycle in 3 unisexual *Cnemidophorus* species.

In calling it "initial," CGT imply that it is to be read in conjunction with the series of papers that followed it and refined it. CGT restrict the significance of Crews and Fitzgerald's original paper to its documenting that malelike and femalelike sexual behavior alternate. Then they offer a version of the claim:

> It [Crews and Fitzgerald] concluded that "It is likely that social interactions play an important role in in the reproductive biology of parthenogenetic *Cnemidophorus*" and raised the question of whether "this behavior may be necessary for successful reproduction in the species (for instance, by priming reproductive neuroendocrine mechanisms) as has been demonstrated for some sexual species." Obviously, Cuellar and others have chosen to interpret this to mean that Crews and Fitzgerald (and the present investigators) believe pseudocopulatory behavior to be essential for reproduction. We would like to emphasize that this is not our intention. Rather, we are suggesting that the presence and behavior of conspecifics may act as a neuroendocrine primer and facilitate reproduction in parthenogenetic lizards as does male courtship in sexual lizards.

The reading of Crews and Fitzgerald by CGT, like the readings by Cole and Townsend or Cuellar, is rather selective; CGT combine the first sentence of the last paragraph with the last sentence of the abstract to make the article claim that social interactions are important, and that they are important only in priming, in facilitating rather than causing reproduction. This claim is much weaker, and much easier to support, than the claim Cuellar atributes to them, that such behavior is necessary.

As Cuellar and Cole show, it is possible to get a number of other claims out of that paper. It is easy to see where Cuellar and others get their reading; the last sentence of Crews and Fitzgerald's article, taken out of context, says almost exactly what Cuellar says they say: "malelike sexual behavior in parthenogenetic *Cnemidophorus* may be required for successful reproduction." This seems to be a fairly definite answer to the question asked by Crews and Fitzgerald earlier in the article: "Is it necessary for sexual reproduction?" How, then, can CGT argue Crews and Fitzgerald don't mean pseudocopulation is essential for reproduction? They go on in another paragraph to define successful reproduction, not as the hatching of eggs, but as the hatching of eggs at a "normal" rate. Also they imply that to understand the claim properly one needs to consider the research context, as CGT do in the last sentence of the paragraph: "It has long been known that eggs laid by isolated unisexual lizards will hatch, (Maslin, 1971), a finding confirmed in our laboratory." It would seem that neither side really thinks the stronger claim—that sexual behavior is required for any reproduction—is being made, for neither side actually tests it with the simple experiment of keeping a lizard isolated and seeing if it lays and hatches eggs. Even Cole and Townsend, arguing this point, have to refer to such an experiment with an entirely different genus—not, presumably, because they couldn't show it with *Cnemidophorus*, but because such an experiment would, in the current context, be trivial. When CGT note that the point was established by Maslin ten years earlier, they imply that Crews and Fitzgerald could not have been ignorant of these established results, and could not have been contradicting them.[12] Crews and Fitzgerald couldn't have meant what Cuellar says they meant, because everyone knows that isolated eggs will hatch, so they must have meant something else. We shall see other examples of assumed contexts for the interpretation of texts in the controversy over *Sociobiology* (chapter 6). The interpretation of a claim depends on the place it is given in the narrative of the research field, especially in the version of this narrative given in the review of research that opens the article.

The Introductory Review

Each article in the file places the research of Crews and Fitzgerald in the larger context of the issues of importance in the research field. Crews and Fitzgerald, as we have seen, claim significance for their observation by showing it fills a gap in the literature, which had emphasized physiology and ecology but overlooked studies in behavior. The introductions of the Cole and Townsend article and the MWBC article show how the different sides of the controversy construct different views of the research field.

Cole and Townsend present a view of the field in which Crews and Fitzgerald are describing an artifact, not a discovery. Artifacts in ethology are somewhat different from those in, say, microscopy (which Lynch describes in *Art and Artifact*), where researchers try to determine which features of an image are the result of experimental manipulation or instrumental procedures (the artifacts) and separate them from the features of the image that are taken as a true representation of nature. The artifact is behavior that falls outside the narrative of normality—in this case, behavior that results from confinement in terraria in a laboratory, and thus does not reflect the way the animals behave in the desert. So Cole and Townsend grant in the first sen-

12. Strikingly similar disagreements about the claim can be found in the "Technical Comments" and response after Cuellar's 1977 *Science* review. For instance, Cuellar responds to critics' versions of his claims, which he sees as oversimplified, by referring, just as Crews does, to a well-known background of research against which the claims must be interpreted.

tence of their abstract that the behavior exists, but state in their second sentence that it is abnormal.

> Abstract. In captivity, females of parthenogenetic species of whiptail lizards (*Cnemidophorus*) occasionally mount other females and behave as if attempting to mate. This occurs under crowded conditions, and probably is not related to reproduction.

Cole and Townsend then go on to outline a history of the field in which Cole's own work is central, reminding the reader of his long experience with this line of research.

> The unisexual species of reptiles that reproduce parthenogenetically may be the only vertebrates in which individual females normally reproduce independently of males (Cole 1975; Hardy & Cole 1981; see Downs 1978 for possible examples in salamanders). Consequently, all aspects of their reproductive biology merit attention. Recently, Crews and Fitzgerald (1980) reported that captive females of unisexual species of lizards exhibit 'behavior patterns remarkably similar to the courtship and copulatory behavior of sexual congenerics'. Although other investigators had observed this also (Schall 1976; Werner 1980; Cuellar 1981; personal observations), only Crews and Fitzgerald (1980) suggested that homosexual behavior is normal in the reproductive biology of unisexual lizards.

Like Crews and Fitgerald's article, this acticle begins by saying that the animals are so important that one must pay attention to all aspects of research on them. But the reference to Crews and Fitzgerald is followed immediately with the qualification that their report is not a discovery, because the behavior has often been observed before, and that their interpretation of the behavior is idiosyncratic, a diversion from the main line of research, in which they have isolated themselves from the consensus of the field.

The title of the MWBC article—"Male-like behavior in an all-female lizard: relation to ovarian cycle"—has two parts forming a narrative of the research project, one taking the existence of the behavior for granted, as a given topic, and the other adding a new contribution. The first sentence defines a line of research conducted entirely by Crews's group: "Recent observations of copulatory-like behavior in all-female species of parthenogenetic lizards have emphasized the dual functions of sexual behavior (Crews and Fitzgerald 1980; Gustafson and Crews 1981; Crews 1982; Crews et al. 1983)." So far, one would not think this a matter of controversy, unless one notes that all the work cited was done in one lab—their own. The MWBC authors provide an ironic reinterpretation of the field—their own work as well as that of their critics—that must be exasperating to anyone trying to disagree with them. In it they simply assume pseudocopulatory behavior as a fact and as a term, and go on to the need for further studies of it.

> Copulatory-like behaviour in unisexual lizards was first described by Crews and Fitzgerald (1980). The occurrence of this behaviour, hereafter called pseudocopulation, has recently been confirmed by other workers (Werner 1980; Cuellar 1981; Cole and Townsend 1983). Crews and Fitzgerald observed that male- and female-like copulatory behaviour was exhibited in separate phases of the ovarian cycle: female-like roles occurred only during vitellogenesis and male-like roles occurred only during pre-vitellogenesis or after ovulation. This led them to hypothesize that individual animals alternate between male-like and female-like behaviour as the ovarian cycle progresses. Recently, this interpretation has been challenged by Cuellar (1981) and Cole and Townsend (1983). However, all reports so far have been descriptive studies, which employed small sample sizes, thereby precluding quantitative analysis.

MWBC incorporate the evidence given by Cuellar and Cole and Townsend into the Crews case, even though these critics gave the evidence only to show that the circumstances refuted Crews's claim. Note that in appropriating these findings, MWBC also rename them in terms of their own terminology, *pseudocopulation,* which Cuellar and Cole certainly would not accept. There is no need for the term if the behavior is merely an artifact of captivity.

MWBC separate the relation of the behavior to the ovarian cycle, which they take as "observed," from the claim that individual animals alternate roles that Crews and Fitzgerald were "led . . . to hypothesize." This hypothesis is the issue that MWBC will address and extend in the paper. On this "interpretation," the same authors who are used for support in the previous paragraph serve as antagonists. But all earlier articles, including those of Crews's group, are made preliminary to the present study, in which "we report . . . the first quantitative analysis of the relationship of copulatory-like behavior to ovarian states in a unisexual lizard." Thus they seem to make the earlier controversy over Crews and Fitzgerald irrelevant, based as it was on the insufficient data then available.[13]

Methods

One striking characteristic that sets these texts apart from noncontroversial articles is the expansion of the Methods sections during the controversy. The Crews and Fitzgerald article can get its methodological details in tiny print in the caption to its figure 1, citing Cole for further details. But the MWBC article, five years later, gives a remarkable amount of detail about the regimen of care, observational procedures, and categories of reproductive state. Latour and Bastide ("Writing Science") show how methods sections, usually thought by students to be dull formalities, become crucial in controversies. One would expect that, under the stress of controversy, studies might become more elaborate, and might be done on a larger scale to be more persuasive. But what also seems to be happening here is that the list of relevant information grows each time one side questions the technique of the other. As in the gravity waves work that Collins describes in *Changing Order,* in which some researchers were trying to refute an apparently bizarre claim, there is no objective standard for what would constitute replication or refutation of the original observations. A modification that may be seen by a researcher as a minor variation or an improvement in the apparatus may be seen by another researcher as invalidating the evidence of that apparatus.

When Crews and Fitzgerald say in their first article that behavioral data are lacking, they raise the methodological issue of what one has to do to see behavior, and thus focus attention on the skills of the established researchers in the field they themselves have just entered. Cuellar defends himself from the implication that he missed this behavior by referring to his long experience.

> During the last decade I have monitored the development and laying of nearly 1000 clutches from captive *C. uniparens.* Since my studies have required precise knowledge of ovulation and oviposition times, copulatory behavior would have revealed itself as a most conspicuous feature of the reproductive cycle of the species.

13. The 1977 controversy in *Science* also included an exchange between Cole and Cueller over which articles should form the basis for future work, both of them referring to their own review articles. See also the comments in chapter 6 on the construction and reconstruction of a research tradition in the sociobiology controversy.

The language is unusual for a scientific article because, as an examination of the subjects of his sentences suggests, it focuses attention on him and his studies, rather than on the animal. "Decade" is a word used in the article only to refer to the passing of human time, of careers, not to the cycle of lizards or the measured time of experiments. And Cuellar points out his "precise knowledge" of the reproductive cycle in a way he would hardly do unless he felt his observations had been challenged. His choice of verb defines a model of observation in which the object makes an imprint on the passive watcher; the behavior "would have revealed itself."

The Cole and Townsend article is in the form of a refutation of Crews and Fitzgerald. But instead of planning an experiment along lines suggested in the article they want to refute, Cole and Townsend reinterpret the data they had gathered for other purposes, and present this reinterpretation as the equivalent of Crews and Fitzgerald's study, or rather as an improvement on it. Indeed, it could be argued that there would be no point in a replication, since both sides agree that the behavior occurs, and they disagree only about its significance (Collins, *Changing Order*). In order to show that their procedures would not miss the behavior, Cole and Townsend must go into surprising detail, and like Cuellar, they refer to their own skills more directly than they might in a noncontroversial article.

> Lizards in our laboratory colonies of unisexual species have been reproducing since 1972 (Cole and Townsend 1977; Townsend 1979). Each animal is uniquely marked for individual recognition and notes are kept regarding genealogy, dates of hatching and death, oviposition, cagemates, and other observations. Although our procedures were designed to investigate non-behavioural aspects of reproduction, genetics, and systematics, we also recorded male-like behaviour among these animals whenever it was seen. Lizards judged to be gravid had a characteristically swollen abdomen and usually oviposited within a week of the observations recorded. Since most of these lizards were kept in one of our offices, they were under close, although casual, observation. We provided nearly all their care ourselves, and because they are diurnal, it is not likely that we missed much behaviour pertinent to this report.

Like Cuellar, Cole and Townsend stress the duration and detail of their observations, in contrast to the short term of Crews and Fitzgerald's work. The long list of categories in which notes were taken (such as genealogy) may not be entirely relevant to the research reported; it does not prepare the reader for their argument but stands as testimony to the detail of their study and to the selectivity of the records kept by Crews and Fitzgerald. It is unlikely that outside the context of public controversy they would need to mention that the lizards were kept in their offices, or that they cared for the animals personally. Twice they refer to the fact that they were not specifically looking for this behavior, but three times they comment on the closeness of their watch. That they can use their previously recorded observations in this new narrative at all shows that exactly the same events can be used to make two different studies in two different research projects.

CGT comment on methodology in a response to Cuellar that offers a reinterpretation of his research project in which his diligence and experience count against him. They take his observation of the behavior as confirmation, and make his failure to interpret the behavior as mating into an indication of his preconceptions. Again the quotation of a phrase signals an unusual use of the text of another researcher. In this case, CGT quote Cuellar because they are appropriating his findings as confirmation of theirs:

> In support of this interpretation, Cuellar states that he has "observed such behavior in *C. uniparens* and [unisexual] species in the laboratory for fifteen years, but only sporadically." But it is significant that Cuellar, as well as other workers, has observed male-like behavior in parthenogenetic *Cnemidophorus*. That these observations have gone unreported in previous studies should not be too surprising. Since the function of these courtship and copulatory behaviors is not obvious, these workers most likely felt that this behavior was an abnormal manifestation of captivity. Preconceptions, however, guide perception, and one does not very often see what one is not looking for.

The first sentence repeats Cuellar's ironic turn of Crews and Fitzgerald's discovery of the behavior—the behavior happens, but they have not discovered it and it is not normal anyway. Then they do an ironic turn on the ironic turn, holding that the important assertion in Cuellar's article is that he *did* see the behavior—so he confirms Crews and Fitzgerald in spite of himself. They propose that there is a need to explain why this behavior went unreported, why the observation was not, until Crews and Fitzgerald, defined as a discovery. And they explain, in the terms I am using, that the narrative of the project comes first, that one has to have an explanation in mind before one will see the narrative of the animal the way CGT do. They propose, not a methodology, but a philosophical rule for the whole field, a reinterpretation of the way researchers do their research: "preconceptions guide perception." This series of reinterpretations follows from the need to explain why experienced researchers would construct two different narratives for what these lizards are doing.[14] Cuellar and CGT are using two different conceptions of what makes a good observer, Cuellar saying observation depends on experience and CGT saying it depends on theoretical orientation. Both imply criticisms of personal scientific practice that are very rare in the form in scientific articles.

The methods section of the MWBC article shows that it is intended to refute Cole and Townsend, and not just supersede their data; it is much more detailed than earlier articles, and responds to the criticisms Cuellar and Cole and Townsend had implied. One peculiarity that suggests this methods section is a response to previous criticisms is the recurrence of the word *careful* when they say that "careful notes were taken" of every pseudocopulation among the experimental animals, "we also kept careful notes" on pseudocopulation among other animals in the lab, and "careful records were kept of egg-laying dates." The word carries no information, for one cannot imagine MWBC reporting that they kept *careless* notes, but it does make sense as a response to Cole and Townsend's claim for the detail of their records and observation.

But the methods section does not give so many details just to demonstrate their care; MWBC focus on details that have a rhetorical purpose in their response to Cole and Townsend. For instance, MWBC give a great deal of detail on their terraria, since Cole and Townsend and Cuellar had said Crews and Fitzgerald were observing an artifact of confinement. The procedures for care of the lizards from Cole's and Cuellar's earlier articles are cited. Exact dates and duration of observation are given (this is a point on which CGT had criticized Cuellar). Observation of behavior is for the first time related to ethograms, more formal and rigorous repertoires of behavior, suggesting that the issue of categorization of behavior ("basking" or "arm waving"), raised by the postscript to Cole and Townsend, has been resolved. MWBC also describe in detail their methods for determining and classifying reproductive state, which they argue will show a correlation where Cole and Townsend's gravid/nongravid distinction did not.

14. Gilbert and Mulkay discuss such explanations in *Opening Pandora's Box*, pp. 63–89.

The response to Cole and Townsend in MWBC's discussion makes it clear why their own methods section has become crucial and has become long. First they attack Cole and Townsend's methods of observing and classifying the state of the lizards:

> Cole & Townsend may have reached this conclusion because their method of assessing reproductive state by visual examination of abdominal distension is not adequate for making the critical (see Fig. 1) distinction between animals that have large yolking follicles and those with oviducal eggs. This distinction can be made only by palpating the abdomen as described by Cuellar (1971).

The methods for observing reproductive state that Cole and Townsend used for earlier studies are held to be insufficient for this new area of research; the preferred methods are supported by a reference to another of their critics.

MWBC's criticism of Cole and Townsend's omissions shows the rhetorical intent of the comments in their own methods section on cage size. In an ironic turn MWBC say Cole and Townsend neglect the data necessary to confirm their own hypothesis, whereas MWBC have given the relevant data in their methods.

> This conclusion [that the behavior has no effect on reproduction] is based solely on egg-laying records; no information on the reproductive history or social environment of their captive animals being provided. Cole and Townsend argue further that pseudocopulation is an artifact of crowded conditions in captivity, yet they present no data to support this hypothesis. They do not give the dimensions of the cages used, nor the number of animals housed per cage. In fact, by their own admission, their experiments were designed to investigate 'nonbehavioural aspects of reproduction, genetics, and systematics.'

When every detail of method is questioned as closely as this, both sides must present cases for each procedure they use. Previously ignored aspects of the study—what room the cages were in, who fed the animals, the size of the cages, which animals were in each cage—now become potentially significant.

Negative Results

One striking feature of the interpretations of the *Cnemidophorus* narrative is the weight given to negative results, not to evidence of the narrative, but to missing evidence that would be needed to support the narrative. This might be seen to support a Popperian interpretation, in which the rival researchers seek to falsify the hypothesis of Crews and Fitzgerald. But the ingenuity with which auxiliary hypotheses proliferate suggests that we are seeing a pattern of ironic rhetoric, a tendency to respond not by refuting, but by reversing the rival claim. The arguments do not generate new data on the same issues, but generate further issues.

Perhaps I can clarify what I mean by negative evidence by referring to the case study of a better-known researcher in a better-known controversy: Sherlock Holmes in A. Conan Doyle's *The Sign of Four*. In chapter 6, Mr. Athelney Jones, the hapless police inspector, is spinning out a narrative of the death of Bartholomew Sholto. He is, like all the police in these stories, totally off the track, and Holmes must set him right.

> "Ha! I have a theory. . . . What do you think of this, Holmes? Sholto was, on his own confession, with his brother last night. The brother died in a fit, on which Sholto walked off with the treasure! How's that?"
>
> "On which the dead man very considerately got up and locked the door on the inside."
>
> "Hum! There's a flaw there . . . " (p. 189)

The narrative proposed by Jones requires a door locked from the outside; that it is locked from the inside indicates the theory is wrong, and that there must be an alternative theory, which Holmes will eventually reveal to us. In the same way (without implying that Crews is as dim as the police in the Sherlock Holmes stories), Crews's critics suggest that there is evidence that would have to be there to support his case, but is not.[15]

The most powerful piece of this negative evidence is simply that no one, including Crews and Fitzgerald, has seen this behavior in the field; Cole and Townsend mention that the most thorough study of their behavior in the wild does not include it. The response by Crews's group is not an offer of evidence of copulation in the field, but an ironic

15. All the studies of controversies in note 1 to this chapter refer to the importance (or disregard) of negative evidence, so the focus on negative evidence here would seem to be a general feature of scientific debate, and not, as it might seem, a sign of the trivialization of debate.

reversal questioning the appropriateness of the evidence called for. They question the critics' contrast of the field (as the place of pure behavior) versus the laboratory (as a place of confined and abnormal behavior). CGT argue that the difficulty of observing *Cnemidophorus* invalidates the negative evidence about such matings in the field.

> That male-like sexual behavior has not been observed in unisexual *Cnemidophorus* lizards in nature does not mean that it does not occur. Anyone who has worked with cnemidophorine lizards in the field knows how difficult they are to observe. *Cnemidophorus uniparens* are extremely active foragers and spend much of their time above ground in thick mesquite and creosote bushes. They are wary of humans and, if approached too closely, will retreat quickly into extensive burrow systems. Furthermore, the literature indicates that matings even in sexual *Cnemidophorus* are observed in nature only infrequently.

The ironic turn of CGT is to move from the question of the observation of this behavior in parthenogenetic species to the problems of observation itself, as shown by the difficulty of seeing the sexual species engaged in mating behavior. If normal mating behavior is *not* observed in sexual species in the field, then the fact that it is *not* observed in parthenogenetic species is insignificant.[16] Instead of responding to negative evidence with positive, they respond with other negative evidence that works against the first negative evidence. The argument is like that which Darwin makes to explain the lack of continuous fossil record of gradual evolution, when he describes the record as a mutilated book (*The Origin of Species* p. 316). But to make this argument from what they *don't* see they must present a case for themselves as field observers, and they do this with the same sort of detail Cuellar and Cole and Townsend give in their defenses of their observations. The descriptions of the bushes in which the lizards are found add authenticity. They use vague adverbs that are rare in this kind of article (*extremely* active, *very* wary, retreat *quickly*). The effect is to create a visual image of the real desert where the lizards are found, and to suggest that the authors are old hands when it comes to field experience.

16. In reviews of the later paper by Crews and Moore, several *Cnemidophorus* workers insisted that it was possible to observe sexual *Cnemidophorus* lizards mating, even though no such observations had been published (of course, such observations would hardly be surprising enough to merit publication). But none of these reviewers uses this denial of the negative evidence as a basis for rejection of Crews and Moore's major claim.

Another piece of negative evidence concerns the stage of the behavior in which the mounting lizard grasps the mounted lizard in its jaws. Cuellar argues the lizards should show bite marks if they have been mounted.

> I have observed such behavior in C. *uniparens* and other species for 15 years, but only sporadically. Moreover, only in rare instances have we observed copulation bites among nearly 2000 individuals collected in the wild. The abnormal constraints imposed by captivity result in a variety of bizarre behaviors of which female-female matings is but one.

The argument here is that the mounted lizard should have a bite mark after copulation, that there are few bite marks in the field, so they could not have been mounted in the field, so the behavior must occur only in captivity.

CGT respond to Cuellar's negative evidence, not by providing evidence of bite marks on parthenogenetic lizards, but by ironically reversing the argument, and pointing out that similar negative evidence would apply to sexual species. This does not prove that sexual species don't mate, but suggests that the sign is not a natural inscription of mating.

> He [Cuellar] provides no information about the frequency of such marks in sexual versus unisexual *Cnemidophorus*. Examination of 1,000 female adult *C. tigris*, a sexual species, collected during the breeding season and deposited in the Museum of Vertebrate Zoology, University of California, Berkeley, revealed that only 3 percent had marks on the back and side; further, the same frequency of males (N=1,100) possessed such marks (Crews, unpublished data). On the basis of our behavioral observations of both sexual and unisexual cnemidophorine lizards, we would suggest that these marks reflect interspecific aggression, predation attempts, or accidents.

CGT have been pushed into a rather strange piece of counting by Cuellar's criticisms. They seem to recognize that part of the persuasiveness of Cuellar's point is simply the large numbers he can muster. If they did not need to make this argument, it is hard to see why they would look at more than a thousand sexual lizards as part of a study of a unisexual species. The response to Cuellar's reinterpretation of their animal narrative is another parallel of sexual and parthenogenetic lizard behaviors. Just as Cole and Townsend had reinterpreted

Crews by granting the behavior, but relating it to aggression in laboratory conditions, CGT find the marks that Cuellar observes, but relate them to the rough life in the field. CGT present this ironic turn, not as a reinterpretation, but as a request for further information ("He provides no information . . . ").

The statistics presented in tables in Cole and Townsend's article present another sort of negative evidence, apparently denying a reproductive role for the behavior. Cole and Townsend conclude, "Our observations suggest there is no correlation between the reproductive states of the mounting lizard and the mountee (Table 1)." This seems a clear refutation, but the categories presented in their table and in the table in Crews and Fitzgerald's article are not quite comparable. Crews and Fitzgerald used the terms previtellogenic, preovulatory, and postovulatory, whereas Cole and Townsend categorize their lizards as gravid and nongravid; the difference, as we have seen, becomes a matter of controversy in the methods sections. Cole and Townsend demonstrate that the behavior "is of no obvious benefit to their reproduction" by counting the eggs laid in each clutch. A reader in Crews's lab notes in the margin at this point, "interval between clutches" suggesting a possible criticism of the use of the number of eggs as a measure of reproduction.[17] As in the gravity waves case that Collins studied (*Changing Order*), when the phenomenon is in question, there is disagreement over what counts as a competent experiment. The controversy moves from the phenomenon, to ways of observing the phenomenon, to checks on ways of observing the phenomenon in what Collins calls "the experimenter's regress."

Cole and Townsend provide another kind of negative evidence with observations of the behavior occurring in contexts in which it could have no reproductive function. The following passage, for instance, is a reinterpretation in which the behavior is observed but is rendered meaningless because it does not correlate with reproductive state as Crews and Fitzgerald said it would.

> In this regard, the two following sets of observations are interesting because they are the occasions on which a female was observed to mount all other inhabitants of her cage in one day. One of these

17. This sentence in an earlier draft read, "A reader in Crews' lab notes in the margin at this point, 'interval between clutches,' suggesting the possibility that these lizards might lay eggs more frequently, if not in larger numbers.' " Crews's comment in the margin of my paper here emphasizes the importance of the methodological differences between his group and Cole's. "The laying of eggs has nothing to do with it," he says, "it is the size of the ovary that is all important."

> mounters was a non-gravid *C. neomexicanus*, which mounted three *C. uniparens* (two gravid, one non-gravid). The other mounter was a gravid *C. uniparens*, which mounted two *C. exsanguis* (both non-gravid) and one *C. uniparens* (gravid). No correlations are indicated.

As the last sentence suggests, the intended significance of these accounts is to show that the behavior has nothing to do with the laying of eggs—it could not be related, if both gravid and nongravid lizards mount, and if the mounted lizards are both gravid and nongravid. Instead, it supports the counter-argument that the behavior is an artifact of captivity. It is significant that they use a narrative form of evidence, instead of sticking to a quantitative argument; both Cole's group and Crews's group seem to realize that cases can be more persuasive in some contexts than large numbers. MWBC examine in detail the alternations of role by just three lizards, and Crews and Moore have made an attractive figure illustrating this case for use in other articles.

The use of negative evidence can be complex, for it requires the reader to imagine a complex series of causes and effects, or rather of noncauses and noneffects. This complexity is apparent in Cole and Townsend's argument that the behavior is a form of territoriality induced by captivity.

> It may be significant in this respect that in 60% of our observations of females mounting females, the partners were not conspecific (Table I), though in most cases a conspecific female also was present in the cage. In addition, in 50% of our observations on captives (Table I), the mounter was *C. uniparens*, although Hulse (1981) reported no such behaviour in a field study of this species, which included observations through two summers (7 months). Hulse (1981) also stated: "*Cnemidophorus uniparens* exhibited no signs of territoriality". We suspect that confinement in captivity enhances this activity. In this regard even Werner's (1980) observations on free-living geckoes are pertinent, as the animals were in a dense population in an artificial environment (human habitations).

The evidence that the lizards often mounted members of other species confirms that they do mount in this way, but suggests that it has nothing to do with reproduction. The argument Cole and Townsend then make by focusing on *C. uniparens* is rather complicated. One would think that the evidence that *C. uniparens* are the most frequent mounters, and that they are not observed to be particularly territorial, would work against the claim that the behavior was territorial and for Crews and Fitzgerald's claim that it was related to reproduction. But Cole and Townsend go on to say that the species might then become territorial in captivity, suggesting again that the behavior is an artifact. It is, in their view, unrelated to the lizards' natural territorial behavior as it is unrelated to their natural reproductive cycles. The one piece of evidence for such mounting in the wild, in another genus, is turned so that it is evidence for such behavior being the result of captivity. The geckos live in people's houses; this characteristic can be reinterpreted so that what Crews calls "wild" (implying naturalness), Cole and Townsend call "human habitations" (implying artificiality).[18]

Closure

The analyst with only these texts for evidence would think that the controversy was always just about to end, for each article ends with a reassuring note of closure, setting out some firm grounds to justify further work or deny any need for it. There is a rhetorical difficulty in such closure, for scientists are suppose to seem open to further questioning, especially at the end of the article, conventionally the place for references to further work. The Crews and Fitzgerald article, for instance, ends with a whole paragraph of questions. Cuellar's ending, if read literally, says that he is open to further findings:

> The abnormal constraints imposed by captivity result in a variety of bizarre behaviors of which female-female matings is but one. A far more common one is pseudocopulations between males of bisexual species, such as *Cnemidophorus tigris*. In my laboratory, the larger or healthier males 'rape' subordinates at will, albeit unsuccessfully, as insertion of the hemipenis requires 'willingness' on the part of the mate, even in male to female 'rapes.' This behavior is so common that the subordinates become emaciated and would die from perpetual harassment, if the 'sexual offender' were not isolated. The implications are similar to those proposed by Fitzgerald and Crews, but it would be premature at best to propose that this abnormal courtship behavior is essential for successful reproduction in *C. tigris*.

The parallel suggested for Crews and Fitzgerald's observation, read literally, seems to add to their article. But of course the abnormal-

18. Crews comments in the margin of my paper, where I quote Cole's comment on the geckos living in houses, that this "is where *geckos* live naturally," The comment supports my point about the rhetorical importance of the natural/artificial distinction.

ity of this behavior implies that the mounting behavior Crews and Fitzgerald have seen is also abnormal, and that they should learn, as competent keepers of these lizards, to guard against it. The understatement of the last sentence ("it would be premature at best to propose" that male–male matings could be essential to reproduction) is part of its heavy irony. Cuellar clearly expects that this dismissal will close the issue.

Similarly, Cole and Townsend present themselves in their conclusion as cautiously undecided, lacking evidence, and open to new ideas.[19]

> It would be interesting to understand the cause(s) and function(s) of malelike copulatory behaviour among female lizards, but few data specifically and positively pertain to these points.

They leave the suggestion of alternative interpretations to references to other *Cnemidophorus* workers, reinforcing the sense that Crews and Fitzgerald are isolated in the research community. They do not definitely claim that the behavior is the result of captivity.

> Regardless of the interesting ramifications of this behaviour, there is no evidence that homosexual activities normally are involved in the reproduction of unisexual species of lizards.

Part of the rhetoric of Cole and Townsend's article is in its not making a counterclaim; by avoiding such involvement they further suggest that Crews and Fitzgerald have made their claim prematurely. Cole and Townsend do not say that they are reinterpreting the observations, but deny that the observations have been established as meaningful. The conclusion strikes a note of openness and caution but actually moves toward closure on the debate, implying that there is no basis for a controversy.

But the Cole and Townsend article ends with a postscript that makes an explicit attack on the competence of Crews's group as observers of behavior.

19. Crews comments "I don't read it this way." But this is because he sees the statement with a detailed knowledge of the context of the controversy, in which the statements that there are "few data" and "no evidence" imply dismissals of his group's work, not openness to further research. A reader coming to this article out of the controversial context might interpret the authors' stance as one of cautious indecision.

> A new paper (Gustafson and Crews 1981) that we received after completing this manuscript further confuses knowledge of the sociobiology of unisexual lizards because it erroneously relies on lifting of the hands during basking as an indication of submissiveness in *C. uniparens*. Such basking behavior in *C. uniparens* should not be confused with the arm or hand waving that appears to be a signal in other species, as in *Cnemidophorus lemniscatus*.

Again, as with the quotations of claims and the unusually detailed description of methods, such a direct and personal criticism would seem very odd except in the context of a controversy. The language of a postscript can apparently be more explicitly critical than that of the main body of the article, where Cole and Townsend only say that their results contradict those of Crews and Fitzgerald. As the personal notes in the methods section are unusual because they focus attention on the authors' competence, the personal note in this postscript is unusual because it focuses attention on the incompetence of the researchers they are criticizing.

As in the main controversy over the meaning of the mounting behavior, the reinterpretation depends on whether the behavior observed—the lifting of a front leg—is parallel to basking in the same species (in which case it has no meaning for reproductive behavior) or whether it is parallel to the narrative of submissiveness in another species (in which case it can be used as a signal of courtship). Behavioral terms similar to those in dispute here also figured in the construction of the behavior and in the CGT and MWBC articles. Cole and Townsend, instead of showing that what they consider to be a misinterpretation would bias the results of Gustafson and Crews, need only point out the apparent error to taint the whole research project. Crews's project is placed in the narrative of the whole field, as an obstruction that "further confuses knowledge of the sociobiology of unisexual lizards." The sweeping nature of this criticism suggests that narratives are arranged in a hierarchy of inductive argument, so that if a researcher can be shown to be wrong in an interpretation of an animal's action, then the study and the project of which the observation is a part both crumble. A researcher who cannot tell basking from handwaving has not just made a mistake, he or she is incompetent and misleading.

CGT end their defense with a move toward closure much like that at the end of the Cole and Townsend article. They refer to a narrative of the whole ongoing project, summarizing current knowledge such that their position represents fact and the other position represents hypothesis.

> We cannot yet say whether the *presence* or *absence* of male-like sexual behavior in captive populations of unisexual lizards is abnormal. However, the following observations are not subject to controversy and still require explanations: . . .

Both CGT and Cole and Townsend seek to remove certain issues from controversy to bring the issue, or at least part of it, to closure. Both strike the cautious note of saying that not enough is known, but Cole and Townsend take this lack of knowledge as a disproof, while CGT take it as a call for further research.

The MWBC article prepares for closure by turning the tables, asserting that the authors have put forth the experimental evidence and that they are involved in testing hypotheses, so that it is their behavior, rather than that of their critics, that is properly cautious:

> Gustafson and Crews (1981) have demonstrated experimentally that the presence and behaviour of cage-mates causes captive *C. uniparens* to produce more clutches of eggs. An understanding of the obviously complex social biology of unisexual *Cnemidophorus* will be advanced only by rigorous testing of hypotheses.

Only if one has the project of Crews's group in mind do these sentences follow one another. MWBC assert, after all the reasons for disregarding other research as flawed, one assertion that is supported *experimentally.* Of course no researcher would argue with the need for testing of hypotheses in general. But researchers might argue with the assertion here that the Gustafson and Crews article supports such a hypothesis in need of further rigorous testing.

The MWBC discussion ends with a closure move very similar to that of the earlier rebuttal, with the authors' position defined as the cautious one supported by data, and supportive of further research.

> Until these data [on pseudocopulation in wild populations] are collected, the only hypothesis that is supported by experimental tests with captive individuals (Gustafson and Crews 1981; Crews 1982) is that pseudocopulatory behavior is adaptive because it enhances reproductive potential.

This closure is an attempt to reinterpret the narrative of the whole field, to present their own project as a starting point rather than as a digression. The key citations for the project are now somewhat later articles than that of Crews and Fitzgerald; they focus attention on articles that have a more complex experimental design, shaped in the course of the controversy. The language of hypothesis and experiment is claimed only for this project; it is the others who are indulging in speculation.[20]

An End to Polemics?

Textually, this persistence of two inconsistent accounts could go on for ever; either side can put the narratives of the other in new contexts for ironic reinterpretation. Practically, in terms of funding and publication, either Crews continues his research, or he doesn't. The issue will be settled, not by something the lizards do, but by the dynamics of one part of the scientific community. That this particular controversy is not closed can be seen in the papers from a symposium on the *Cnemidophorus* held by the American Society of Ichthyologists and Herpetologists in 1984, which Crews said "promised to be a modern version of the shoot-out at the OK corral." This seems not to have been the dramatic occasion that the tone of the articles I have presented so far might suggest. But we can see signs of how other researchers were responding to the continuing work of Crews's lab in both Cuellar's paper and in the comments of referees who reviewed Crews and Moore's paper before its publication.

Cuellar's response seems to be to continue on his own line of research, continuing his doubts about the relation of the observed behavior to reproduction, but answering Crews's publications only where they directly criticize his methodology. Cuellar was not able to attend the symposium, but submitted a paper for the conference proceedings, "Further Aspects of Competition and Some Life History Traits of Coexisting Parthenogenetic and Bisexual Whiptail Lizards," As the title suggests, most of the article is devoted to issues unrelated to Crews's claim, and he responds to Crews, again, only in a postscript. Cuellar quotes the passages criticizing his observations that I have quoted, starting with the line, "Preconceptions, however, guide perception, and one does not very often see what one is [not] looking for." His tone in response to the response to his criticism involving bite marks is apparently mild.

20. Again there are parallels between these closings and those in the "Technical Comments" on Cuellar's 1977 *Science* article. Each text there acknowledges the existence of a controversy, and then concludes with a suggestion for closing it, and each of these suggestions identifies the author's own positions with facts, objectivity, openness, or usefulness for further research.

> These are legitimage points raised by Crews et al., for indeed I had not previously documented the frequency of these bite marks, and had assumed they would stand out conspicuously during routine examination of freshly-collected live animals.

But there are two criticisms of Crews' response implied here: it is suggested that these marks should be noticed as a matter of competent collecting, and also that Crews's study—looking for the marks on *preserved,* rather than freshly collected specimens—would not provide relevant evidence. Cuellar then goes on to present extensive data about bite marks in the field, data Crews's group had said were lacking. As he himself points out, such data would not have been tallied, much less published, were it not for controversy that made them suddenly become relevant:

> Prompted by the report of pseudocopulations in the laboratory by Crews et al. (1981), and by the challenge by Crews et al. (1983) to document my field observations, I have since recorded the location and extent of the marks and correlated their occurrence with reproductive condition in samples captured and released during three years from 1982 to 1984 (Table 5).

In interpreting these data, Cuellar makes a point of his giving every possible benefit of the doubt to Crews's case. His conclusion is that while the marks occur, they do not occur in such a pattern as to support the claim that pseudocopulation facilitiates reproduction. He continues to quote publications from Crews's group extensively and always ironically. For instance, he quotes the comment in MWBC about the difficulty of observing these shy lizards, and then quotes his own work describing *C. uniparens* as relatively easy to observe. He ends by turning their criticism of him back on them:

> In fact, the extensive laboratory documentation of such behavior strongly suggests it is common in the field, but as Crews et al (1983) appropriately note "perceptions being subjective are not readily changed by argument and riposte."

This may seem to grant Crews and Fitzgerald's original claim, but on what is now the key issue, the relevance of this behavior to reproduction, he remains unconvinced. There is a kind of closure here: he has decided, as has Crews's group, that there is no chance of persuading the other side, that there is a limit to the amount of back and forth criticism that is worthwhile, and that resolution will come only by persuading those *Cnemidophorus* workers who haven't been involved in the controversy.

Crews and Moore's paper for the same ASIH conference, "Reproductive Psychobiology of Parthenogenetic Whiptail Lizards," opens with a reference to Crews and Fitzgerald (1980) and then reviews the work on pseudocopulation up to 1985. The reviewers of this paper, who are *Cnemidophorus* workers uninvolved in the controversy, are generally enthusiastic about Crews and Moore's work, but less enthusiastic about their rhetoric. One begins: "Overall, this is a well done and important paper presenting additional information on an inherently interesting topic. As presented, however, it will continue to foster controversy and rabid-dog type criticism. Some of the reasons for criticism are valid." This reviewer thinks that the whole issue of whether pseudocopulation occurs in the field—the issue that led to the back and forth exchanges on bite marks and on the shyness of the lizards—leads researchers away from more important issues: "The important point is that whether or not the unisexual species do it in the field, they certainly do it in the lab and this offers a unique opportunity to observe a 'male' behavior in the absence of the heretofore assumed payoff, insemination." This reviewer would resolve the controversy by redefining the context. It is an important step toward a resolution that would acknowledge Crews's interpretation while leaving open its relevance to behavior in the field (this is analogous to the kind of resolution Martin Rudwick sees in *The Great Devonian Controversy*). But for Crews's evolutionary argument, it would seem that the occurrence of the behavior in the field does matter, and he is not ready to abandon that part of his argument so easily.

All the reviewers question the elaborate negative evidence for the failure to see the behavior in the wild, but they do not go on to question the "naturalness" of the behavior itself. One of the reviewers is especially dubious about the sort of redefinition of the claim of Crews and Fitzgerald that I have discussed:

> The authors request here an end to polemics (good idea!), but then appear silly in defending a poorly stated conclusion in the 1980 paper. To most readers, "successful reproduction" means "producing any offspring." The authors claim they meant to say that degree of reproductive success (i.e. number of offspring) is related to

> pseudomale behavior. It seems time to bury the hatchet . . . who's going to go first?

The reviewers' comments suggest that a controversy may sometimes be resolved, not when new evidence comes in to settle it, but when everyone else gets tired of it, and finds ways of either using or getting around the new claims. At this stage, as one reviewer points out, it is still necessary to follow the story of the controversy from the beginning. This reviewer suggests that the volume include an essay by "one of the critics." The general reader "might be a bit confused without such a chapter." (The reviewer even suggests that the critics and Crews and Moore collaborate on a review, though this seems not to have happened.) But the controversy is approaching the point when these arguments will no longer matter. When a controversy is closed, as Collins has pointed out in his study of research on gravity waves in *Changing Order*, all the social processes will be forgotten. The construction of narratives and their ironic reinterpretation will no longer be an issue. There will be only the story of the lizards—not necessarily the same as the first story presented by either side. And the stories of the studies, projects, and the field will be subsumed into the one exemplary story of the progress of science.

One place one might expect to find this exemplary story would be in popularizations. As it happens, both Cole and Crews have written *Scientific American* articles on *Cnemidophorus*. If these articles are any indication, research continues without any generally accepted view of *Cnemidophorus* behavior because the various groups are able to pursue two completely separate lines of research. Cole does not mention Crews at all in his *Scientific American* article "Unisexual Lizards" (nor does he mention Cuellar). At the end of his article, he presents a story about the study and significance of the physiology of parthenogenetic lizards:

> Today interested workers find themselves in a position not only to ask new questions and design new experiments but to utilize these specialized organisms in ways that would not have been imagined a few years ago. Among the possibilities that come to mind are gaining a better understanding of the role of sperm in fertilization, clarifying how it is that some animals are quite successful with multiple copies of genes whereas others are not, studying switching mechanisms in embryonic development, producing cloned animals of known genetic composition for biological experimentation, and even inducing cloning in normally bisexual species to increase pro-

> ductivity in animal husbandry. If unisexual reptiles contribute to progress in any or all of these areas, it will have begun with a few startling observations concerning a form of wildlife that practically no one considered significant.

Crews, in his own *Scientific American* article, "Courtship in Unisexual Lizards: A Model for Brain Evolution," cites Cole's article in his paragraph on the various species of the genus and their chromosome makeup. But he makes a more important reference in his introduction to a much older *Scientific American* article by his mentor Daniel Lehrman. Crews's story, as summarized in his introduction, is really about circuits in the brain controlling behavior; the lizards are interesting as a natural experiment showing how these controls work.

> The brain, which controls mating behavior in males and females, not only has adapted to a new set of stimuli in this species but has also mediated a switch to females of behavioral patterns that are normally associated with males. This reinforces the observation that the brain is equipped with neural circuits for both male and female behavioral repertoires, regardless of biological sex. By investigating the manner in which that has come about, using unisexual lizards as my model, I have gained insight into the ability of the brain to adjust to changing conditions during the course of evolution.

These underlying differences in the narrative of the discipline into which they insert their studies make for quite different approaches throughout the articles. For instance, they both discuss the descent of the unisexual species from hybrids of other species, but they cite different evidence and different researchers on different species. Cole cites traditional descriptive work done in the 1960s by two important *Cnemidophorus* workers:

> Lowe and Wright found that in such pertinent attributes as color, color pattern, scale shape, chromosomes and preferred habitats the character of *C. neomexicanus* appears to be that of a first-generation hybrid produced by the mating of *C. inornatus* and *C. tigris*.

Crews cites more recent work that is far from the traditional methods of natural history:

> By comparing the DNA sequences of various whiptail lizards, Llewellyn D. Densmore III, Craig. C. Mortiz and Wesley M. Brown of

> the University of Michigan were able to determine that the maternal ancester of *C. uniparens* is the bisexual species *C. inornatus*.

There is no conflict between these two findings, but each author uses an approach to the determination of descent that is most appropriate to the methods of the rest of the article.

Since the *Scientific American* editors published both accounts, they must consider both Cole's line of research and Crews's to be of general interest to the public, and to be generally accepted by the *Cnemidophorus* community. There is no confrontation between them, and indeed, neither article mentions any controversy. I am not suggesting that either Cole or Crews is hiding something in these articles, just because they don't dwell on the controversy that interests me. They are following a convention of popularizations, by which writers usually present the current consensus on any topic, not still-controversial views by individual researchers. What is it about popularizations that eliminates just those features that tend to show the social construction of science? In the next chapter I shall look more closely at the construction of popularizations.

Whenever I have presented papers on this controversy, it has struck audiences as funny that scientists would argue with such heat about whether a lizard's lifting of a leg is hand-waving or basking, or about whether there are bite marks on a lot of preserved specimens of dead lizards. If it were only the technical details were at stake, the argument would indeed be trivial, and probably wouldn't even interest other *Cnemidophorus* workers. But such technical details have their place within narrative of studies, that themselves have their place within a larger narratives of the field; Crews, for instance, sees implications for the important question of the hormonal control of human sexuality. In fact, there are few controversies in biology that do not have broader implications. The implications are brought out most clearly, not in controversies in the core set, or in typical popularizations like these, but in a few persistent controversies that take place in the public forum. These public controversies require writers to address a different audience and use different techniques, but one still sees the basic strategies of construction and ironic reinterpretation of narratives. In chapter 6 I shall examine the strategies in one such public controversy, in which participants display the larger implications of technical details, in a study of response to E. O. Wilsons's *Sociobiology*.