deed, there are so many different ways of looking at this most important of biological processes that we are all in the position of the blind men in the fable, firmly grasping our own piece of the elephant. Perhaps nobody is capable of seeing the entire elephant any more. We are often reduced to employing metaphors and parables to get our points across. The perils and pleasures of this process are nowhere more apparent than in Dawkins's book.

Dawkins here takes the concept of the selfish gene developed in his 1976 book of that title one step further. The earlier book was written primarily for lay persons, although it caused considerable stir among evolutionary biologists. The new book is more technical and will appeal to (or perhaps annoy) a narrower audience. Dawkins's main argument is twofold: that the gene rather than the organism is the unit we should think of when we consider evolution, and that genes can exert their influence at a distance. The genes of a beaver can actually exert an influence directly on the environment several miles away as a result of the beaver's dam-building activities. Genes of a parasite can force alterations of the host's phenotype and ultimately its genotype that work to the detriment of the host's fitness while enhancing that of the parasite (sorry, I mean the host's genes' fitness, and the fitness of the parasite's genes). Genes confined to or only operating in one sex can greatly influence the behavioral or physiological evolution of the other sex. The examples Dawkins uses are commonplace in the evolutionary literature (and some have come from his own laboratory). Many are presented in the most entertaining terms, reflecting the high spirits of workers in the field. We find organisms falling victim to the Concorde and Ace of Spades fallacies and behaving altruistically because of the armpit effect. Dawkins emphasizes at the beginning that he is simply presenting a new way of looking at these phenomena, a way that may help us to quantify them more easily.

Throughout the book he is charmingly deprecatory about his metaphors, particularly that of the selfish gene, emphasizing again and again that they are only metaphors and that genes are not *really* behaving in an anthropomorphic fashion. This does not prevent him from being a trifle disingenuous about the birth of his most treasured metaphor. In the earlier book he had referred to the organisms that genes control as "robots," a term many people took exception to. He now says (p. 15), "The word robot has other associations, and rigid inflexibility was not the association I was thinking of." Perhaps not, but the term used in the previous book and not mentioned here was actually "gigantic lumbering robots" (p. 21). Metaphors have a way of attracting adjectives the way magnets do iron filings. Perhaps they should be demagnetized occasionally.

Such demagnetization may already be occurring with one important offshoot of the concept of the selfish gene, that of selfish DNA. Orgel, Crick, and Doolittle, in a note subsequent to their original papers dealing with the subject, suggest that a better term for these molecules that have hitched a ride in the nucleus would be "parasitic DNA." Perhaps some categories of DNA could be even more accurately described as commensal or mutualistic. I certainly feel much more comfortable with these terms than with the anthropomorphic "selfish."

Dawkins makes the point that it is often easier to think in terms of the advantage accruing to a gene than in terms of the advantage accruing to an organism. He considers that the gene makes a better unit of selection than the organism because the gene or copies of it may persist for very long periods of time and because different genes within the same organism may often be working at cross purposes with each other. Possibly so, but he has hold of only one part of the elephant (the left ear, perhaps). Thinking in terms of genes rather than organisms is just what population geneticists have been doing for years, and it has gotten them into some terrible jams. Population geneticists periodically nod their heads wisely and say, "Of course, we must never forget that it is *organisms* that are being selected, not genes." Then they go right back to their one-locus, two-allele models just as if they hadn't been listening. This kind of thinking led to such problems as Haldane's dilemma. Haldane showed, using genes rather than organisms, that the process of gene substitution in evolution had to be exquisitely slow, otherwise a terrible genetic load would be imposed on the population. But this problem can largely be made to disappear if organisms are considered as the units of selection, as many other workers subsequently pointed out.

In short, as I think Dawkins would agree, any model of evolution that treats genes or organisms exclusively as the units of selection will be found to be flawed. Indeed, at the end of the book, he resurrects the organism and has some penetrating things to say about why the organism, and especially its ontogeny, is important in evolution. It is exactly this lack of dogmatism about his ideas that ultimately disarms the reader.

Dawkins specifically claims nothing more for the book than that it is a device for stimulating thought about evolution by looking at it in a different way. It certainly does that. Leaving aside his central thesis, in which I think his metaphors tend to run away with themselves, there are some penetrating discussions of standard evolutionary topics. I especially enjoyed the chapter on fitness, one of the slipperiest concepts in evolution, and the discussion of why developmental alterations cannot be passed on to the next generation. This book is an excellent illustration of why the study of evolution is in such an exciting ferment these days.

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The Workings of Ecosystems

Ecology of Coastal Waters. A Systems Approach. K. H. MANN. University of California Press, Berkeley, 1982. x, 322 pp., illus. Cloth, $36; paper, $18. Studies in Ecology, vol. 8.

How do the prolific coastal ecosystems operate to produce their highly visible and valuable products such as kelp, clam, lobster, fish, bird, and seal? Mann suggests that the answer is to be found through elucidating ecological processes (fluxes and cycles of energy and materials) at the ecosystem level of integration, rather than through analyzing species structure at the population or organismic level. Yet Mann contends that many popular systems models are inadequate as quantitative predictors. He points out that most such models are assembled part by part. Ecosystems are often represented by flow diagrams, whose compartments are of functional groups of organisms or energy stores—plants, microbes, dissolved organic matter, detritus, detritivores, herbivores, carnivores, and so on—and whose transfers are of carbon or biomass between compartments. These static diagrams become dynamic simulation models by the addition of the biotic mechanisms of transfer (rate functions) and the abiotic mechanisms of environmental regulation (forcing functions). Compartments are filled and transfers estimated by observing particular organisms and measuring their biomass and metabolic rates. Thus the scheme is reductionist. It describes events at the ecosystem level of integration in terms of events at the organismic level, bypassing events at intermediate levels of community (guild) and population. It assumes that the whole operation runs as the sum of its parts, and so it neglects important system-wide rates governed by complex control mechanisms. Hence Mann sees simulations based on compartmental models as "doomed to failure" as quantitative predictors (although providing "valuable insight") for two reasons: The inherent variability of organisms and their physiologies magnifies statistical confidence limits through the system, and the hierarchical order of the compartments places processes on disparate temporal and spatial scales. Since, for instance, many turnovers of a bacterial population would take but a moment in the life of a higher carnivore, it would be difficult to integrate suitable scales of observation for both.

Herein lurks a potential "catch 22." Mann makes a convincing case that the reductionist approach yields quantitatively unreliable results, yet he does not develop a synthetic approach that would offer a clear alternative. How does one observe a whole ecosystem? Attempts by others to simplify all organisms to particles of different logarithmic size classes or to reduce complete systems to microcosmic analogues have not proved entirely successful. Can one at least observe collective mechanisms and rates of transfer at the population or community level, a step or two up from the organismic level? Mann contends that we need new ecological theory to link cumulative properties of populations with "emergent properties" of the whole.

Perhaps this is why most of the book actually deals with functional groups of species and how they contribute to the workings of various coastal systems: salt marshes, seagrass beds, mangrove swamps, seaweed forests, plankton communities, coral reefs, and benthic communities in sediments. To me, in fact, some of the more interesting accounts were of Mann and his co-workers' own investigations of the demise of kelp forests off Nova Scotia. These had less to do with holistic insight than with relations between species populations: kelps, sea urchins (with alternative food supplies), and their predators. After all, these relations yield a reliable, though qualitative, prediction: Without effective predators, sea urchins can destroy kelp forests. Maybe this kind of forecast is the best we should expect.

A major strength of the book is the recurrence in chapter after chapter of major themes having to do with fundamentally important processes: (i) the existence of alternative pathways of energy flow through ecosystems; (ii) the storage and availability of nitrogen and the need to measure nitrogen as well as carbon fluxes; (iii) the important contribution of detritus (detatched plant matter and associated microbes) to the flow; (iv) the conversion of nitrogen-poor detrital food (plants) to nitrogen-rich detrital food (microbes); (v) the recycling of dissolved organic matter produced by plants; (vi) the importance of water movement for plant growth and for transport of nutrients and ecosystem products; (vii) the significance of form and surface-to-volume ratios of plants; and (viii) the overriding influence of weather in shaping ecosystem function. Thus systems ecology complements fisheries biology (p. 259): "Nature cannot be confined to our mathematical representations of recruitment, growth and survival."

Although his references are selective and rarely postdate 1979, Mann covers a broad topic with enthusiasm and verve. He is interested in processes, not species lists, and he offers a rationale for this emphasis at the outset. Professionals and graduate students will benefit greatly from this concrete approach to understanding coastal systems, although parts of the work may be a bit advanced for all but the more enlightened upper-division undergraduate.

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Andesite Genesis

Orogenic Andesites and Plate Tectonics. JAMES B. GILL. Springer-Verlag, New York, 1981. xiv, 392 pp., illus. $38.50. Minerals and Rocks, 16.

This is an excellent book for earth scientists and students. It is in the same class as *Origin of Granite . . .* by Tuttle and Bowen and *Generation of Basaltic Magma* by Yoder, which deal with the other principal igneous rocks on earth.

It is no accident that a book on andesites should appear later than its counterparts on granites and basalts. Granites make up much of the continental crust. Because they occur in a region of low pressure amenable to both observation and easy experimentation, their origins in the crust were worked out first. Basalts cover the sea floor. Experimentation and analysis during the '60s and '70s confirmed that most physical and chemical attributes of basalts originate below the crust. Andesites occupy the middle ground. Compositionally they are between basalt and rhyolite (or granite), and ideas about their origins have included aspects of ideas about both the crustal origins of granites and the mantle origins of basalts. Geographically andesites are between land and sea, located at the boundaries of convergent lithospheric plates, and ideas about their origin also postulate an essential role for plate subduction.

Like Tuttle and Bowen's and Yoder's books, Gill's is critical, comprehensive, and systematically organized. In the first chapter, Gill adopts a compositional definition of orogenic andesite that few would argue with: andesites are hypersthene-normative volcanic rocks characterized by 53 to 63 weight percent of SiO_2, and orogenic andesites are andesites with certain limited concentrations of K_2O and TiO_2. The familiar modifiers tholeiitic and calc-alkaline are retained by Gill to further characterize andesites.

The rest of the book comprises three parts: physical features (chapters 2 through 4, pp. 25–96); chemical features (chapters 5 through 7, pp. 97–229, and appendix, pp. 317–336); and evaluation of ideas (chapters 8 through 12, pp. 230–315). In my judgment Gill has chosen the best order and has achieved an excellent balance, one that reflects well the extent of knowledge and agreement. Related subsections are referred to extensively.

I praise in particular the factual summaries, which include numerous original figures compiling such information as distance from volcanoes to plate boundary, height of volcano above the seismic zone, dip of seismic zone, and rate of duration of plate convergence and crustal thickness for 34 volcanic arcs; heat flow and seismic velocity structure; observed and estimated temperatures, densities, and viscosities; elemental and isotopic compositions of rocks and minerals in space and time; and observational and experimental determinations of mineral-melt equilibria. The appendix summarizes the available data on the chemical composition of active volcanoes at convergent plate boundaries and notes 159 references to the sources of data for 317 separate volcanoes, located in 22 figures.

Gill's review of ideas, ancient and modern, is frank, cautious, and skeptical. He concludes that the traditional