critiques put forth by lawyers—fears of “liars, damned liars, and experts.” Was, then, this intense anxiety justified? Were there in fact many charlatans who would say anything for the right price? Or did the legal vitriol stem more from legal misunderstandings about the nature of science than from the prevalence of unscrupulous experts? Golan’s book illustrates but does not fully explain an intriguing rhetorical mismatch between the enormous skepticism expressed by legal commentators and the internal debates of the expert communities.

Throughout the 19th century, judges saw their screening role for scientific expertise as extremely limited. Generally they evaluated only the credentials of the expert, not the substance of the testimony. This changed, in Golan’s account, with the advent of the polygraph, which was seen to threaten legal fact-finding processes and even legal autonomy, because it suggested substituting the judgment of an expert, or still worse, a machine, for the judgment of a jury. According to Golan, the famous 1923 Frye test became the wedge through which scientific evidence began to get more substantive scrutiny, a trend that has continued and accelerated up to the present. Indeed, in his final chapter and his epilogue, Golan suggests that the story of the reception of expert evidence is a tale of an increasingly “exclusive elite,” a growing distrust that has led to the increased scrutiny and control of scientific information by the judiciary.

How Species Arise

David B. Wake


Charles Darwin was so convinced that species arose as an outcome of adaptation driven by natural selection that he did not consider alternatives. He also gave surprisingly little attention to species formation, apparently considering it to be an inevitable consequence of natural selection operating on diverse organisms in a heterogeneous world. The 1930s and 1940s saw the publication of two books focused on species: Theodosius Dobzhansky’s Genetics and the Origin of Species, and Ernst Mayr’s Systematics and the Origin of Species. Jerry Coyne and Allen Orr’s new book, Speciation, is clearly inspired by Dobzhansky’s. Coyne and Orr largely ignore the work of those who actually identify, define and describe species. Nevertheless, their book, which offers a critical analysis of the enormous lit-
terature dealing with speciation, is an impressive achievement of great depth and broad scope. It will be required reading for those studying species and species formation.

The authors are major contributors to research on speciation. They are Drosophila geneticists, and their work has taken advantage of the experimental opportunities provided by those model organisms and their relatives. In addition, Coyne and Orr have made important contributions to many theoretical aspects of speciation. Their experience is evident in the strategy developed for this book. The 12 chapters have predictable titles and cover predictable ground. But the book is novel in the depth of its scholarship, the exceptionally thorough treatment of central questions and the superb use of published literature. The references fill nearly 50 densely packed pages, and most of the literature cited is recent, much of it from the last 10 years, including a number of yet-unpublished papers. This inclusion of recent material suggests that the field is progressing so rapidly that the book will have a short life, but what will surely last is the analytical and integrative treatment of what we know now.

The authors' strategy is to select a topic and present a thorough introduction that goes into considerable depth. Next they review theory, treating some topics briefly and others in great detail. They consider experimental evidence and, where appropriate, survey evidence from nature. Topics that have an enormous literature—for example, allopatric speciation—are broken into subtopics (such as "concordance between present or past geographic barriers and genetic discontinuities within species"), which are then treated in detail. The logic of organization and presentation is excellent, and the quality of the writing is high.

Although the authors open with a vigorous argument for the reality of species, they admit that "we don't take the rigorous studies needed to convince skeptics that nature is discontinuous." Their discussion of species concepts is a spirited argument in favor of biological species, viewed from many perspectives. They do consider objections, often in depth. However, the second chapter ("Studying Speciation") makes clear that their research program envisions the evolution of reproductive isolation as the central, and nearly the only, issue. Coyne and Orr take the view that complete cessation of gene flow between incipient species is not required for speciation to occur; this gives them much latitude. They emphasize "isolating barriers," arguing that although some are likely to be more important than others, only comparative analyses can determine which.

Allopatric speciation is the favored mode, and Coyne and Orr give it the most attention, but they also consider parapatric speciation and sympatric speciation. The sharpest critiques in the book are of sympatric modes, which the authors consider to be possible but probably of minor importance. Coyne and Orr note that "parapatric and sympatric modes of speciation require that the evolutionary forces causing populations to diverge must be stronger than the gene flow causing them to fuse," whereas there are few roadblocks to allopatric speciation. However, in lengthy chapters on various kinds of isolation, the authors make clear that they have high standards for evidence. The examples are especially rich, though fruit flies, stickleback fishes, sunflowers and monkey flowers are invoked repeatedly. The most fly-heavy chapter deals, predictably, with the genetics of postzygotic isolation; here Coyne and Orr focus attention on their research relating to hybrid sterility and inviability. Their analysis of the number of genes required for speciation concludes that "total postzygotic isolation often involves many genes," although only a few genes may be necessary to initiate such isolation.

Polyplody speciation (based on chromosomal dysfunction), reinforcement (in which incipient isolation is enhanced by natural selection when populations become sympatric), and natural selection versus genetic drift are all considered in analytical detail. Coyne and Orr argue their positions well. They maintain that polyploidy has been overemphasized, even in plants. Although reinforcement has been widely debated, they conclude that at least it is possible ("Put bluntly, theory said it could not happen until the data said it probably did"). Founder-effect speciation (isolation arises by dramatic genetic divergence in a newly established population—as might occur, for example, when an island is populated by a few individuals from the mainland) is unlikely, as theory has shown, and Coyne and Orr argue that data provide little evidence that various controversial models operate in nature. They see selection as playing a far more important role in speciation than does drift.

Given that "Cold Cape Cod clams, gain their wish, do it! Even lazy jellyfish do it," shouldn't evolutionary biologists have already figured out why almost every multicellular species reproduces sexually? Maybe so, but we're not sure. Why bother making offspring with a partner—and thus sharing only half your genes with your children—when asexual reproduction ensures that babies carry 100 percent of the parent's genes? Niles Eldredge, in Why We Do It (W. W. Norton, $24.95), takes a perspective that encompasses deep time and deep patterns in the fossil record. His answer? Sex prevails because species that engage in it, on average, resist extinction and give rise to new species more often than do their asexually reproducing counterparts. Sexual reproduction is, in effect, a hitchhiker, incidentally carried along to predominance by processes occurring at the species level. The "ultradarwinian" notion that all of nature is the struggle of genes to make copies of themselves is replaced with a more nuanced view: Genes are just chronicles of the rising and falling fortunes of the species in which they are found.

This book, meant for non-specialists and informal almost to a fault (the multiple mentions of "hummingbird" grace), comes as a welcome antidote to the raft of books providing evolutionary rationales for every aspect of human behavior, from philandering to capitalism. Nature, Eldredge reminds us, is organized much like those nested Russian dolls: genes within cells within organisms within populations within species. This hierarchy guarantees that events at one level affect all the others, greatly complicating the search for a single causal layer. We live in an age that wants to privilege genes as the real currency of evolution, but the very history of life argues for a more encompassing view.—R.D.
Should you be worried about the threat of black holes to your continued existence? Have you ever wondered why some birds stand on one leg while they sleep? And what, exactly, is the difference between a level I and a level II multiverse? The answers to these and many other questions can be found in the eclectic and entertaining mix of essays that editor Steven Pinker has crammed into *The Best American Science and Nature Writing 2004* (Houghton Mifflin, $27.50, cloth; $14, paper). Pick up this volume and you'll find a short, witty explanation of how to avoid upsetting an introvert, longer profiles of medical and scientific pioneers, and a great deal more.

To its credit, the collection tackles head-on some of the current cultural and political debates surrounding scientific work. Covering a diverse range of topics, the pieces share a disregard for opinions that, although widely held, do not stand up to close examination. Thus we read that suicide bombers are neither inherently "evil" nor simply the product of poverty and lack of education. Another essay discusses the implications of Iraqi marriage practices for the process of democratic nation building. Multiple articles assess the perceived threats of genetic engineering. It may be impossible to identify a single common theme, but certainly Pinker has succeeded in his quest to find writing that has both style and substance.—A.E.

One example of strong pollinator isolation can be found in *Mimulus lewisi* (left) and its sister species *Mimulus cardinalis* (right). The former has broad pink flowers, recessed anthers and low nectar volume and is pollinated almost exclusively by bees (here by *Bomus vosnesenskii*), whereas *M. cardinalis* has red tubular flowers, protruding anthers and high nectar volume and is pollinated nearly always by hummingbirds (here by *Selasphorus rufus*). From *Speciation*.

A final chapter on macroevolution is rather anticlimactic and very different from the other chapters. As always, the analysis is deep, and the standards for evidence are high, but in this area we know less than we think we do (about such things as the foundation for punctuated equilibrium, for example). The authors accept species-level selection but question whether it is a central issue in establishing evolutionary trends. Although there is very much to admire in this book, as a systematicist I am left unsatisfied. I specialize in biodiversity issues, including the discovery and naming of new species, criteria for recognition of species, and the generation of robust phylogenetic hypotheses about the relationships of those new species with known taxa. Delimiting species and studying speciation are very different activities, but they are related. How to interpret geographic variation remains a perplexing problem, especially when there are breaks in geographic ranges.

In a 25-page appendix, species concepts are catalogued and critiqued. This is a fair-minded, but opinionated, discussion that still leaves some major issues unresolved. I was dissatisfied with the treatment here of evolutionary and phylogenetic species concepts, and with the general lack of a true lineage perspective throughout the book. Many phylogenists and systematists reject the biological species concept, and although Coyne and Orr try to explain the reasons for this deep division between evolutionary biologists, they fail to satisfy me. The division exists because of disagreements over the reality of species and the criteria for their recognition. In order for Coyne and Orr to pursue their research program, species must be real. Yet they ignore the important theoretical contributions of workers such as Michael Ghiselin and Kevin de Queiroz. Although they do cite some of de Queiroz's work, they ignore two of his more recent, influential papers ("The general lineage concept of species, species criteria, and the process of speciation" in *Endless Forms*, edited by D. J. Howard and S. H. Berlocher [Oxford University Press, 1998] and "The general lineage concept of species and the defining properties of the species category" in *Species*, edited by R. A. Wilson [The MIT Press, 1999]). De Queiroz argues that biologists implicitly share a common species concept: that of general lineages that differentiate through time for various reasons. A species under this view is a lineage concept. Other so-called concepts (including even the biological species concept) become criteria for deciding what to recognize as species. Coyne and Orr do not bother with such philosophical issues. Furthermore, the publications they cite for evolutionary and phylogenetic species generally predate the integrative papers of de Queiroz. For example, in rejecting the phylogenetic species concept, Coyne and Orr contrast "thin" (gene trees) versus "fat" (species trees) branches, and choose to focus on such important matters as genetic coalescence. But they ignore the critical role of extinction in delimiting species and in giving the impression of species reality when what has happened is that continuous but geographically variable lineages have been broken up.

I study a controversial "ring species" complex, and my job would be far easier had a little extinction occurred. I suspect that many species are "born" with geographic variation and are never integrated by gene flow—as evidenced by the phylogeographic structure and
high $F_{ST}$ values (indicating greater population divergence) that are characteristic of widely distributed species, especially those with low vagility. One of the lessons of the many allozyme and phylogeographic studies is that genetic variation within species is so great that species-wide gene flow is rare. This finding raises doubts about "speciation"—rather than fragmentation (with differing patterns of adaptation in the fragments)—being the primary cause of biological diversity. For me the central question is not "What are species?" but rather "What do we want them to be?" Coyne and Orr are explicit on this point, and although many will agree with them, many others will disagree.

Setting aside my objections, some of which are admittedly philosophical, I highly recommend Specification. I advise buying the hardback version; although the paperback is well bound, it is heavy and slippery, and this is a book you will want to read and use. Coyne and Orr are to be congratulated on their very well-researched, well-written and integrated, and highly stimulating book.

March Madness

Robert M. Bernero


The accident at Three Mile Island Nuclear Generating Station in Pennsylvania on March 28, 1979, was "the single most important event in the fifty-year history of nuclear power regulation in the United States," says J. Samuel Walker. Many critics of nuclear power point to the accident as a turning point for the industry, noting that no new plants have since been ordered in the United States and that many planned in prior years were subsequently canceled.

Walker provides a gripping, detailed account of the accident and an analysis of its impact and significance in Three Mile Island: A Nuclear Crisis in Historical Perspective. It is his fourth book as the official historian of the U.S. Nuclear Regulatory Commission (NRC). In the preface Walker assures readers that he had complete independence as the author and that the NRC placed no restrictions on what he could say.

However, Walker provides an historical account of the events; he does not evaluate the performance of the NRC during those events.

The first two chapters effectively fill readers in on the historical context for the accident, giving a brief overview of the government-supported growth of commercial nuclear power in the 1960s and 1970s, and describing the emerging controversy during that period over the safety of nuclear power. The public worried both about the risk of accidents and about routine low-level releases of radioactive material. (The latter concern was inspired in large part by fears of cancers caused by exposure to radioactive fallout from nuclear-weapons testing.)

Many people contended that the Atomic Energy Commission (AEC) could not acceptably regulate nuclear power at the same time that it was engaged in promoting it. So as Walker recounts, Congress passed the Energy Reorganization Act of 1974, dividing the AEC into two entities: the NRC, which was charged with regulating commercial nuclear technology, and the Energy Research and Development Administration, which assumed all of the other roles of the AEC and later evolved into the U.S. Department of Energy. Walker provides some interesting descriptions of the last days of the AEC and the selection of members of the new commission.

Reacting to the OPEC oil crisis of 1973, the Nixon and Ford administra-