Chapter Five

The Social Construction of Popular Science: The Narrative of Science and the Narrative of Nature

My stance in the studies so far is to assume that many readers will be surprised by the view that science is constructed in social processes of claims and negotiations, carried out in revisions of articles and proposals and in ironic reinterpretations in controversies. This stance assumes that many readers, especially nonscientists, will start with a different view of the work of science from that which I am proposing, a view that sees the main work of science as passively observing naturally occurring facts. But if people do hold this view of science, where would it come from? And why would anyone come to think of scientific texts as just conveying information? I shall argue that even very sophisticated popularizations tend to promote a view of science that focuses on the objects of study rather than on the disciplinary procedures by which they are studied.

Those who have studied popularizations have generally agreed that articles for the general public and articles for scientific specialists are strikingly different, but there is tendency to take either articles for popularizations or specialist articles as primary and dismiss the other form as a distortion. Either the popular article is seen as watering down the difficult truths of the professional version, giving the false impression of easy comprehension, or the professional version is seen as complicating the simple truths of the popular version unnecessarily, using jargon and technical details to exclude untrained readers. These two accounts are evident, for instance, in the responses to a striking experiment conducted in 1971 by F. J. Ingelfinger, the editor of the *New England Journal of Medicine*. Exasperated with immunology articles so difficult that only other immunological researchers could read them, Ingelfinger had one rewritten by Barbara Culliton, a journalist on the staff of *Science*, and published both versions. Culliton kept all the information in the original, but reorganized the article, rewrote the sentences, and included some definitions of terms in appositives, so that any practicing physician could read it. The editor received a number of letters saying he had proved that even difficult topics could be made accessible to a wider audience with some attention to organization and clear writing. But he also received some letters from immunologists saying that they found the revised version harder to read; it was as if the housekeeper had come in and nothing was where they were used to finding it. Both groups of letter writers thought that the experiment showed that there was a right way and a wrong way of writing immunology; they disagreed only about which was which.

I shall use the approach to narrative introduced in chapter 4 to argue that popularizations and scientific articles present two views of what a scientist does, two views that are incompatible but that both play a part in creating the cultural authority of science. I shall look at the ways the narratives are constructed in articles in some articles on evolutionary biology in *Science* and *Evolution* and in articles by the same authors on the same topics for more popular journals, *Scientific American* and *New Scientist*. Textual differences in narrative structure, in syntax, and in vocabulary can help define two contrasting views of science. The professional articles create what I call a *narrative of science;* they follow the argument of the scientist, arrange time into a parallel series of simultaneous events all supporting their claim, and emphasize in their syntax and vocabulary the conceptual structure of the discipline. The popularizing articles, on the other hand, present a sequential *narrative of nature* in which the plant or animal, not the scientific activity, is the subject, the narrative is chronological, and the syntax and vocabulary emphasize the externality of nature to scientific practices.[1]

1. The two categories I use may be compared with those in several recent sociological studies of scientific discourse. In *Opening Pandora's Box,* Nigel Gilbert and Michael Mulkay distinguish between two ways scientists account for their work. In the empiricist repertoire of formal scientific discourse, actions are explained in terms of purely scientific factors, whereas in the contingent repertoire, excluded from formal scientific discourse, actions are explained by other social and personal factors (p. 40). But in these terms, both the narratives I describe use the empiricist repertoire. Both the scientist and the public have an interest in treating the facts of science as something apart from contingent processes. The narrative of science accomplishes this separation by certifying the acceptability of the methods and concepts used; the narrative of nature accomplishes this separation by focusing on the object studied and excluding the conditions of study.

I define these two narratives through two kinds of comparisions. First I illustrate the kinds of textual features relevant to these two narratives by comparing published professional and popular articles by the same authors on the same research, looking especially at differences in titles, abstracts, introductions, organization, and illustrations, features that guide the reader in constructing a narrative. Then I compare the manuscripts of the popular science articles by scientists to the published versions as edited and extensively rewritten by the editors of popular journals. In this comparison I go from large-scale changes to small: from changes in the overall organization of the text to changes in the syntax of sentences and of individual words. The negotiations between authors, who try to write a narrative of science, and editors, who want more of a narrative of nature, are where these two views of science meet. Finally I shall briefly compare popularizations of these authors' studies in several different publications for several different audiences.

The differences between these discourses have implications for the study of the public understanding of science. Many studies of popularization treat science as information that is merely communicated to nonscientists in more or less efficient language. As in the previous chapters, my approach is based on the assumption that science is embodied in language, so the translation of one form of words into another changes the meaning in some way. Even when two articles seem to be about the same research, it may turn out that one is about garter snakes and the other about isolation of a pheromone. One consequence of this assumption is that we should not expect the writers or readers of either narrative to enter easily into the other.[2] If

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The distinction between the narrative of science and the narrative of nature also parallels the levels of "externality" analyzed by Steve Woolgar ("Discovery"), and in a different way, Trevor Pinch ("Towards an Analysis of Scientific Observation"); these scales are discussed in chapter 3. Pinch shows that scientific texts are chararacterized by a tendency to claim the greatest externality possible. I shall argue that popular texts, on the other hand, assume the externality of all scientific findings, and omit whenever possible any suggestion of scientific artifice.

Still another distinction that could be usefully compared to mine is Michael Lynch's analysis of "talk about science" and "talking science" in *Art and Artifact in Laboratory Science*.

2. This comment does not mean to imply that there are not many biologists publishing important specialized research who also publish popular science articles. Such scientists, I would argue, can handle both the narrative of science and the narrative of nature; they do not necessarily make the two narratives indistinguishable. For a lively presentation of the changes necessary in popularization, by one of the masters of the genre, see J. B. S. Haldane, "How to Write a Popular Scientific Article," now collected

words embody science, then both sides, the professionals and the public, have a stake in the form of words they use. As we saw in the introduction, many studies have shown that the narrative of science is part of what maintains the scientist's sense of objectivity and cumulative progress and the definition of the discipline. The popularizers have a stake in popular language, in the narrative of nature, as well. Scientists interested in the public understanding of science should consider, not just attitudes toward science or how much scientific knowledge the public has, but how the public interprets scientific activity.

The Narrative of Nature and the Narrative of Science

I shall base my descriptions of the narrative of nature on two articles in *Scientific American* and one in *New Scientist*. The publication process for these journals is different from that of the professional journals with their competitive peer review; both journals commission articles by authors recommended to them and edit the articles to suit their audiences. *Scientific American* is an American monthly with a general audience; many of its readers have some scientific or technical training. It publishes rather long articles (authors are told to keep them to about 4,000 words), all of them by research scientists. *New Scientist*, a British weekly, has shorter articles (2,000–2,500 words) and a broader readership that includes many secondary school students. Gail Vines, one of the editors (in a letter pointing out that scientists' articles sometimes need to be edited to make them readable) notes that this readership is not exactly the general public:

> Our market research . . . tells us that half our readers have at least one A level in science. People working in science say they read it to

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in *On Being the Right Size* (Oxford: Oxford University Press, 1985). Other well-known examples of scientist-popularizers include Julian Huxley, Peter Medawar, James Watson, Francis Crick, and Stephen Jay Gould; I would also think of Mark Ridley, Richard Dawkins, John Maynard Smith, and E. O. Wilson, in the areas at which I have been looking.

Robin Dunbar points out that the heavy editing I describe is not necessarily the rule; a recent article on his area of research was accepted with only the deletion of one sentence by *New Scientist*. But Dunbar has written often for *New Scientist*, and may be said to have internalized their style. The article focuses on the animals in the way I say is typical of the narrative of nature, but it contains much more about scientific debates than any of the articles studied in this paper.

> keep up with other fields. The maxim in the office is that the physicist should be able to understand the biology (and vice versa).

Many of its feature articles, like the one I shall study, are by researchers themselves, but other material is reported by staff journalists and freelance science writers. Both journals have extensive illustrations, usually photographs in *New Scientist* and elaborate and lovely paintings and color charts in *Scientific American*.

All three of the authors I am studying in this chapter had recently published a number of articles reporting their research in professional journals. I have chosen for comparison articles in *Science* and *Evolution*. *Science* is one of the two weekly general science journals that provide biologists with a prestigious outlet for rapid publication (see chapter 3). *Evolution* is one of the core journals of evolutionary biology; that is, it is a journal read regularly by nearly all specialists in a broad field.

Why do scientists write for the popular journals, when all the professional rewards are for articles in professional journals? Not for the money; the fee is small, considering the disproportionate amount of time such articles take to write (though one researcher I interviewed paraphrased Samuel Johnson's comment that no man but a blockhead ever wrote, except for money). They don't get rewarded with citations either; these journals are not usually places for first reports or findings, and they do not allow for extensive review or theoretical development. But there is clearly prestige within the research community attached to being asked to speak for one's field, and there is the chance to address a broad audience that includes many researchers and administrators in related fields who would not ordinarily read one's work in specialist journals. One of the authors I am studying tells his coauthor in a letter, "Remember that this article is as much an advertisement as it is informative." The writing of such advertisements is in many ways similar to the writing of the introductions to grant proposals; in both cases the researcher must put his or her work in its larger disciplinary context. But in popularizations there is a convention of presenting the representative nature of one's own work (and thus the author's appropriateness as spokesperson for the field), rather than stressing its uniqueness (and thus the author's worthiness for funding, in competition with others in the field). Although such articles may not directly advance the career of the individual writer, they are essential to the survival of the discipline, dependent as it is on public support for research. A 1985 report by the Royal Society on *The Public Understanding of Science* says, in bold type, "Our most direct and urgent message is for the scientists—learn to communicate with the public, be willing to do so, indeed consider it your duty to do so"(p. 24).

Why do readers read these journals? Advertisements for both journals suggest that readers are interested in technological developments, in scientific controversies, in the newness of ideas, in ideas with immediate practical implications. The journals offer, not only entertainment, but access to a kind of power. The articles make no attempt to draw the reader in, as scientific features in general-interest magazines and newspapers must do. A *Scientific American* article definitely takes as given the reader's curiousity about the topic, whether it is the sexual behavior of lizards or the opperation of zippers. *New Scientist* tries harder to be catchy, but still assumes a reader interested in the subject matter and not just in its current news value. None of the three articles I have studied tries to attract readers' attention by linking the topic to some popular debate or public interest. This is odd, because all of them can be presented in such a controversial context: one writes on ecology, another on the roles of hormones and of environment in controlling sexual behavior, and the third on sociobiology.[3]

I present three researchers, rather than just one, because, as the brief descriptions suggest, their research methods, and problems of popularization, vary in many ways. One effect of popularization is that they come out sounding rather the same: they are all presented as direct observers of nature in the natural history tradition. But in each case, they could also be seen as biological thinkers participating in debates over biological concepts and addressing various discipline-specific problems.

Lawrence Gilbert works on the problem of coevolution: how the evolutionary changes of two species in the same enviroment relate to each other. In 1981 he and Kathy Williams, then an undergraduate student working with him, published in *Science* an article reporting their studies on how the passion vine mimics butterfly eggs on its leaves and prevents the butterflies from laying real eggs on it. He was then asked to write an article on this topic for *Scientific American*. As

3. In Geoffrey Parker's article, "Sex Around the Cow-pats," the editor cut out the concluding comments on applications of sociobiological findings on sexual selection to man. Perhaps the section was cut, not because it is sensational or controversial or unrelated to the main point of the manuscript, but because the parallel of man with nature here makes sense only if one sees them both as following the same model of evolution. To see this one would have to focus on the concepts of the article rather than on the animals themselves.

we shall see, part of the popular fascination of his work is that it takes place in an exotic setting and deals with butterflies, which are beautiful, delicate, and perhaps more appealing to nonbiologist than, say, garter snakes or dung flies.

David Crews's work is also of interest to a broad range of readers, but is also, I think, open to misinterpretation by nonbiologists who see him as just a voyeur watching the sex lives of lizards and snakes. As we have seen in previous chapters, he studies the evolution of the systems that control reproduction, and of sexual behavior. He works through observation of the animals' behavior, examination of organs, and analyses of the substances in the animals' blood, and through comparisons of his findings to the findings of other researchers working with other species. But what comes across most strongly to a popular audience is simply the strangeness of the mating process in these species—not the parthenogenetic lizards we saw in the last chapter, but some garter snakes that live in the Arctic. He asked *Scientific American* to allow him to have as coauthor William Garstka, then a graduate student at Harvard, now an Assistant Professor at the University of Alabama. Garstka was first author of a *Science* article that provides the basis of much, but not all, of this popular article. Crews had written a previous article for *Scientific American* (and has written another one since; see chapter 4) so he was familiar with their expectations and editing techniques, and he warned Garstka in a letter, "I fully expect some changes, perhaps extensive."

Geoff Parker's work can also be seen in two quite different perspectives. Parker was asked in 1978 to write an article on his work for *New Scientist* after a series of nine articles in various journals between 1970 and 1975, in the course of which he presented a mathematical model for various aspects of the mating behavior of dung flies. The model has a purpose in a larger sociobiological controversy; it shows that the behavior he had observed accords with the assumption that certain behaviors are selected through evolution. Unlike Crews, Parker had had no experience with popularization before, and he found it took a considerable investment of time and rewriting to learn to write for this new audience.

In each case, nonbiologists have difficulty conceiving of the activities of these researchers the way the researchers themselves see their activities. One source of the difficulty is suggested by Ernst Mayr's comment on the popular reporting of biology.

> Discoveries are the symbol of science in the public mind. The discovery of a new fact is usually easily reportable, and thus the news

> media also see science in terms of new discoveries . . . Yet to think of science as merely an accumulation of facts is very misleading. In biological science, and this is perhaps rather more true for evolutionary than for functional biology, most major progress has been made by the introduction of new concepts or the improvement of existing concepts. (*Growth of Biological Thought*, p. 23)

In each of the cases I am presenting, what is so difficult for the public to understand is a concept based on evolution: coevolution of populations, adaptation of control systems, and evolutionarily stable strategies. What makes these concepts so difficult, I will argue, is not that they are forbiddingly abstract, but that in order to see why they are useful concepts one must also see science as a set of socially defined disciplines in which there is conflict and change. The news media present science as an accumulation of facts, not just because such an accumulation is more easily reportable, but because the value of such an accumulation to the public is reassuringly certain.

The value of discipline-specific conceptual structures and of debates among scientists is not so easily seen. Thus the popular accounts of the researchers I study stress their discoveries. A reader of Gilbert's article in *Scientific American* will picture him walking though the jungle and discovering the struggle between the butterfly and the passion vine. The reader of *Science* will see him, if he is imagined at all, in his greenhouse or his office, manipulating nature and marshalling the textual support of other researchers. A reader of *Scientific American* will picture Crews with his crew in Manitoba, learning all he needs by watching the snakes, or cutting them open and seeing their structure, without any experimental or conceptual mediation. They may even think he discovered these creatures, though he cites the earlier workers who studied them. A reader of *Science* will picture him performing assays and making inferences from them. A reader of *New Scientist* will imagine that Parker's main activity is lying in fields watching dung flies, while another biologist, reading *Evolution*, would see his work as devising mathematical models.

In conversation, the authors describe the differences between their articles for professionals and their articles for popular audiences in terms of levels of information: they can't go into so much detail, or can't mention all the qualifications, for a general audience. This description is consistent with a view of science as an inductive activity in which facts lead to concepts. I argue that the information is there in each of the popular articles, but the connection to scientific activity is lost. In emphasizing the narratives, rather than the information, I try to show how different views of science can frame the same facts. These two views can be distinguished in an analysis of the sorts of textual features that most obviously distinguish the two kinds of article: the titles, abstracts, opening sentences, organizational devices, and illustrations.

Titles

Titles are crucial indicators because they show what the authors or editors think will arrest the eye of the typical reader skimming the title page, or will categorize the article correctly for a reader looking for it in an index.[4] Williams and Gilbert's *Science* title, "Insects as Selective Agents on Plant Vegetative Morphology: Egg Mimicry Reduces Egg Laying by Butterflies," states an index heading and a claim to be proved. The *Scientific American* article, "The Coevolution of a Butterfly and a Vine," states a topic to be described. In the same way, the Garstka and Crews *Science* article implies a claim: "Female Sex Pheromone in the Skin and Circulation of a Garter Snake." The title supplied by the *Scientific American* editor, "The Ecological Physiology of a Garter Snake," states a topic. In both cases, the titles imply two different time scales, one of the time of an experiment showing reduced egg laying or pheromone presence in the skin and circulation, the other of the millennia required for evolution of these populations, or the months required for reproduction of the snakes.

Parker's *Evolution* and *New Scientist* titles hardly seem to refer to the same topic. The *Evolution* title is bewilderingly precise:

> The Reproductive Behavior and the Nature of Sexual Selection in *Scatophaga stercoraria* L. (Diptera: Scatophagidae). IX. Spatial Distribution of Fertilization Rates and Evolution of Male Search Strategy Within the Reproductive Area.

This specifies an ethological topic (sexual behavior), an evolutionary topic (sexual selection), the scientific name of the species, and the relation of a quantitative finding (spatial distribution) to a behavioral feature (male search strategy). From the point of view of a biologist skimming the table of contents, the most important words in this title are the *ands* that link topics normally considered separately, while leaving the article to say just how they are linked. The *New Scientist* title links the two areas most intriguing to a general reader: "Sex Around the Cow-pats."

4. Charles Bazerman, "Physicists Reading Physics."

Abstracts

The abstracts of the articles by Gilbert and by Crews and Garstka (Parker's have no abstracts) confirm the emphasis on the work of the scientists and its importance to other scientists in the articles for professionals, the emphasis on nature and its fascinations in the popular articles. For instance, Williams and Gilbert's abstract for *Science* suggests the structure and argument of the article:

> Experiments show that *Heliconius* butterflies are less likely to oviposit on host plants that possess eggs or egg-like structures. The egg mimics are an unambiguous example of a plant trait evolved in response to a host-restricted group of insect herbivores.

The subject here is *experiments,* actions scientists perform; the structure of the article will follow the experiment/control comparisons. The key adjectives for showing the importance of these experiments are those that claim an *unambiguous* example of the relation in question, and with a *host-restricted group.* The *Scientific American* version changes the sentences into a narrative with the butterflies and vines as the subjects.

> *Heliconius* butterflies lay their eggs only on *Passiflora* vines. In defense the vines seem to have evolved fake eggs that make it look to the butterflies as if eggs have already been laid on them.

The key words here are the words that dramatize the situation: *in defense, fake eggs.* The narrative of this article will follow the time relation suggested in the summary: butterflies *lay,* vines have *evolved,* eggs *have already been laid.*

The abstract of the Garstka and Crews *Science* article also focuses on experiments, but on experiments that give a *since/then* structure to the article.

> Serums and extracts of tissues from the female garter snake (*Thamnophis sirtalis parietalis*) each act as a pheromone and elicit male courtship behavior when applied to the back of another male. Since pheromonal activity is present in the yolk and liver tissue of untreated females and can be induced with estrogen treatment in serums and livers of males, the pheromone may be associated with circulating yolk lipoprotein, vitellogenin.

The abstract makes an argument, in which the presence of the pheromone in yolk and its production in males given estrogen, taken together, suggest its association with vitellogenin. The *Scientific American* abstract is similar to that of Gilbert's article in emphasizing the unusual features of the story as natural history: the harshness of the environment, the precision of physiological control, and the spectacular appearance of the mating behavior:

> In order to survive in the harsh environment of western Canada the red-sided garter snake has evolved a precisely-timed cycle of physiology and behavior with several spectacular features.

Again the emphasis in the popular abstract is on the narrative of the animal.

Introductions

We can see by looking at opening sentences that the scientific articles by these authors are quite different while the popular articles make the three researchers' work sound similar. Like the titles, these openings are meant to attract the interest of a typical reader of the journal. For instance, Gilbert and Williams's *Science* article begins by outlining a problem for biologists:

> The idea of coevolution between insects and plants is attractive to biologists attempting to account for patterns of plant chemistry and the use of plants by insects. (1) However, it is difficult to demonstrate a causal connection between a plant characteristic and a particular selective agent [because most plants have so many plants and animals attacking them]. . . . One approach is to study plant groups that support only one or a few herbivore taxa.

Thus study of this plant, with only one major predator, presents "one approach" to the general evolutionary problem, an approach that is "attractive to biologists." Note that all three articles for professional journals have a citation after the first or second sentence; It is necessary to place the article immediately in the context of the literature.

The second sentence of Gilbert's *Scientific American* article also presents a problem, but it presents a problem for mankind, not for biologists:

> Perhaps the most significant category of ecological interactions in terms of the net transfer of energy in the global food web is the interactions between plants and animals.

The introduction goes on to discuss parasitic pollen carriers that both help and injure their hosts, so we are reminded immediately that this is a biological topic of great agricultural interest.

The opening of Garstka and Crews's *Science* article also stresses the way their findings fit into the existing scientific literature:

> In many vertebrates, urine, feces, and vaginal contents, as well as exocrine glandular products, function as sex attractants and serve to facilitate the location and recognition of mates (1). We now report an additional source for a vertebrate sex pheromone.

Stated this way, their findings would seem to be of interest mainly to other researchers on sex pheromones. The opening of their *Scientific American* article, on the other hand, stresses the problem the snake has in its extreme northern habitat, rather than the problem pheromone researchers have in locating pheromones:

> The red-sided garter snake (*Thamnophis sirtalis parietalis*) is found farther north than any other reptile in the Western Hemisphere. It ranges into Western Canada, where the winter temperature is often below −40° Celsius and the snowcover is often continuous from late September through May. . . . In the den the overwintering snakes undergo a set of profound physiological changes. Their blood becomes as thick as mayonnaise.

The reader is drawn into the article, not by a suggestion of the economic importance of garter snakes, but by the oddity of a snake in the Arctic.

Parker's professional and popular openings offer the widest contrast, for the whole first paragraph of his *Evolution* article is about concepts and approaches, whereas the first paragraph of his *New Scientist* article ranges over anecdotes of a number of species. The *Evolution* article begins:

> The present series of papers is aimed towards constructing a comprehensive model of sexual selection and its influence on reproductive strategy in the dungfly, *Scatophaga stercoraria*. The technique used links ecological and behavioral data obtained in the field with

> laboratory data on sperm competition, for which a model has already been developed (Parker, 1970a).

The appeal of this article to its biologist readers is the promise of a comprehensive mathematical model, and the link between findings of one method (field observations of behavior) and another (laboratory data on sperm competition). The *New Scientist* version begins:

> Why do peacocks sport outrageously resplendent plumage compared with their more conservative mates? Why do majestic red deer stags engage in ferocious combat with each other for possession of harems, risking severe injury from their spear-point antlers?

The reader here is drawn in by consistent anthropomorphizing of animal behavior: *sport, resplendent, conservative, majestic, harems, spear-point.*

Organization

One of the great popularizers of biology, J. B. S. Haldane, reminds scientists in "How to Write a Popular Scientific Article" that they will have to rearrange their statements for a popular audience, right down to the level of the phrases in a sentence.

> Try to make the order of phrases in your sentence correspond with the temporal or causal order of the facts with which you deal. Instead of 'Species change because of the survival of the fittest,' try 'The fittest members survive in each generation, and so a species changes.' Not that I like the phrase 'a species changes.' It would be better to say 'the average characters of the members of a species, such as weight or hair-length, change.' (P. 157)

Haldane's problem here, besides his usual conscientious wrestling with the qualifications necessary for precise statement, is how to reorder statements from the simultaneity of a research report to the chronology of what he calls "a coherent story." We see the same rearrangement in contrasting the professional articles we are studying to the popularizations.

Each of the professional journal articles constructs a different sort of narrative of similar materials, but these narratives all depend on rearranging a number of events into a simultaneous order of argument, Gilbert by comparisons, Crews and Garstka by since/then formulations, and Parker with the definitions of the parts of one formula. For example, Gilbert and Williams make the argument that the structures that look like eggs evolved to look like eggs, by linking findings in this sentence:

> That these structures have evolved specifically to mimic *Heliconius* eggs is indicated by the facts that (i) heliconiines are important defoliating agents of *Passiflora* (7); (ii) larvae of many *Heliconius* feed on congeneric eggs and larvae (6); and (iii) females exhibit great care in inspecting oviposition sites (6, 8).

The sentence compresses three separate narratives, concerning observations reported in 1975, 1977, and 1963, respectively, into one statement to serve as the starting point for the present research. Similarly, the article presents itself as one study among several parallel studies of coevolution; the notes cite supporting parallels in work on other butterfly species. At the end of the article there is a list like the opening list compressing all the successive experiments reported in the article into a simultaneous argument.

> We have demonstrated that (i) *Heliconius* females respond to the presence of eggs; (ii) this response has a strong visual basis (8) although chemical clues are not altogether excluded, and (iii), the response to egglike structures of *Passiflora* and to real eggs both reduces the possibility that real eggs will be laid after host discovery and increases the time required to oviposit.

Between the introductory summing up of the literature, and the closing summing up of the article's narratives, Gilbert and Williams's article is arranged in short narratives, each reporting a controlled experiment. Within each of these narratives, the sequence of events is arranged, not chronologically, but in a hierarchical order following the argument (figure 5.1). These narratives are dominated by the control group/experimental group comparisons, another kind of simultaneity. For the reporting of controlled experiments is framed to assure the reader that all the relevant conditions of one group (except for the experimental treatment) were experienced by the other; such reports are a way of reshaping time. Further narratives within narratives are contained in the notes setting forth materials and methods. The statement each narrative is to support comes at the end or near the end of the narrative, an order suggesting induction, the collecting of information leading to generalizations. Similarly the statement of the larger evolutionary importance of the *Heliconius/Passiflora* example comes near the end of the article, instead of at the beginning as in *Scientific American*.

Whereas the *Science* article is arranged by concepts divided hierarchically into small narratives of experiments, the *Scientific American* article is arranged in a large narrative following the activities of the butterfly and the vine.

> To answer this question one must understand three aspects of the interaction between the butterfly and the vine. The first aspect is how the female butterfly finds the host plant. The second is how the butterfly makes a choice between depositing its eggs or not depositing them. The third consists of the factors that affect the survival of the eggs and caterpillars after they are in place on the vine.

The experiments are still reported within this narrative, but they are subordinated to the chronology, instead of the chronology being subordinated to the argument.

Garstka and Crews's *Science* article also tries to make the events of research simultaneous, but their device is the since/then of result/cause argument, rather than the comparisons characteristic of Williams and Gilbert's controlled experiments. We have seen this structure, in which a series of details precedes a conclusion to which they seem to lead inescapably, in the abstract. It is also apparent in most of the paragraphs (I have emphasized the key words here).

> *Since* the female attractiveness pheromone of *Thamnophis* is present in the liver, *but not* in the fat bodies, of untreated females, *and since* estrogen treatment can induce the pheromone in the liver and serums of males, *we suggest* that the pheromone is either the lipoprotein vitellogenin or a lipid-rich part of that large molecule. *The finding* that yolk elicits male courtship when applied to males *further supports* this conclusion.

There are eight such since/then sentence structures in the seventeen short paragraphs of the article, most importantly in the abstract and the conclusion. The penultimate sentence of Garstka and Crews's article is similar in form to the sentence Gilbert and Williams use to bring their various findings to bear on one point:

> Because of the findings that (i) there is no sex or treatment difference in lipid staining within the epidermis, (ii) the epidermal lipid is trapped under a heavily keratinized layer, and (iii) lipid is present

on the outside of the skin, we suggest that the sequestering of the pheromone in *Thamnophis* is a consequence of an active process analogous to the ejection of poison in certain related snakes.

Another sort of atemporal arrangement in this article is the comparison to other species; findings on three other genera, from 1935, 1938, and 1980, are combined to show how the mechanism for this species could work. This sort of comparison is at the core of all Crews's work. It depends, not on the chronological sequence of research findings, but on the bringing together in one narrative of several separate sequences. So our focus as readers is neither on the organism, nor on the activities of the individual scientists, but on the conceptual structure of biology, the parallels between species and systems, in which these comparisons can be made.

In Crews and Garstka's *Scientific American* article, as in Gilbert's, the narratives of experiments are inserted into this larger framework of the narrative of the organism. So after the opening outline of the reproductive process before, during, and after mating, the article covers the isolation and action of the pheromone that attracts males, and the methods by which this pheromone reaches the skin. Then the article discusses the pheromone that makes the females unattractive when they have mated. Then it discusses hormonal relations after mating. Thus the experiments are seen as pieces fitting into a puzzle, the overall shape of which is given by the snakes' life cycle.

Parker's *Evolution* article also achieves simultaneity of a number of narratives, but its principle of organization is that of a mathematical formula. The formula describes what should be the end product of sexual selection; the males should behave so as to allow equal fertiliza-

Hierarchial order, as in the article

(numbers in parentheses are authors' notes; sentence numbers are added)

1.–In the first set of experiments, we examined the response of the butterflies to the presence of real eggs on *P. oerstedii*, , the host without mimics.

2. Host plants were available to the butterflies only during experiments, when females were presented with combinations of plant cuttings with and without eggs.

3. The cutting were of similar morphology, and *H. cydno* eggs were placed on tendrils near meristems where eggs are naturally laid.

4. Eggs laid in the course of each trial were immediately removed from the test plants.

5. Three types of *H. cydno* eggs were placed on the cuttings: bright yellow eggs, just as they appeared in the field; green eggs, which were eggs that had been tinted with food coloring and rinsed with distilled water to blend with the plants' coloring; and washed yellow eggs, which were yellow eggs washed with distilled water and which served as controls.

6. In each test of oviposition preference, the butterflies were offered four *P. oerstedii* cuttings; two had single eggs of one type and two had no eggs or had a single egg of a different type.

7. The cuttings were arranged at random with respect to one another and the buitterflies were allowed to oviposit until they lost interest in the plants.

8. Most trials lasted 1 to 2 hours and the butterflies laid eight to ten eggs per trial.

9. The oviposition behavior of *H. cydno* was consistent.

10. The butterflies, probably responding to a combination of olfactory and visual cues (11), usually noticed the host plants as soon as the plants were brought into the greenhouse.

11. While fluttering around a plant, they repeatedly tapped it with their antennae, then landed on the leaves to drum the cuticle with their forelegs, presumably using chemoreceptors to "taste" and further identify the plant (12).

12. They would then fly around the plant, tapping and searching for a satisfactory oviposition site, or reject the plant by flying away.

13. Often, when a butterfly noticed an egg or egg mimics, it would stop searching the plant and fly to some other part of the greenhouse.

14. Percent oviposition (ratio of number of eggs deposited to number of inspections) on plants with no eggs was significantly higher than on plants that had either a natural or washed yellow eggs present (Fig. 2, A and B) (13), indicating that the presence of a yellow *Heliconius* egg does indeed reduce oviposition on plants.

15. When eggs were laid on plants already bearing a yellow egg, they were usually placed several centimeters away on another part of the cutting.

The same experiment, reconstructed in chronological order

3. The researcher gathers similar cuttings (the gathering of the original stocks is described in a note).

5. a. Researcher gathers eggs and divides them into groups.
   b. Researcher tints green eggs.
   c. Researcher washes green and some yellow eggs.

3. The researcher places the eggs on the cuttings.
2. The researcher keeps the butterflies from plants, except during the experiment.
6. The researcher prepares groups of cuttings such as to offer alternatives.
7. The researcher presents the cuttings to the butterflies [and observes].
10. The butterflies notice the host plants.
11. The butterflies tap and drum the host plants.
12. The butterflies fly around the plant searching for a site, or fly away.
13. The butterflies stop searching and fly away if they see an egg or egg mimic.
4. The researcher removes any eggs laid.
8. The researcher stops the trial after 1 to 2 hours.
9. The researcher concludes that the behavior is consistent.

15. The researcher figures the significance of differences in ratios (using a method described in note 13).

16. The researcher presents this information on a graph.

Figure 5.1. Hierarchical and chronological order. Hierarchical order reset from "Insects as Selective Agents on Plant Vegetative Morphology: Egg Mimicry Reduces Egg Laying by Butterflies," by Kathy S. Williams and Lawrence E. Gilbert. *Science*, Vol. 212, 24 April 1981, p. 468. Copyright © 1981 by the American Association for the Advancement of Science.

tions for all localities around the cow pat. The first part of the article consists of a series of sections, each of which discusses a factor in the males' search, and each of which leads to a part of the mathematical model. For instance, one section compares the likelihood of successful mating for the male searching in the dung to rates for males searching on the grass, in relation to the total number of males searching. First Parker describes what the males do, then he describes what he and his wife did to observe the flies, and then he calculates "gain rates" for each strategy males could follow. This calculation fills that one slot in the formula. The formula removes the element of chronology from the flies' narrative, summing up the chances of all flies, and also removes the chronology from the scientist's narrative, telling us what he did only in its place in the development of the formula.

In the second part of the article, Parker compares the results predicted by the model to observations, and he attempts to account for the differences by introducing factors not included in the general model. Again the form is based on the relation between the formula and observation, but now, rather than derive parts of the formula from observations, he works out the whole formula and compares the results to his observations. Then he discusses the implications of the model for the evolution of sexual behavior, putting the implications last, as they are in the other two professional articles.

As with the *Scientific American* articles, Parker's *New Scientist* article must cover a much broader range of material than an article for a professional journal, summing up studies published over the course of years. Like the *Scientific American* articles, it organizes this material around the experience of the animal, in this case the male dung fly, first summarizing the mating process, then discussing the arrival of the males, the guarding by the males, the capture of the females as they leave, the behavior of the males after capture, and finally, the subject of the *Evolution* article, the strategies of searching. (This bit is discussed out of chronological order, perhaps because it requires an understanding of the other parts of the process.) In each section, Parker first calculates what the flies should do, then compares this to his observations. So the formal principle is the reverse of that of the *Evolution* article, in which behavior was given its narrative structure by the formula; here the formula is given its narrative structure by behavior.

Illustrations

The differences in the narratives of the articles for professionals and those for popular audiences are even more apparent in the illustrations than in the verbal texts. Because space is at a premium, most scientific journals discourage extensive photographs and figures. But the illustrations in a popular journal are a large part of the magazine's appeal to a casual reader; the illustrations in *Scientific American* are particularly lovely and eye-catching. They also contribute to the popular narrative's chronology, and to its focus on organisms rather than concepts.[5]

The Williams and Gilbert *Science* article has just two illustrations: a line drawing of the cuttings used in the experiment (figure 5.2) and a series of graphs comparing the rates of oviposition with various preparations of leaves. These show part of the preparation for the experiment, and summarize its results. The *Scientific American* article has beautiful drawings prepared from photographs provided by Gilbert, rather than stylized line drawings: detailed drawings of the butterfly and caterpillar, an elegant display of variations in leaf shape (figure 5.3). It also has three electron micrographs that, though they illustrate the rather subsidiary point that caterpillars can get stuck on spines, are the most dramatic of the illustrations (figure 5.4). The difference, then, is that *Scientific American* shows what these plants and animals look like; *Science* shows what Williams and Gilbert did. This difference is also apparent in the graphs used in *Scientific American* (also attractively done in colors; figure 5.5a); they illustrate all the graphs in the *Science* article except the one that shows the control group (figure 5.5b). This graph is unnecessary in the *Scientific American* presentation because it illustrates a feature of experimental design, not a feature of nature.

5. Nigel Gilbert and Michael Mulkay have discussed *Scientific American* illustrations in *Opening Pandora's Box*. They suggest that these illustrations give a physical reality to biologists' conceptions that are both more complex and less definite than the realistic picture would suggest. (A good example from the articles discussed here would be the cutaway drawing of a snake's skin in Crews and Garstka's *Scientific American* article.) I am making a somewhat different point about the apparent realism of the striking illustrations in the articles I am considering: not that they show details that are conjectural, but that they divert attention from the evolutionary argument to the appearance and stories of the particular animals and plants studied.

There is an excellent selection of articles on illustrations in Bruno Latour and Jocelyne Noblet, *Les 'Vues' de L'Esprit* (special issue of *Culture Technique* 14 Juin 1985). Michael Lynch discusses three scientific illustrations in detail and, in his notes, presents a very thorough review of studies of scientific illustration, in "Discipline and the Material Form of Images: An Analysis of Scientific Visibility," *Social Studies of Science* 15 (1985): 37–66. Lynch and Steve Woolgar have edited a special issue of *Human Studies* devoted to the theme, "Representation in Science"; my contribution is on the illustrations in *Sociobiology*, and other papers deal with a wide range of sciences (*Human Studies* 11 [1988]).

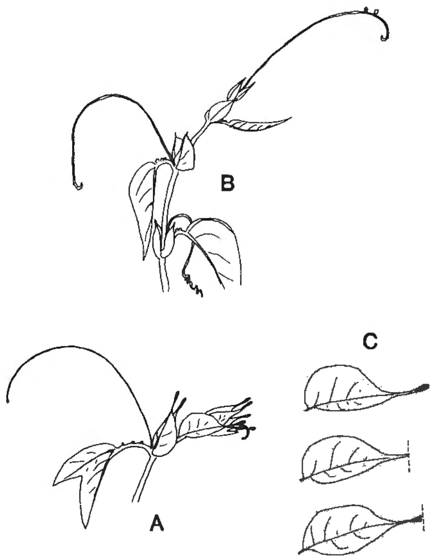
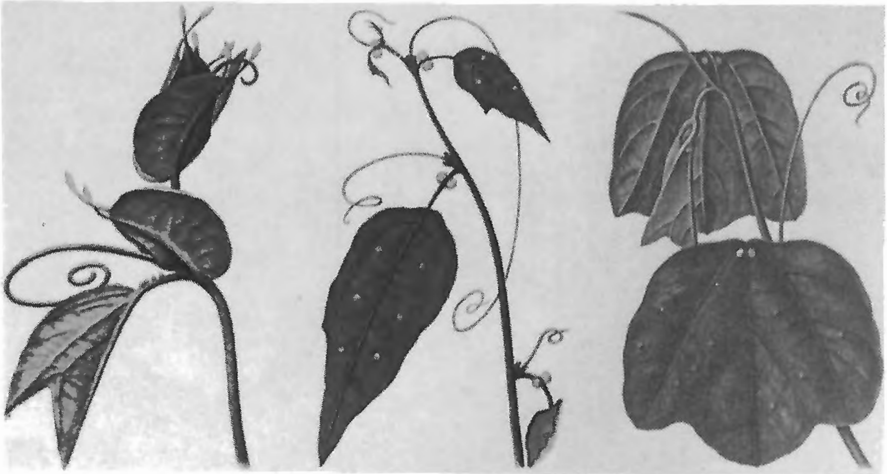


Fig. 1. ***Passiflora*** **cuttings used in experiment: (A)** ***Passiflora cyanea*****, showing display of egg mimics on stipule tips. (B)** ***Passiflora oerstedii*****, showing yellow egg (open circle) placed near green egg (closed circle) on tendril. (C) Enlarged view of** ***P. cyanea*** **stipules showing (top) unaltered stipule, (middle) stipule with egg mimic removed, and (bottom) stipule cut but retaining egg mimic for control.** ***Passiflora cyanea*** **stipules are 3 to 4 cm in length.**

Figure 5.2. A *Science* illustration. From "Insects as Selective Agents on Plant Vegetative Morphology: Egg Mimicry Reduces Egg Laying by Butterflies," by Kathy S. Williams and Lawrence E. Gilbert. *Science,* Vol. 212, 24 April 1981, p. 467. Copyright © 1981 by the American Association for the Advancement of Science.

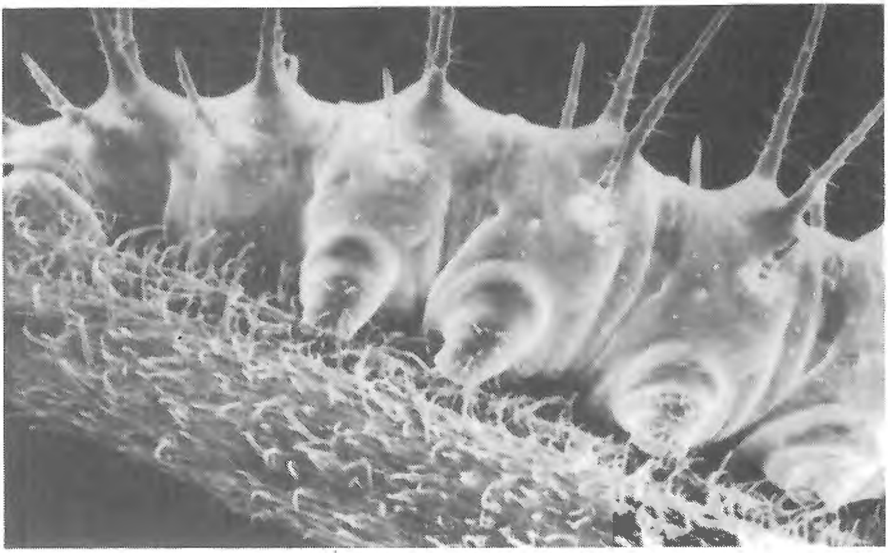
The illustrations on every page of Crews and Garstka's *Scientific American* article also focus attention on the garter snakes themselves rather than on the biological point about the garter snakes. Dr. Crews's articles for professional journals often have graphs showing cycles of various hormones, but the *Science* article has, and needs, no illustrations. The *Scientific American* version, on the other hand, has a cover painting of the snakes, done by Ted Lodigansky, an artist commisioned by the journal, who worked from frozen specimens provided by Dr. Crews. The article is dominated by a color photograph, opposite the first page, of a mating ball, a large mass of male snakes. The next two pages of the article feature a series of drawings, done by a *Scientific American* artist from Dr. Crews's photographs, of the mating behavior

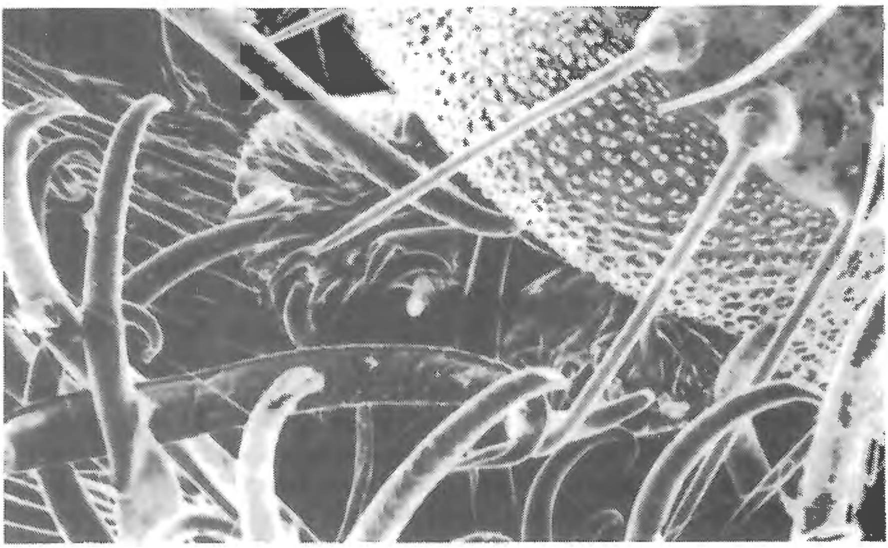


EGGLIKE YELLOW STRUCTURES appear on the three species of *Passiflora* (passion flower) vines shown in this painting. They mimic the yellow eggs of *Heliconius* butterflies that lay their eggs on the vines. The larvae of the butterfly then feed on the vine. At the left is a stem of the *Passiflora* species *P. cyanea;* the main modified egglike structures are the swollen ends of stipules: paired leaflike appendages. In the middle is a stem of the species *P. auriculata;* the main modified structures are nectar glands of the leaf stem. At the right is a stem of an undetermined species of passion-flower vine of northeastern Peru; the main modified structures are nectar glands of the leaf near the point where the leaf is attached to the leaf stem. In this species delayed expansion of stem-developing leaves keeps growth points hidden behind leaf displaying fake eggs. Growth points are vulnerable to being fed on by caterpillars that hatch out of real eggs.

Figure 5.3. A *Scientific American* illustration. Painting by Tom Prentiss, from "The Coevolution of a Butterfly and a Vine," by Lawrence E. Gilbert, *Scientific American*, August 1982, p. 111.

of garter snakes (figure 5.6). These four drawings outline the stages that, as I have suggested, provide the narrative for the article. The next two pages feature graphs of hormonal and gonadal cycles, illustrating the central findings of Dr. Crews's studies (figure 5.7b). Similar sorts of graphs in a later article in the journal *Hormones and Behavior* are such more stylized (figure 5.7a); *Scientific American* includes at each stage a little picture showing sperm in the testicles or showing little snakes growing in the eggs and then hatching. These certainly help the unbiological reader see what the stages mean, and they attract attention to what would otherwise be an off-putting graph. But they also help focus attention on the organism rather than on the concept of cycles, or on the measurement of hormonal levels and gonadal sizes that are the data reported here. The next two pages feature textbook-style illustrations of reproductive anatomy and some color micrographs by one of Crews's colleagues. These too give a sense that one is seeing the organism directly, rather than through the





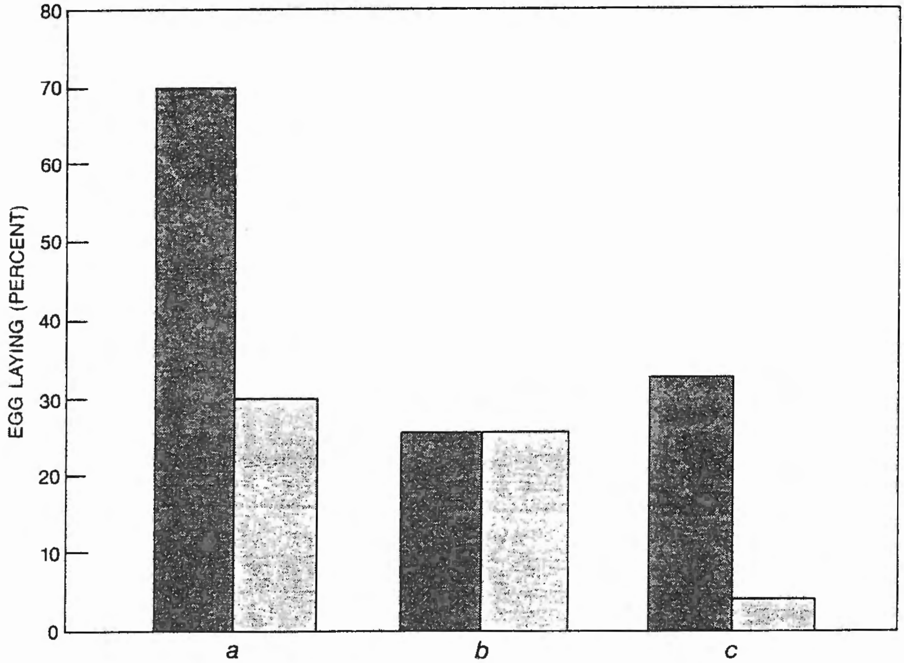
mediation of scientific theory and experiment. Finally the article illustrates the skin of the snake in a cutaway view like a radial tire advertisement, showing the hexagonal network of capillaries through which the pheromone reaches the skin (figure 5.8). It shows the path of the vitellogenin so clearly that the reader may wonder why Garstka

CAPTURED HELICONIUS CATERPILLAR, one of its prolegs hooked by a sharply pointed hair on the leaf stem of a *Passiflora* vine, is seen in this series of scanning electron micrographs showing another defensive measure of some species of the plant. At the top [A] the entrapment is not apparent; the hooked leg is the third from the right. In the middle [B] the tip of the plant hair has penetrated the surface of the proleg at the center of the micrograph. At the bottom [C] the tear that has been made in the proleg by the plant hair is visible just above the hook's point of entry. The vine in the micrographs is *P. adenopoda;* the caterpillar is *H. melpomene.*

Figure 5.4. An electron micrograph. These micrographs were arranged in a column when originally published in *Scientific American.* They have been rearranged here. From "The Coevolution of a Butterfly and a Vine," by Lawrence E. Gilbert, *Scientific American,* August 1982, p. 119. Copyright © 1982 by Scientific American, Inc. All rights reserved.

and Crews, or their predecessors, had any difficulty tracing it. Most of the *Science* article is devoted to the complex argument necessary to show that this is likely to be the pathway.

Parker's *Evolution* article contains three graphs illustrating the probability of capture of females (in the first part of the article) and comparing predicted and observed profiles for various search strategies (in the second part). The *New Scientist* article begins with a series of photographs of the mating process that function like the drawings illustrating Crews and Garstka's *Scientific American* article. It also includes three figures from his professional articles, with new captions. Considering the informal tone of the article, these presentations of mathematics come as a surprise. But we should note the way they isolate the mathematics from the rest of the article. And there is an interesting difference in the captions. The *Evolution* caption includes various adjustments and ends cautiously: "To emphasize that this profile must be regarded



COLOR DISCRIMINATION in *Heliconius* females was demonstrated in a series of experiments with passion-flower vines. When *H. cydno* females were presented with a choice (*a*) between a vine bearing no eggs (*gray bar*) and a vine bearing an egg (*colored bar*), in a total of 217 inspections the butterflies selected the egg-free site 70 percent of the time. To determine whether color or chemical cues govern this behavior the butterflies' next choice (*b*) was between a vine bearing no eggs (*gray bar*) and a vine bearing an egg that had been dyed green (*colored bar*). In a total of 80 inspections the butterflies showed no greater preference for the egg-free site. Finally the butterflies were offered a choice (*c*) between a vine bearing a green egg (*gray bar*) and a vine bearing a normal yellow egg (*colored bar*). In 66 inspections the butterflies selected the site with the green egg more than 30 percent of the time and the site with the yellow egg less than 5 percent. Where the percentages in bars do not add up to 100 percent, the remaining fraction is accounted for by inspections in which the butterfly did not lay an egg.

Figure 5.5a. *Scientific American* graphs. The "colored" bars are those on the right. From "The Coevolution of a Butterfly and a Vine," by Lawrence E. Gilbert, *Scientific American*, August 1982, p. 114. Copyright © 1982 by Scientific American, Inc. All rights reserved.

as approximate only, half the grid lines from each axis are omitted as compared with the expected profile." The *New Scientist* caption ends more confidently: "The fit between the two is encouraging." This example supports Parker's comment, in an interview, that popular versions are less cautious than professional versions; the two illustrations give the same graph, but the professional article emphasizes the differences between the model and nature whereas the popularization presents the model as a reflection of nature.

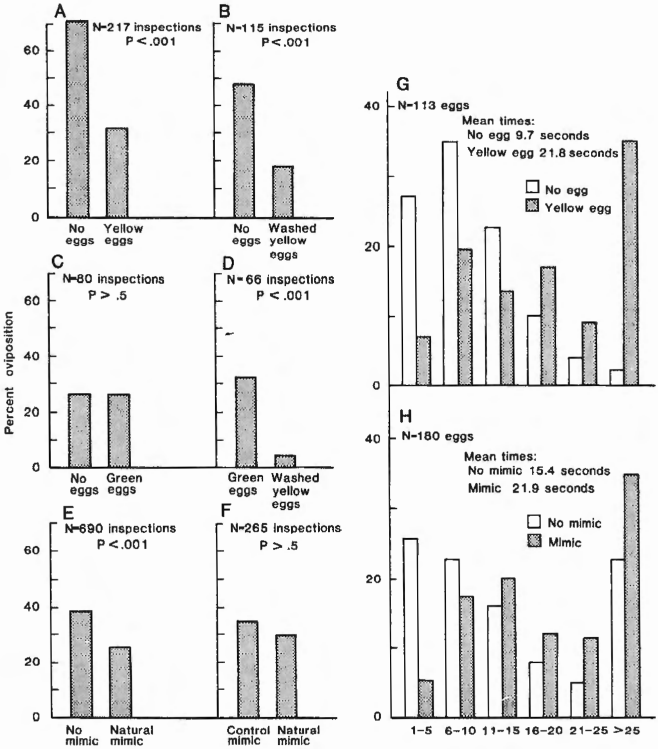
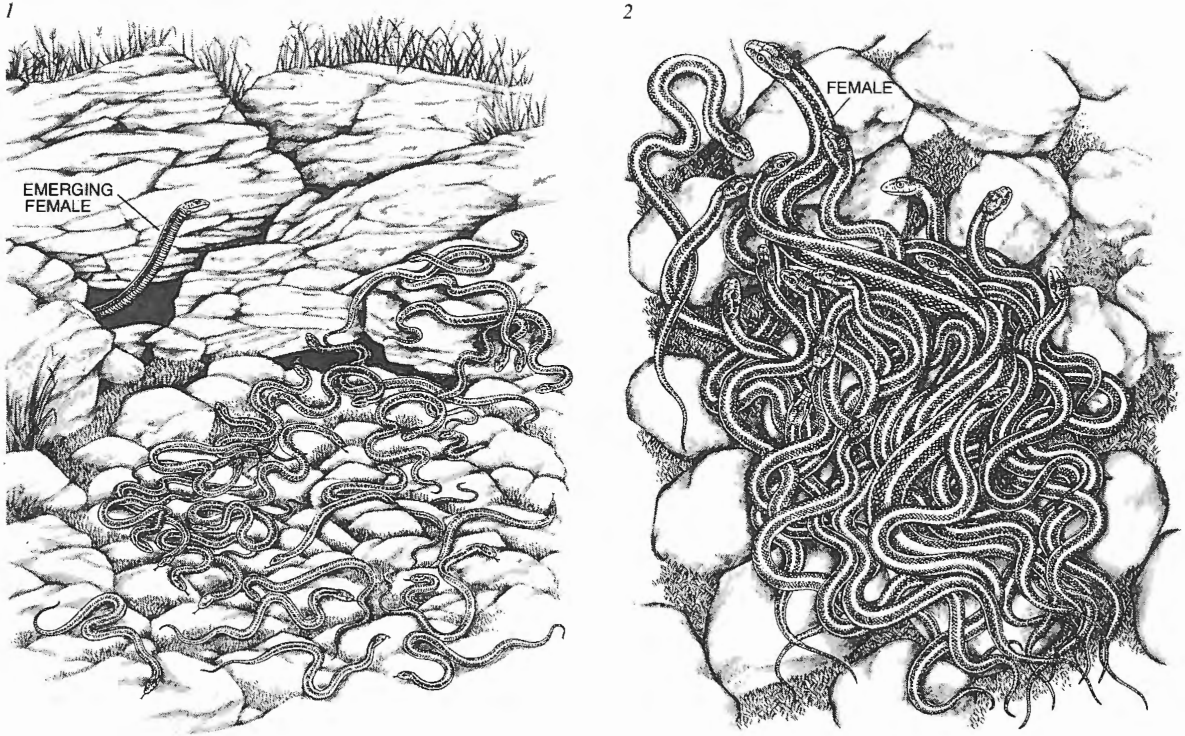


Fig. 2. Graphs which show oviposition response to various treatments. (A to D and G) Experiments were done with *P. oerstedii.* (E to G) Experiments were done with *P. cyanea.* In (G) and (H) the *x*-axis indicates seconds elapsed between recognition and oviposition. See text for details.

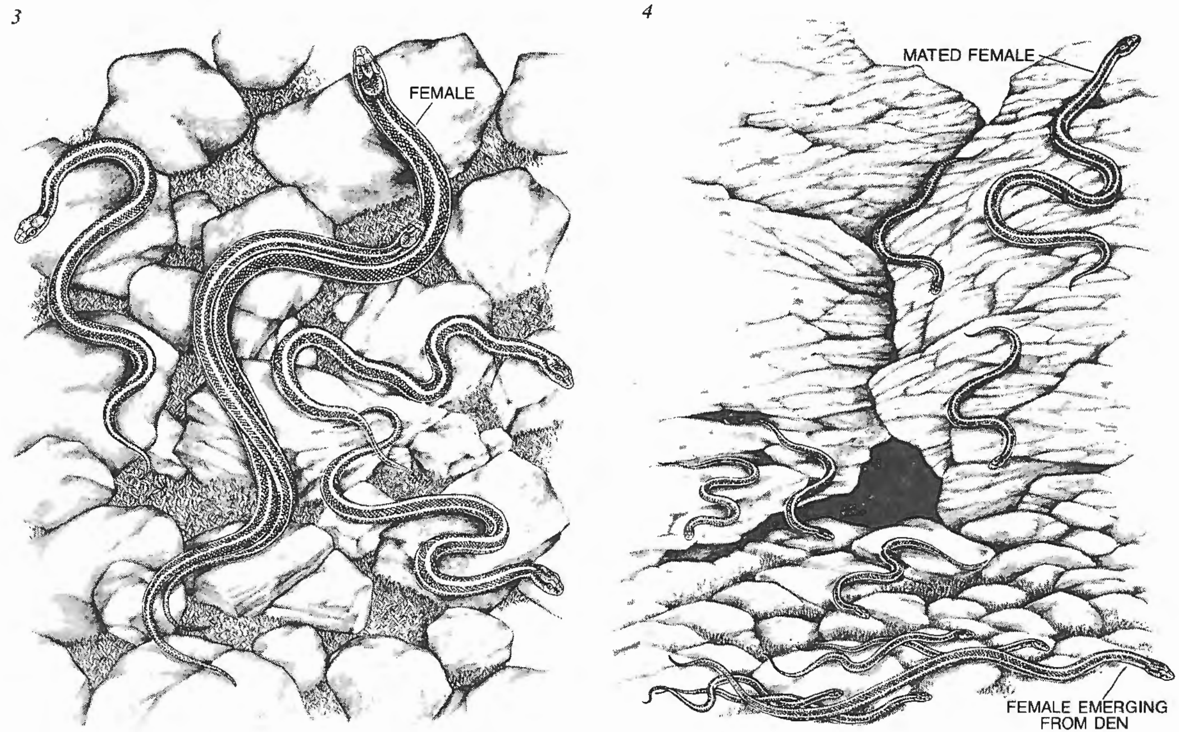
Figure 5.5b. *Science* graphs. From "Insects as Selective Agents on Plant Vegetative Morphology: Egg Mimicry Reduces Egg Laying by Butterflies," by Kathy S. Williams and Lawrence E. Gilbert. *Science*, Vol. 212, 24 April 1981, p. 468. Copyright © 1981 by the American Association for the Advancement of Science.

The Social Construction of the Narrative of Nature

The differences between the published texts of the popular and professional articles suggest two views of the activities of science. If we look at the revisions of the manuscripts of the popular articles by the editors of those articles, we can see these two views meet, and see how their differences are negotiated. I consider the changes made by the editors on three textual levels: (1) major changes of organization, (2) syntactical changes in many sentences, and (3) systematic changes in vocabulary. A nonscientist reader might see these changes as straightforward improvements that tighten the organization and make it easier to follow, bring out dramatic and memorable details, simplify syntax, and cut jargon. But the changes can also be seen as



MATING BEHAVIOR of the red-sided garter snake is confined to a short, intense springtime breeding season. For a period of from three days to three weeks the males sun themselves near the den from which they emerge. Females emerge singly or in small groups (*1*). Attracted by a pheromone (a messenger substance) on the back of a female, as many as 100 males form a "mating ball" (*2*). One male



in the ball succeeds in mating with the female by inserting one of his two hemipenes into her cloaca (her urogenital opening). The other males immediately disperse (*3*). The mated female, rendered unattractive to males by a pheromone her mating partner conveys into her cloaca, immediately leaves the vicinity of the den. The males stay near the den to await the emergence of another unmated female (*4*).

Figure 5.6. A narrative in illustrations. Illustration by Patricia V. Wynne from "The Ecological Physiology of a Garter Snake," by David Crews and William Garstka, *Scientific American*, November 1982, pp. 160–61. Copyright © 1982 by Scientific American, Inc. All rights reserved.

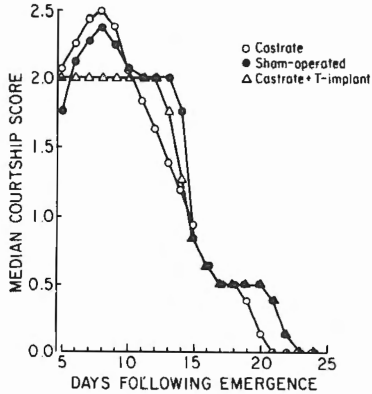


FIG. 1. Changes in courtship behavior of male red-sided garter snakes (*Thamnophis sirtalis parietalis*) on emergence from low-temperature dormancy. In nature following hibernation, or in the laboratory following low-temperature dormancy, courtship behavior initially is vigorous but then gradually declines in intensity; males will not exhibit courtship behavior again unless exposed to cold temperatures. Depicted here is the decline in courtship behavior in males that were castrated, castrated and given testosterone replacement therapy, or sham-operated in the fall prior to entering winter dormancy.

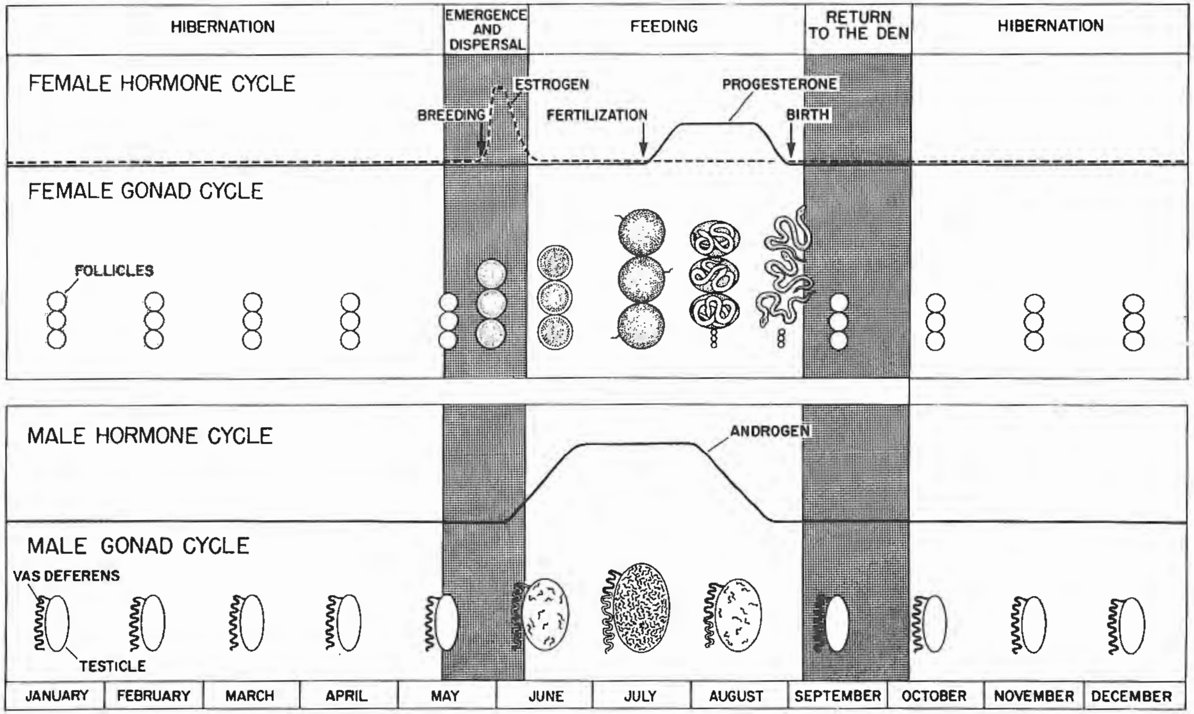
Figure 5.7a. A *Hormones and Behavior* graph. Illustration from "Hormonal Independence of Courtship Behavior in the Male Garter Snake," by David Crews et al., *Hormones and Behavior*, Vol. 18, p. 34. Copyright © 1984 by Academic Press, Inc.

subtly changing the message of the article, changing a narrative of science into a narrative of nature.[6]

6. A similar variability of views is evident in responses to my own paper. For instance, Gail Vines (an editor at *New Scientist*, though not the editor of Parker's article) points out that, in my effort to stress the changes involved in popularization, I go too far toward taking the scientific texts as primary:

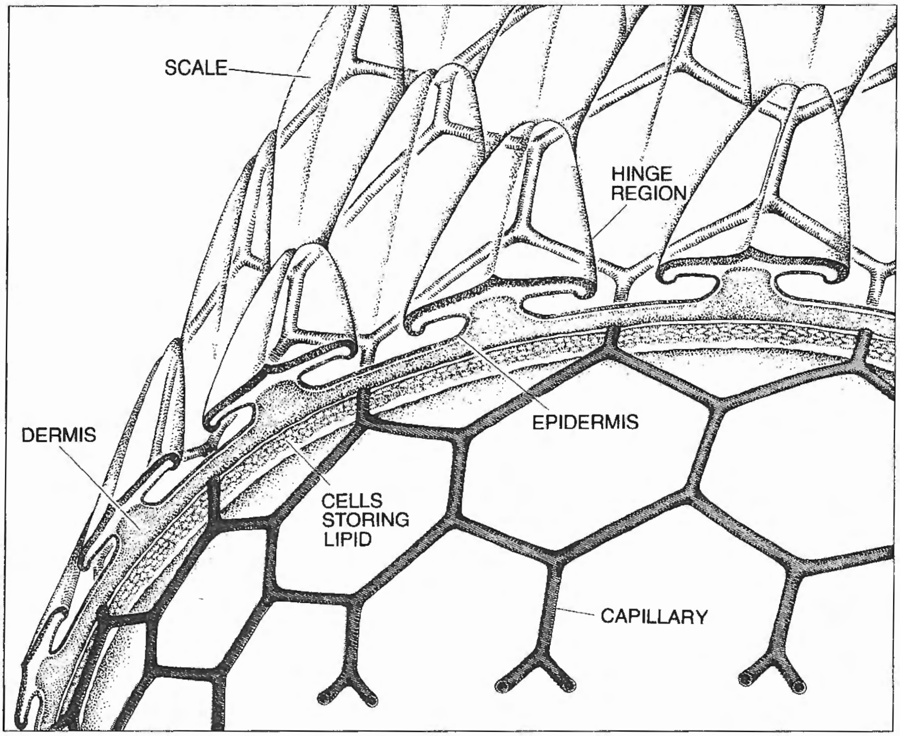
> "I think you are too kind to the scientists. The style of academic journals creates a misleading air of "objectivity" which I think can be dangerous to both science and the public. I take your point that such articles also set a study in an explicit theoretical framework, but so do many good popularizations of science. Most weeks in *New Scientist* at least one article will be "theory-led." I wonder about the generality of your observations.
>
> Popularizations of science often do start with "nature" but I don't see how one can make a physicist understand the concept of sexual selection, say, without a few good examples of the phenomena that are, arguably, a result of the process."

Dr. Vines may well be right in pointing out the ways these articles are not typical. I have dealt with a wider range of popularizations in two other papers, "Making a Discovery" and "Reporting Genetic Fingerprints." I discuss the issue of authority she raises in my concluding chapter.



MISMATCH of physiology and behavior characterizes the reproductive behavior of the red-sided garter snake. From January through early May the snakes are in their den. In the female the blood level of the sex hormone estrogen is low, and the gonads (the ovaries) contain only small egg cells (follicles) lacking a yolk. In the male the blood level of the sex hormone androgen is low, and the gonads (the testicles) are small. The male's vas deferense, or sperm duct, is packed with stored sperm. The snakes emerge and mate late in May. Their gonads are still small and their sex hormones are still at an ebb. Only after mating are changes observed. In the female the mating causes the level of estrogen to rise. In response the eggs grow large and are filled with yolk. In the middle of July the eggs are fertilized by sperm the female has stored for six weeks. Then the level of progestorone, the pregnancy hormone, rises. In the male the level of androgen starts to rise at a time when the females have left the vicinity of the den. During the summer the testicles grow large and produce the sperm the male will need the following spring. In August or early September the female gives birth, and by about the end of September both the male and the female have returned to their den.

Figure 5.7b. A *Scientific American* graph with iconic illustrations. Redrawn from an illustration by Patricia V. Wynne from "The Ecological Physiology of a Garter Snake," by David Crews and William Garstka, *Scientific American*, November 1982, pp. 162–63. Copyright © 1982 by Scientific American, Inc. All rights reserved.



PATH OF VITELLOGENIN onto the back of the female so that it attracts males during the mating season is deduced from the presence of a network of capillaries and of cells that store fatty molecules in the dermis, the deep layer of the skin. The vitellogenin leaves the blood as it flows through the capillaries and then percolates through the hinge regions between scales.

Figure 5.8. A cutaway drawing. Illustration by Patricia V. Wynne from "The Ecological Physiology of a Garter Snake," by David Crews and William Garstka, *Scientific American,* November 1982, p. 168.

That there are differences is suggested by an editor's letter to one of the authors, sent with the edited version of the article. He seems to have expected the author to be surprised by the revison, because he asks the author to read the letter before looking to see what has been done to his manuscript. He has two different lines of explanation:

> As you will see, we feel that it is necessary for the article to come to grips with the main subject somewhat more quickly. If that is not done, the reader may lose interest and the battle of popularization will be lost at the outset.

> We also feel that a fair amount of simple-minded explanation is necessary in order to make your argument fully accessible to the general reader.

The question such changes raise is what the main subject *is* that the article is supposed to come to grips with, and what part of the argument has been made accessible.

Organization

We saw the differences in structure in the comparison of popular and professional articles; in the revisions, we find that the authors tend to organize their manuscripts with simultaneous elements as in their articles for professional journals, whereas the editors tend to bring out the narratives focused on the organisms. The authors set their findings in the context of disciplinary history and concepts, whereas the editors emphasize the direct confrontation with nature.

The opening paragraphs of the three manuscripts all differ considerably from the published versions. Interestingly, since scientists are often accused of lacking historical context, both Gilbert and Parker begin their manuscripts with bits of scientific history. Gilbert, after a brief visual image of the butterfly in the jungle, spends two paragraphs describing how the work of H. W. Bates on these species helped to support Darwin's theory, and how recent work of Ehrlich and Raven developed the concept of coevolution. Parker also begins his manuscript with a historical review of sexual selection since Darwin. The authors may have been following the conventions of articles for professional journals, which usually include a brief review of the literature in their introductions, placing the current work in that context. For the *Scientific American* editor, such a context is a distraction. When the *Scientific American* version does give some of the development of these concepts, the passage is prefaced with an apologetic statement: "To answer the question, we must turn to a bit of history. . . . "

In both cases these introductions are cut, presumably, as the editor says of one of the articles, to get to the point quickly. The point, for the editor, is the organism itself: Gilbert's butterflies, Crews's snakes, and Parker's dung flies. Thus the editor of Gilbert's article revised his manuscript opening to mention the interactions of plants and animals in the first paragraph and the concept of coevolution in the second. Gilbert then revised this revised introduction to begin with the idea of coevolution, giving some well-known examples in the second paragraph, and finally coming to the *Passiflora* and the *Heliconius* only in the middle of the third paragraph. What is negotiated in the back-and-forth of drafts is the proper way to introduce the research: *Scientific American* sees the concept of evolution as a way of explaining "one of the most remarkable interactions" of animals and plants, whereas Gilbert sees the concept as the "main subject," and the plants and animals as a way of studying it.

The cuts throughout Gilbert's article either move the focus away from evolutionary concepts and methodological issues, or sharpen the focus on the plants and the animals. For instance, the editor cuts Gilbert's statement of the null hypothesis in his outline of experimental design of leaf shape, his statement that understanding of the effect of *Heliconius* on *Passiflora* is only qualitative, his comments on the randomization of leaf mimics, and his hypothesis about the possible evolution of other antilarval traits in the species. All these passages introduce concepts and procedures of biology.

The narrative of the butterfly searching for a vine that I have used in the previous section as an example of the narrative of nature is almost entirely the creation of the editor, who reorganizes eight paragraphs of information into four, and sequences them around stages of searching numbered 1, 2, 3.

> To answer this question one must understand three aspects of the interaction between the butterfly and the vine. The first is how the female butterfly finds the host plant. The second is how the butterfly makes a choice between depositing its eggs or not depositing them. The third consists of the factors that affect the survival [of] the eggs and the caterpillars after they are in place on the vine.

The evolutionary point is explained in terms of this sequence of search and evasion: "Natural selection . . . would favor the mutant vine that was harder for the butterfly *to find,* that was less likely to be selected for egg-laying *once it was found,* and that was inhospitable to the butterfly's offspring *once they were hatched.*"

The correspondence between Crews and Garstka about their *Scientific American* article shows that the two authors discussed the implications of a temporal organization versus a thematic organization. Garstka, responding to Crews's first version, raises some "disagreements" about the emphasis on adaptation, and then goes on to raise questions about the organization:

> I strongly feel that the paper needs a central focus. The paper can't and shouldn't be built around a gee-whiz story of adaptation. I used post-nuptial gametogenesis in the previous draft, and here I've

> tried to follow a temporal progression, at least in the female stuff. . . . The paper should stick to the point, i.e. the animal, and not include other data (melanogaster and radix).

But their disagreement is not about whether the article needs a point, but about whether the animal is the point, or whether the point is rather a larger story of adaptation that includes these other species of garter snake, *melanogaster* and *radix*. Crews's outline of his preferred organization for Garstka stresses the progress of the research:

> The paper has four main section[s] in my view: description of the natural history and behavior, female sexual attractiveness pheromone, male inhibiting pheromone, and male sexual behavior—I am placing the male sexual behavior section after the perhormone [sic] stories because it is still incomplete.

The structure here is just the chronological narrative I have outlined, but the rationale for it follows his topics, not the mating process of the snake.

The negotiation between Crews and Garstka is like the negotiation between Gilbert and the *Scientific American* editor. Crews, like Gilbert and Parker, started with an introduction to the general concepts underlying his research: "Environmental extremes in temperature, food, and water require that animals have specialized physiological and behavioral adaptations to survive . . . " But Garstka revised the opening before the manuscript was submitted to start with the snake itself; in this he probably anticipated the preferences of the editor. Their general point is then in their fifth sentence: "The major problem we have addressed in our research is how the synchronization of physiology and behavior with environmental demands occurs in species that live in regions with extreme climates." The editor of *Scientific American* retains a version of this statement, but puts it in the twenty-first sentence, after the narrative of the hardships of the snake's life and the oddity of its mating behavior, which is expanded considerably from the manuscript. The extremes and images that draw in the general reader are emphasized by this rearrangement.

With the Crews and Garstka article, as in Gilbert's, the editor draws together scattered passages wherever possible to emphasize the narrative of the garter snake. For instance, on page 2 of the manuscript Crews and Garstka interrupt the story of the snakes mating to discuss the research of two herpetologists who showed that snakes will emerge only with warmer temperatures. The *Scientific American* version continues the story through the mating, the birth of new snakes, and the return to the den before discussing the herpetological research through which these events are known. As with Gilbert's article, the *Scientific American* editor has added adverbs suggesting time: "the males come out *first* . . . *then* the females emerge . . . all trying *simultaneously* . . . *meanwhile* the mated female embarks . . . *at the end* of the mating season. . . . *Early in the fall* . . . *then* the females and the males return."

The *Scientific American* editor also makes two small but striking additions to the Crews and Garstka article that are consistent with this organizational focus on the life cycle of the snake. The first paragraph of the *Scientific American* version ends saying "Their blood . . . becomes as thick as mayonnaise." Crews says this is based on something he mentioned in a phone conversation with the editor, but that the mayonnaise analogy was suggested by the editor. It is both strikingly effective and, Crews thinks, rather misleading; it has caused a number of questions and comments from other herpetologists. Later an added sentence says that when crows and ravens catch the exposed garter snakes they peck out the snakes' livers. This is rather distantly related to the topic of the reproductive physiology, and one of Crews's colleagues has commented in the margin of the *Scientific American* manuscript, "anecdotal." It doesn't fit with the research, but it does provide a striking detail for a narrative of the hard lives of these snakes.

Parker says in his cover letter to the editor that he tried, through many revisions, to make his draft of his *New Scientist* article "as interesting and conversational as possible." But he still follows, in his organization, the principle of his *Evolution* article, explaining how he deals with each term of a formula and each formula of his model, paying particular attention to how this apparently mathematical work relates to concepts in biology. The *New Scientist* editor makes changes that emphasize, rather, the researcher's own activity, on one hand, and the narrative of nature, on the other. For instance, Parker ends the introduction in the manuscript, "The approach I used was to make predictive models that could be compared with the observed strategies shown in nature by male dungflies." The introductory paragraphs of the *New Scientist* edited version end, "My approach was to make predictive models of optimal mating strategies. Then, notebook, pencil, and camera in hand, I set out among the cowpats to discover what the flies really did." The edited version is more vivid, but also changes the emphasis from a comparison of prediction and observation to simple observation of what the flies "really" do.

Most of the organizational changes by the *New Scientist* editor shift the emphasis away from mathematical concepts to simple observation. For instance, the first section of Parker's manuscript is headed, "Dungflies obey the input matching law," and the next is headed, "Models dependent on sperm competition." There are no headings in the *New Scientist* article. Most sections of the manuscript begin with a problem seen in terms of filling in the formula. "Many aspects of male dungfly sexual strategy are difficult to analyze unless we establish how the sperm from different males is used during fertilization." There follows a way of figuring mathematically what the chances are for each male. The editor replaces this with a simple question, "Why do males guard their females when they are laying eggs?" The effect of such changes, throughout the article, is to shift the emphasis. Consider the difference in meaning between these sentences from the manuscript and their revised version:

> With a suitable correction to include the effect of mating with virgins, this data can be used to construct a curve of expected gains (eggs fertilized) with time spent copulating (Figure 2). Gains follow an exponential law of diminishing returns with mating time.

The editor tries to avoid all this mathematics:

> But, as we determined in laboratory experiments, the benefits of prolonging copulation are subject to diminshing returns.

In avoiding the mathematics, the editor makes it seem that experiments reveal this relation without the need for mathematical interpretation.

Syntax

The editors' changes in the authors' syntax may seem to be merely matters of editorial taste, and often they are. But three kinds of significant changes are common in all three articles: (1) rephrasing of introductory statements as questions and answers; (2) rephrasing of compound and complex sentences into several more simple sentences; and (3) rephrasing passive and impersonal constructions in active voice. Each of these changes relates to the differing views of the authors and editors.

One of the most powerful syntactical patterns of popular science texts is the question and answer.[7] The reader is conducted through a

7. Kenneth Morrison analyzes this pattern in "Some Properties of Telling-Order Designs in Didactic Inquiry," *Philosophy of the Social Sciences* 11 (1981): 245–62.

sort of dialogue in which, ideally, each of his or her vague confusions is stated as a clear question, which then receives a clear answer. In such a format, the direction of research, the point of all these facts, is always clear. This direction is often clear to the researcher only in retrospect, as many episodes in the history of science tell us. The decision about which questions are to be asked in a discipline at any time is just what is at issue. The popular audience thinks of science in terms of large and universally agreed-on questions (How can we cure cancer? How can we build a satellite weapons system?). But these may have little relation to the way an individual research program proceeds. The effect of the question and answer structure of popular texts is to imply that in research, as in undergraduate education, the questions are always given, and that, as in undergraduate education, the answers must surely follow.[8]

Question and answer patterns are extremely rare in articles for professional journals; the question is usually implied by some lacuna in the literature as described in the introduction. For instance, Williams and Gilbert's *Science* article has no questions; the question is only implied when the introduction says, "it is difficult to demonstrate a causal connection between a plant characteristic and a particular selective agent." But Gilbert introduces the pattern twice in his manuscript of the *Scientific American* article.

> With respect to which traits of passion vines are mutant individuals likely to be more or less successful in avoiding attack by *Heliconius*?

> What traits of *Passiflora* have evolved to deal with *Heliconius* eggs and larvae that have appeared on the plant?

In introducing these patterns, Gilbert shows he is aware of the different needs of his popular readers. But the editor of *Scientific American* uses the structure ten times, to introduce almost every new line of inquiry. So, for instance, Gilbert uses this introductory statement:

8. The alternative view of scientific questions and answers is suggested by George Eliot's account of Lydgate's physiological research in *Middlemarch* (1870); he approaches a "more scientific" view of the body even though he makes the wrong assumptions and asks the wrong questions in his search for a primitive tissue. One famous account of disagreements within a field about appropriate questions that should direct research is James Watson's *The Double Helix*. The Norton edition of this best seller (ed. Gunther Stent, 1981) includes a number of essays and reviews commenting on the popularization of science.

> From a passion vine's perspective, the effect of discovery by *Heliconius* depends upon the plant's size and location.

The editor changes this to a question:

> What are the consequences for the passion flower of being parasitized by a *Heliconius* butterfly?

But this question arises only if one considers as relevant certain variations in the population of passion vines. The researcher is often primarily concerned, not with answering a question, but with making a question of what may not have been a question before.

Crews and Garstka's *Scientific American* article seems not to follow this pattern of introductory questions; they begin only two sections with a question in their manuscript, and the published version has only three such questions. But a closer look shows that many of their introductory sentences focus the inquiry in just the way a question would:

> *The question* addressed in our research . . . is how a species such as a red-sided garter snake, which lives in a region where the climate is extreme, comes to have its physiology and its behavior synchronized with the demands of its environment.

> Investigators . . . *have long been puzzled* by the fact that the skin of garter snakes is devoid of any obvious glands that might produce, store, or release such a chemical.

> *We did not know* how the vitellogenin gets from the blood to the surface of the skin.

> . . . the mismatch between the onset of mating behavior and the size of the ovaries *is paradoxical.* [emphasis added]

Crews and Garstka use key words suggesting a question or a problem to mark for the general reader the line their inquiry has taken, or seems to have taken, so as to weave together a number of seemingly scattered experiments into one string.

Parker uses the question and answer format consistently in his manuscript, before any editing. It includes six questions, one at the beginning of each new mathematical problem, so that the question comes to signal the beginning of a new topic. Even the conclusion, cut from the article, suggests a question: "How sexual selection might have operated on man was a problem that fascinated Darwin." As we have seen, the editor adds several questions at the beginning to catch the reader's interest. But, surprisingly, he does not keep Parker's apparently logical form, with a question for every section. This seems to be because the editor wants to emphasize the long narrative of the dung fly's mating, where Parker's questions focus attention on his mathematics. Parker breaks the narrative into more problems that the *New Scientist* editor, and perhaps the *New Scientist* reader, wants to deal with.

Another major syntactical revision in all three articles, the breaking of compound and complex sentences into more simple sentences, would seem to be a straightforward improvement of readability. Certainly the densely packed sentences of many scientific articles make them difficult to read. But often this dense packing is the equivalent, on the syntactic level, of the tendency to simultaneity we saw on the level of organization; the authors want to bring as many ideas as they can, at once, to support their assertion. If this is the purpose for such dense packing, then the simplification of syntax also alters the article's presentation of space and time.

At least eight times the editor rewrites a very dense sentence in Gilbert's *Scientific American* manuscript as two or even three sentences. For example, this sentence, though grammatically quite correct, requires the assimilation of background information and the organization of two categories of processes, within a contrast, within a cause/effect structure:

> Because most species placing eggs singly are cannibalistic as larvae, females adding eggs to both occupied and unoccupied shoots at random will leave less progeny than females possessing egg avoiding behaviors.

The editor unpacks this into an entire paragraph:

> To consider predation, the emerging caterpillars of most *Heliconius* species that deposit single eggs are cannibalistic. One may suppose, then, that a major criterion affecting the decision of a female of these species to deposit eggs or not to deposit eggs would be the presence of another female's eggs at the selected site. A mechanism favoring the avoidance of such sites could easily evolve because mutant butterflies with such a mechanism will have more

> numerous progeny than butterflies that deposit eggs at occupied and unoccupied sites randomly.

The editor's version gives the background in one sentence, the comparison in another, and the cause and effect in another. I assume it is easier to read (though some nonbiologists reading it have disagreed with me). So why do scientists persist in writing more tightly packed sentences? Here Gilbert wants to do several things *at one time:* to limit his statement to certain species who place eggs singly, to take into account their cannibalistic tendencies, to compare two patterns of female behavior, involving two appearances of vines, and to put all this in an evolutionary context. The structural terms that join the clauses and phrases in this complex sentence—*because, and, less . . . than*—are the key terms in an argument like Gilbert's, linking all his isolated observations and findings into a general explanation. The *Scientific American* version subtly alters his meaning by putting the decision of the butterfly first and the evolution of this mechanism second. It contributes to the sense a reader gets in the article, and in most popular articles in this field, of the purposefulness of evolution for the individual.

In the Crews and Garstka article, as in the Gilbert article, the most common change by the editor is the breaking up of single sentences into two or three shorter sentences. Ten of the twenty sentences in *Scientific American's* opening narrative are under eleven words long; only three of the first twenty of the sentences in Crews's manuscript are this short. As with Gilbert's article, the breaking up of these sentences subtly alters the relation of observation to concept. For instance, this sentence is hard to read partly because a complex process, and the location of an organ, are explained in a subordinate clause and an adjective phrase in a statement about researchers:

> John Kubie and Mimi Halpern have shown that the tongue-flicking investigation of the female's body by the male delivers pheromone molecules to the male's vomeronasal or Jacobson's organs situated on the roof of the mouth.

The rewritten version separates, syntactically, the observation of nature from its conditional statement as part of research, and transforms nominalized behavior (*tongue-flicking investigation*) into an active verb:

> The work of John Kubie and N. Mimi Halpern of the Downstate Medical Center of the State University of New York suggests how

> the pheromone acts. The male catches pheromone molecules on his tongue, which he repeatedly flicks as he nears the female, and the tongue delivers the molecules to the vomeronasal organs, which are on the roof of the mouth. The chemical-sensitive cells of the vomeronasal organs send signals over nerve fibers to the brain.

Again, this is easier to read, but it takes a statement about nature out of the frame of the research that demonstrates it.

The third major category of revision of syntax, the transformation of nominals and passives into active constructions, has frequently been discussed in analyses of scientific prose. Both *Scientific American* and *New Scientist* remind their contributors, who have become accustomed to the passive constructions of scientific journals, to use the active voice wherever possible. Still, Gilbert writes a sentence in this form:

> When branches of the host plant having similar oviposition sites were placed in the area, no investigations were made by the *H. hewitsoni* females.

The *Scientific American* editor rewrites this as:

> I collected lengths of *P. pittieri* vines with newly developed shoots and placed them in the patch of vines that was being regularly revisited. The females did not, however, investigate the potential egg-laying sites I had supplied.

Some readers see the second, active voice version as more realistic because it emphasizes the intervention of the scientist. But it emphasizes only his activity, not the conceptual framework he brings. Also, the second version, in making two sentences to describe his action and the butterflies' response, makes the claim a narrative.

Gilbert does not simply slip into impersonal constructions from an article-writer's habit; we can see his preference for them in looking at his comments on the *Scientific American* editor's version. For instance, he uses a long and rather difficult impersonal construction as the subject here in the manuscript version:

> The observation that inexperienced females are strongly attracted to wire models of tendrils . . . suggests . . .

Scientific American revises this to require Gilbert to supply a personal subject:

> For example [WHAT INVESTIGATOR OF WHAT INSTITUTION?], working with *Heliconius* females in the laboratory, showed that they were strongly attracted to wire models of passion vine tendrils. This behavior suggests that . . .

Gilbert, in his revision of the revision, changes this back to an impersonal contruction:

> Studies of young, inexperienced *Heliconius* females, carried out by Peter Abrams in my laboratory, showed that . . .

The information here is the same, but the emphasis is insistently on the studies rather than on the investigator.

Crews and Garstka have active sentences more frequently in their manuscript. But in their case, the *Scientific American* editor makes revisions that seem to have just the opposite effect from those in Gilbert's article. Here the manuscript version attributes a finding to the researchers in another field:

> Molecular biologists have established that estrogen acts on the fat bodies to induce the mobilization of stored phospholipids . . .

Scientific American rewrites this with the natural substance itself as the subject:

> They [phospholipids] are released into the blood when estrogen acts on the fat bodies.

So the mere proportion of active sentences will not tell us the degree to which the article emphasizes the activity of the scientist; here the editor brings out the narrative of nature, not the narrative of science, by making the sentence passive.

In Parker's article, too, revisions of syntax alter the meaning of the statements. One addition of a personal element that makes a difference in how we read the article occurs where he makes a strong claim for the relevance of his findings to natural selection:

> There can be no doubt that the behavior of male dungflies, with its intense struggles between males for females, offers impressive qualitative evidence for Darwinian sexual selection.

The *New Scientist* version keeps the statement, but in a qualified form:

> It seemed to me that the behavior of male dungflies, with their intense competition for females, offered impressive evidence for sexual selection.

In a scientific article, such a qualification would be a serious weakening of the claim, but here it seems to be added just to keep the personal tone and the detective-like narrative. (Note also that the *behavior* has the struggles in Parker's version, not the animals, and that the *New Scientist* version deletes Darwin.) It is interesting that Parker actually changes one of the editor's passive sentences back to active, questioning whether this sentence needs to be in the passive: "A hind's interest in sexual encounters . . . terminates once she has been mated with just once." Such a description of mating reminds us how loaded the active/passive distinction can be.

Terms

The most frequent changes in revision by the editors, and the changes the authors notice most, are the substitutions of popular terms for scientific terms. The substitutions may not change the informational content of the article, but some of them may change the narrative, again shifting attention from the narrative of science to the narrative of nature. For instance, the *Scientific American* version of Gilbert's article changes *oviposition* to *egg-laying* throughout, to Gilbert's annoyance. Certainly more people know the words *egg* and *laying* than know the word *oviposition*, and certainly the substituted phrase means the same thing. But *oviposition* is one of the many technical terms that changes a process into a concept. It may confuse some people, but it allows scientists to talk about this action as a category of behavior, as an entity in itself. Consider the similar term that begins this sentence in Gilbert's manuscript:

> Germination, and therefore small plants, occur[s] in forest gaps where disturbances such as treefalls and landslips have exposed the soil to sunlight.

The *Scientific American* version of this sentence changes the noun to a verb:

> A passion vine seed can germinate only on open ground where the soil is exposed to sunlight.

The *Scientific American* editor avoids the awkward ambiguity about the number of Gilbert's compound subject, but does so by making the *seed* rather than the *germination* the subject. Again the plant is given a narrative of its own.

The problem such revisions raise for the biologist is that nominalization is what allows him or her to talk about processes rather than organisms. One cannot talk about evolution, for instance as something an organism *does*. There is a difference in meaning between the phrase "divergence in the visual appearance of sympatric vine species" and the more readable *Scientific American* version, "Where different passion vine species coexist they differ from one another in leaf shape." The more familar words, *different* and *coexist* do not have the evolutionary meaning of *divergence* and *sympatric;* they do not suggest how these relations in the population came to be as they are through variation and selection.

Gilbert notices the tendency of the editor's changes of words to eliminate the activity of the scientist while foregrounding the direct reading of nature. The editor's version starts a new paragraph, after making the evolutionary point that mutant individual *Passiflora* having features that resist the *Heliconius* will contribute a larger proportion of seedlings to the next generation, with the transitional phrase, "This being self-evident . . . " Gilbert changes the phrase on the editor's typescript to "This being the case . . . ", and comments in the margin, "It sure isn't self-evident until you make the observations." Gilbert makes a similar change when he deletes the editor's *of course* in the sentence, "This suggests, of course, that the pressure of *Heliconius* parasitism has favored the evolution of passion vine leaves that deceive the female butterfly." Ironically the popular narratives, which often try to build up the authority of the scientist as a genius with an immediate relationship to nature, often end up leaving the genius with little work to do.

The terms in the Crews and Garstka manuscript are also changed in the editor's revision. Most of the technical terms—pheromone, cloaca, vesicles—are simply defined in appositives or in parentheses. For the more detailed accounts of experiments on hormones, many definitions are needed. These definitions show how much scientific texts depend on certain terms; the reader cannot follow the narrative of the experiments unless he or she knows, not just what these substances are, but why they would figure in this experiment, why they might be thought by other researchers to initiate sexual behavior in male garter snakes. Only a few of the changes of Crews and Garstka's words make the kind of conceptual difference that those in Gilbert's article do. For instance, the phrase "preceding testis growth cycle" is changed to "when the gonads are growing the previous year." This substitutes a familiar concept (*year*) for an unfamiliar one (*growth cycle*), but misses the concept of a cycle, the concept Crews illustrates with his graphs.

The *New Scientist* editor also finds that he needs to substitute more popular expressions for some of Parker's scientific terms: *gravid* becomes *heavy with eggs*. Some of the changes of vocabulary alter the reader's view of evolution. The manuscript has the definition of *sexual selection* in this sentence:

> Any inheritable characteristic that increases a male's mating chances should increase in the population because the male will have more offspring than other males.

The editor, in trying to simplify this statement, removes the element of competing for mates, leaving simply natural selection:

> any inherited characteristic that helps an individual produce more offspring than its competitors will become common in the population.

Similarly, Parker, in reviewing this revised version, changes the editor's phrase in the last sentence, which says that "behavior patterns evolve through passive selection of the most successful strategies." The word *successful* suggests a "survival of the fittest" tautology; Parker changes the phrase to say instead that evolution favors the most "fruitful" strategies, those which produce the most offspring. In another of the editor's revisions, "What determines the spatial distribution of searching males?" becomes "The males also face the problem of where to mate." The scientific concept is turned into the organism's narrative.

One crucial term that is all but eliminated is the *Evolutionarily Stable Strategy*, a concept at the heart of Parker's mathematical model. Parker introduces the term with a definition and an attribution: "To use a term developed by John Maynard Smith of Sussex University, we need to find an 'Evolutionarily Stable Strategy' (ESS)—i.e. a strategy which, once established, is unbeatable in evolutionary terms." *New Scientist* cuts the term, and the name of Maynard Smith, the originator of the mathematical approach to evolution furthered by Parker. Instead the editor substitutes, where necessary, the phrase "optimal strategy." That seems to be close in meaning, but again, the ESS specifies a process taking place in an entire population over the course of evolution; optimal implies the choice of an individual over a lifetime. The article uses the author's phrase only once, referring to the "so-called evolutionarily stable strategy." This is one of the devices for making the jargon seem unnecessary. Where possible, Parker changes "optimal" back to "evolutionarily stable" in reviewing the editor's typescript.[9]

Forms of Popular Science

Further study would be needed to show whether the distinction between the narrative of science and the narrative of nature applies to popularizations written by science journalists in general-interest magazines and newspapers. After all, if the narrative of nature were found only in such relatively sophisticated publications as *Scientific American* and *New Scientist*, it would not necessarily be very important in describing the views of the public as a whole. The articles in *Scientific American* and even in *New Scientist* are closer in form to scientific articles than are the reports in the science sections of the *New York Times* or the *Guardian* (which have excellent science reporting), or in *Time* or *Newsweek* (which are more sensational), or in the general news sections of newspapers. Articles in these publications are by science journalists, or journalists with no scientific background; they must compete for the readers' attention with daily headlines, football scores, and pictures of celebrities. As a first step toward applying the two narratives more broadly, as poles to which other articles can be related, let us consider more popular reports of the findings of Crews and Gilbert.

In chapter 4 I discussed the controversy following the publication of Crews and Fitzgerald's article in *The Proceedings of the National Academy of Sciences* titled " 'Sexual' behavior in parthenogenetic lizards (*Cnemidophorus*)." I have noted that both Cole and Crews published *Scientific American* articles on *Cnemidophorus*. But there were other popular texts responding to the news after the *PNAS* article. For instance, *Time* reported Crews's research in an article titled, "Leapin' Lizards! Lesbian reptiles act like males." The titles show how different the articles will be; the *PNAS* article puts "sexual" in cautious quotation marks, and gives its scientific name, while the *Time* article plays up parallels to human behavior. The *PNAS* article, like the other scientific articles studied, begins by presenting the findings in the context of the concepts of biology: "All-female, parthenogenetic species pres-

9. I have developed the syntactic and lexical comparison between scientific and popular articles in more detail in an unpublished article, "Lexical Cohesion and Specialized Knowledge" *Discourse Processes* (forthoming).

ent a unique opportunity to test hypotheses regarding the nature and evolution of sexuality." The *Time* article begins, "Readers of science journals know a good deal about bisexual aphids, 'homosexual' gulls, and 'transvestite' fish." The emphasis is on the strangeness of nature, but also on its accessibility through familiar concepts, even through familiar human stereotypes.

The subjects of the sentences in the *PNAS* article, like those in the *Science* and *Evolution* articles used for comparison, tend to be nominalized activities: "The initial observation. . . . Dissection revealed that. . . . A female-female mounting was also observed." The article does not strictly follow the Introduction—Methods—Results—Discussion format; methods are relegated to a caption, and a strong sense of chronology is retained. But it reads coherently because it focuses on the narrative of the scientist (the level of the narrative of the study in chapter 4), from collection, to observation of the activity, to dissection, to observation of related species. *Time* has two kinds of narrative, an opening and closing in which the sentence subjects are always the researchers or the readers of research, and a central section in which the subjects are the lizards. The central section is quite scientific in tone:

> An active female mounts a passive one, curves the tail under the other's body, strokes the partner's back and neck, and rides on top for one to five minutes. The active female lizard always has small undeveloped eggs, while the passive female has large pre-ovulatory eggs. But there are cyclic variations in behavior and egg size in these reptiles, and the roles reverse.

But there is no discussion of the experimental arguments supporting the observation of such behavior. The framing narrative is not the activity of the scientist, but the comedy of a scientist humorlessly watching lizards mate. Crews's own words are satirized:

> Says Crews, "We are now trying to determine whether this male-like behavior facilitates reproductive function." Translation: the psychobiologist does not yet know why the females mock the male-female behavior of related two-sex species.

Note that *Time*'s translation is not easier to understand than Crews's version; the translation is a way of characterizing scientific jargon as a smokescreen. The paragraph on theoretical implications stresses the dubiousness of any theorizing: "It is too early to announce. . . . " Real science, in this view, is just watching and waiting. The *Time* article reassures its readers that Crews, though he makes startling discoveries, doesn't really know anything they don't know. Crews is special, rather, because he has immediate contact with nature. He is an explorer to be admired for his feats.

The narratives of other news articles provide further evidence for Mayr's assertion that science journalism is more interested in discoveries than in concepts.[10] One example is the profile on Crews recently published in an *Esquire* issue devoted to "The Best of the New Generation: Men and Women Under Forty Who Are Changing America." The entry presents his work as that of discovery and practical application:

> Crews' discoveries about the reproductive patterns of reptiles have challenged some time-honored assumptions about human sexual behavior. Based on his studies of the all-female whiptail lizard, Crews theorizes that "sexual behavior" preceded the evolution of sex. He has established that certain male behavior can both stimulate and inhibit female ovarian growth, a discovery that helps explain why overcrowded animals often experience a drop in birth rates. "Crews has unraveled an important piece of information with application to humans—that the human brain has the potential to go in either a male or female direction," says Dr. John Money, professor of medical psychiatry and pediatrics at Johns Hopkins University.

Though time-honored assumptions are mentioned, it is not suggested that Crews is trying to transform the concepts of his own field, but rather that he is working against unscientific superstition. The biological concepts, in the popular view, must have been scientifically established. He is put in two familiar roles: the theoretical iconoclast who challenges unscientific prejudices, and the practical medical technologist who produces work with applications to humans.

A more restrained example of such popularization in general-interest publications is an article on Gilbert's work in the (London) *Times* in August 1982. Pearce Wright, the Science Editor, presents only a summary of the *Scientific American* article, but he selects from it and rearranges to make it appeal to a casual reader. As in the *Time* article, the organisms studied are anthropomorphized; the headline reads, "Deceiving vine keeps butterflies at bay." Gilbert's work is mentioned briefly, emphasizing his persistence ("ten years of field

10. Mayr, *Growth of Biological Thought*, p. 23.

observations and controlled experiments in botanical conservatories . . . "). But the article's angle is to present Gilbert as a Darwinian with a "theory."

> At a time when controversy is rife about the theory of man's evolution, Professor Gilbert, director of the Brackenridge Field Laboratory, University of Austin, and a postgraduate of Oxford University, indicates that the idea that plants and animals interact on each other to influence their respective path of evolution may raise eyebrows.

Controversy is not rife about the evolution of man among population biologists, but it is rife in Gilbert's home state; the *Times* presents Gilbert as he would be seen by the public, not as he would be seen in his discipline. As in the articles on Crews, the disciplinary and conceptual basis of the work is excluded.

A recent article on Gilbert in the University of Texas alumni magazine *Alcalde* provides another example of this exclusion. The cover has a lovely color photograph of the *Heliconius,* and the article by Don Massa focuses on the organisms Gilbert studies, on the fact that he works in greenhouses, and on the possible economic applications of his work. Evolution is mentioned only in passing. Ironically, the article ends with a quotation from Gilbert that neatly defines the journalist's own approach: "If you work on butterflies as model organisms, people have difficulty seeing past how you study to ask what scientific questions motivate the work." Gilbert implies that a researcher will have special problems popularizing the scientific issues when the model organism is fascinating and beautiful. But as we have seen, the same sort of focus is possible in articles on such neglected and potentially unappealing animals as lizards and dung flies.

Gilbert's comment about misconceptions of his particular sort of research raises the question of whether we could expect to find the narrative of nature in popularizations of other scientific fields. Some biologists complain that popular biology articles are more heavily edited than those on physics in the same journals, that the physicists get to keep more of their vocabulary and physical constructs, partly because there are no familiar alternatives available. It may be that research on subatomic particles is harder to fit into a narrative of nature than research on butterflies and plants.[11] But there are other devices for

11. Gilbert makes this point in his response to a passage in my manuscript. "The terms of physics have no connection to familiar objects. Editors have no handy alternative to 'quark.' The same can be said of molecular biology. What do you do with

making the objects of physics seem immediately perceptible and unambiguous, such as pictures of the equipment, emphasis on the technology, or shaded diagrams of spherical particles. What I have called the narrative of nature draws on a tradition of natural history writing, but there are several other traditions of popularizations, such as the dialogue, or the detective story, that have similar effects, in foregrounding the activity of the object and obscuring the activity of the scientist.

Understanding the Public Understanding of Science

I have shown that the popularization of texts in evolutionary biology does not involve merely translating some technical terms, substituting active for passive voice, and focusing on some angle of popular interest, but in effect turns one sort of narrative into another. The narrative of science and the narrative of nature remain consistent, and consistently different from each other, because they support two different views of science. As I noted in chapter 1, a number of studies have shown how the form of the scientific article embodies the assumptions of the scientific community about the impersonality, cumulativeness, and empiricism of scientific knowledge. Such texts function to integrate researchers and their findings into the work of the research community. Researchers show that their findings are real because they meet disciplinary standards for methodology, they fit their work within disciplinary concepts, they submit the personal point of view to certain constraints. Each article is a demonstration of the need for scientific expertise.

The popular texts support an equally coherent and definite view of scientific practices, but one that is inconsistent with the view embodied by the scientists in their articles. In this view the scientist is alone, and proceeds without concepts or methodology, by simple observation of nature. There are no choices to make about the course of research, which proceeds from given questions to unambiguous answers. Just as scientists have an interest in promoting scientific expertise, the public, and those who edit journals with the public in mind, have an interest in this view, which minimizes expertise and emphasizes the unmediated encounter with nature. All scientific knowledge is brought within the

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'restriction enzyme' or 'transposon'? I objected to having 'petiole' (a word found in Webster's) changed to 'leaf stem.' Maybe it gets the idea across but I didn't care to be laughed at by my colleagues!"

Gilbert and Mulkay, in *Opening Pandora's Box*, p. 168, show a textbook picture in which biochemical processes have been anthropomorphized.

realm of common sense, all scientific knowledge serves public goals. Articles for the public, when they are well edited to appeal to a general audience, reproduce science as the public wants it to be.

The effect of popular articles on the public perception of science is apparent in public discussions of scientific authority. The century-old public controversy over Darwin that the *Times* editor mentions is perhaps a reflection of the tension between these two views. Evolution has been presented as a fact of nature, so the public thinks it must be unchanging, but it is also a concept of science, so it must be open to modification. When one of these biologists calls evolution a theory, he means it is a central disciplinary concept enabling further thinking about life. When the *Times* calls it a theory, the connotation is that it is another airy idea dreamed up by scientists; there is no place for theory in popularizations. In Texas, Gilbert's and Crews's home state, the state textbook commission for a time banned the use of the word *evolution* in biology textbooks except when it was labeled as a theory. I would argue that, in a more subtle way, popular science texts, and even those on Darwinian topics, tend to exclude evolution because of the way they tell their story. As long as the popularizations focus on individual organisms, a concept like evolution is very difficult to imagine.

The effect on scientific discourse of the split between these two narratives is less easily traced, but it may be equally important. My study tends to present the popular articles as versions, more or less accurate, of the professional articles. But D. R. Crocker, a biologist comparing best-selling books on ethology with academic books by the same authors, takes the interesting view that the popular texts have priority:

> I suspect the authors let their genuine feelings spill out into their nature books and that academic pressure to be objective simultaneously encourages them to dissemble. My bet is that the popular informs the academic rather than the other way around.[12]

As I suggested at the beginning of the chapter, I would not give the popular view priority in this way. The complex form of professional scientific texts cannot be explained merely as the result of academic pressure to be objective. But the ways in which the popular could influence the professional versions may be suggested by an anecdote

12. D. R. Crocker, "Anthropomorphism: Bad Practice, Honest Prejudice?" *New Scientist* 16 July (1981): 159–62.

told by one of my subjects. He heard that a senator was about to make his research—which can be made to sound pretty silly—an example in an attack on wasteful spending; he was told by sources who knew the senator that he had to explain in a letter of one page (the senator would read no more) just what his work had to do with the larger good of the nation. This letter might be taken as an unusually direct example of the public relations function of popular science. But this and other popularizations are not just exercises in persuasion; to find such words is also to find a place in the larger society outside science, and in some degree to adapt the research to it. Both Terry Shinn today and Ludwik Fleck fifty years ago have stressed the important part that writing for a general audience plays in the production of scientific knowledge.[13]

To show how the categories "narrative of nature" and "narrative of science" might apply to other texts, let me take my own paper as an example. I could rewrite it as a fairly straight sociological paper in an Introduction-Methods-Results-Discussion format, setting out my findings against the background of other sociological studies of popularization. Instead, I find as I rewrite it that it is becoming more and more like a popular article. Note, for instance, that I have focused on the scientists, just as they focused on the butterfly and the vine, or the snake, or the dung fly. I have arranged the texts in a sort of chronology, from professional article to manuscript of popular article to published popular article, giving the professional version the appearance of priority, in the same way the editors arranged the articles in the chronology of the animals involved. I have added introductory questions, and omitted quite a bit of sociological jargon. I have removed to the notes almost all my references to the work of other sociological discourse analysts.

The irony, then, is that the narrative I use works against the concept I want to present: it conceals the evidence of the construction of my own text within disciplinary practices. But I would prefer not to revise away this irony. Instead, I will use it to point out the same tension in the study of discourse as in the study of evolutionary biology: between assuring the world at large that there is an external object, totally accessible (in this case, the process of popularization), and assuring that the analysis of this object is possible only in the terms laid down by a discipline, only by someone with the proper expertise. In the cases of

13. M. Cloître and T. Shinn, "Expository Practice: The Social, Cognitive, and Epistemological Linkage," *Expository Science: Forms and Functions of Popularisation* (Dordrecht: D. Reidel, 1985), pp. 31–60; Ludwik Fleck, *The Genesis and Development of a Scientific Fact* (rpt., Chicago: University of Chicago Press, 1981), p. 118.

the articles by Gilbert, Crews and Garstka, and Parker, and in the case of my own study, the tension between these kinds of authority in popularizations does not become an issue for the reader because there is no controversy in the public forum that would prompt nonscientists to question the assumptions of the various disciplines, or to compare various claims to expertise. Such evaluations enter into popularizations in public controversies like those over the safety of nuclear power, the efficacy of star wars, the alleged relation between race and I.Q., or the claims of sociobiology—the subject of chapter 6.