particularly difficult to indicate what sort of characters are likely to be taxonomically valuable. New ones are being discovered all the time, and the only real criterion of a "good" character is that it leads to good results. The proof of the pudding is always in the eating.

Chapter 10, which deals with the procedure of classifying, is especially concerned with recognition and grouping of the higher taxa. It includes a discussion of Hennig's views. Mayr replaces Hennig's terms "plesiomorph" and "apomorph" by "ancestral" and "derived" as being more neutral and more self-explanatory. He does not follow Hennig in putting much more weight on the point where groups diverge than on the amount of change that has occurred since divergence. Thus he is happy to treat the birds as a class and to put the crocodiles from which they diverged with the reptiles. He also has a useful discussion of numerical taxonomy, which has become a bandwagon for taxonomists who cannot afford the Rolls Royce of molecular biology. His views, described very succinctly, are pessimistic about the value of the numerical work done so far but not altogether so about the future. The last chapter in this section deals, in a very practical way, with the publication of results and will be very useful to students.

Finally, the volume concludes with two chapters describing the code of zoological nomenclature and how to interpret it. This is essentially a dismal subject. All one can say in mitigation is that if we had no rules we should probably be worse off. It seems impossible for human beings to agree on these topics, and for that reason some code to which we can all refer appears to be essential. Mayr gives a brief history of the subject, sets out the whole code, and comments on a large number of points on which opinions often differ. These chapters also are very useful for a working taxonomist, though they will not attract workers in other branches of zoology.

It will be seen that I regard this book as a major contribution to general zoology, of special value not only to working taxonomists but also to the many zoologists with other interests who want to find out what the names of the animals they study really mean.

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## Ordering the Phenomena of Ecology

**Evolution in Changing Environments.** Some Theoretical Explorations. RICHARD LEVINS. Princeton University Press, Princeton, N.J., 1968. x + 120 pp., illus. $6.50. Monographs in Population Biology, No. 2.

How much does the evolved complexity of life owe to its physical setting? Cracked, contorted continents; tidal oceans; a blanket of capricious vapors—were these the necessary cradle for life? Are they a continuing incentive? Is it perhaps no mere coincidence that Darwin found the physical grandeur of scenery as much an inspiration as its living mantle? In his old age Darwin wrote of his regret that age had diminished his once intense love of landscape; it seems that the mind which first saw clearly the process of evolution was a mind unusually laden with memories of rivers, islands, deserts, and mountains. These distilled, evidently, into the habitats and barriers of his theory. Since his time our estimate of the importance of the physical pattern has certainly not decreased, but many questions remain. Is physical heterogeneity as important for macroevolution as it is for speciation? Should dolphins look back to the birth of their intelligence on land as we should look back, perhaps, to the birth of ours in the canopy of the forest? Concerning that forest itself, did its mazy richness originate in the plains where it now stands or in the more broken and physically tougher world of bordering mountains and savannas?

These are some of the thoughts stimulated by reading *Evolution in Changing Environments*. They seem rather effusive for the severe pages of *Science*, but I record them as showing the breadth of relevance of this small book and as associations hovering indistinctly behind the seemingly dry concepts and technical problems of evolutionary theory which are discussed within. This is a very theoretical essay which pauses only briefly to point to possible illustrations of the new concepts or to data which may bear out predictions from the theory. The subtitle is "Some Theoretical Explorations," and the reader must be warned to expect more of "adaptive landscapes," "gene pools," and "pure white noise" than of mountains, lakes, and birdsong. I feel that the book could well have been less dry. For example, so little detail is given of the living complexity of real examples of a community that one is led to wonder what the author's idea of one actually is. From the book one might gain the impression that the animal communities of Caribbean islands consist almost entirely of species of *Drosophila*, which are sometimes eaten by lizards and a polymorphic spider—the last, in one table, floating in on an archipelago of quaint-named islands. However, this point is in passing; the book could have gained in more serious respects by being longer. First let me say what I found good.

It is stimulating and provocative. It begins with a persuasive defense of theoretical biology and with a particular plea for a theory which tries to reduce the manifold phenomena of ecology to some sort of order without waiting for the nearly impossible preliminary of descriptive precision in physical and chemical terms. The models of the book are to sacrifice "precision for generality and realism" —meaning especially the realism of environments that are spatially diverse, changing, and themselves partly made up of living systems. The author is more concerned with the general shape of phenomena as described in mathematical terms than with quantitative predictions. At the same time he sets out to discover formulas that will summarize the data of population and community ecology in much the same way that sample mean and variance summarize a variate. Quantities of this kind he calls "sufficient parameters," and they are to facilitate meaningful description and comparison of the entities to which they refer: species, communities, genetic systems, and so on.

This approach seems to me very promising, and the first of the new conceptional tools which Levins presents—the idea of the fitness set and adaptive function—raised my expectations high. This tool cuts a fair chip from the problem of his title, and will provide a useful way of visualizing an important problem. Already I see certain situations in the "classical" theory for unvarying environments in a new light. To give an example of its application, the idea gives us a basis for thought about a very diverse set of developmental polymorphisms differentiating cells, castes of social insects, migrant versus nonmigrant behavior in birds, immediate germination versus dormancy in seeds, and so on. Fitness set analysis, as the author calls it, is

perhaps shown to be less than necessary for an understanding of such phenomena by the fact that Dan Cohen has been able to give a more precise theoretical framework for migration and dormancy problems without reference to Levins's work; nevertheless, for me, Levins's method has the "generality and realism" which he claims, and an immediate appeal to intuition.

I am not so happy about all the other applications of fitness set analysis outlined in the book. Evidently it can be applied to some simple genetic polymorphisms but to meet realistic situations will certainly require elaboration both of the concept of fitness and of the adaptive function. This criticism amounts to restating what I think is the admission of most evolutionists, that we do not yet know what natural selection maximizes. Levins gives the impression that it is certainly some sort of mean of individual Darwinian fitness. With his chosen examples, mainly concerned with adaptation to the physical environment and to competing species, this seems reasonable, but I think he would have met with grave difficulties if he had tried to carry the idea into the field of social adaptation, or even to apply it to such a minimally social character as the production of an unequal sex ratio. I am doubtful about his application to tendency to inbreed, and in another particular application (mentioned below) I think his claims are definitely erroneous.

There may be other ideas as important as the fitness set analysis in relation to community ecology. I am less qualified to judge and have read the relevant sections less carefully. I feel that a great deal of complexity that Levins does not care to face has been rather lightly brushed aside. One may ask, for example, what is the value of a "proof that the number of species cannot exceed the number of resources" without any discussion of the fact that each new species provides new resources for many others—for predators, parasites, scavengers, and so on. In whole communities in any but the most inhospitable environments the number of species does exceed the number of physical and chemical resources of the bare environment, usually by a large factor. However, such obvious objections apart, there are some interesting predictions which if upheld may prove the worth of Levins's simplistic optimism in this field also.

The importance of the problems attacked and Levins's evident facility with mathematics at first made me hope that this book might prove a kind of sequel to Fisher's *Genetical Theory of Natural Selection*, broadening Neo-Darwinism into a more complex and realistic ecological setting. I regret to say that this hope dwindled as I read. Misgivings began when I found that the author spent more time in explaining what he meant by a "cluster of theories" than in explaining what he meant by "fitness." Too many new terms are created and then used with too little care and qualification. Other terms are brought in on the basis of distant and unfocused analogies (for example, "memory" and "noise"). The book acquires a pretentious air: it seems committed to being a cornerstone of some new imposing structure. So was Fisher's book pretentious, but when his statements drove me into arduous asides I almost always had to admit in the end that his assumptions were reasonable, his prose at least sufficient, and his algebra correct, whereas I cannot say the same for my attempts to follow Levins's similarly dense arguments. The book is careless at all levels of its composition. There are too many misprints; there are too many unexplained steps into approximate formulas; and there is a general lack of precision as to what assumptions are being made. The diagrams are very carelessly done and often have little relation to the model in the text which they are supposed to illustrate. This is a great pity, for concepts like that of the fitness set should have great appeal to those who (like myself) feel a need for graphical aids in mathematical problems. As one example, Levins points out that his "adaptive function" at one extreme must have a hyperbola-like form asymptotic to both axes, but the reader who does not catch this point the first time cannot hope to be reminded of it by the diagrams, since all seven that illustrate the case in question show the functions curving away from an axis. Elsewhere a diagram (fig. 2,7a) indicates heterosis in each of two environments whereas the caption states that polymorphism would not be maintained in either environment occurring alone; another (fig. 3,4) shows arrows leaning away from lines which, the caption says, they should always lean toward, and also curved lines which the model indicates should be straight.

Less talented readers have to put up with the seven-league-boots of a mathematical writer, assuming they care to follow him at all. It is another matter when he plays with a magic cloak as he goes along. This is the effect I receive from most of Levins's algebra. Instead of taking the opportunity to dilate from the necessary terseness of his scientific papers he has contracted his account still further, and in doing so seems to have lost sight of some of his original qualifications. Difficulties and sometimes terms disappear, and symbols change their meaning in mid-argument. The $\theta$ which on page 45 means the probability of accepting a less favorable environment on page 46 is only interpretable as the probability of accepting a particular environment whether it is the favorable one or not. On pages 51 and 52 his jump into an unexplained "good approximation" seems to imply that he thinks that if $a$ individuals of one type and $b$ of another are wandering at random in an enclosure the probabilities of the three types of encounter are proportional to $a^2$, $ab$, and $b^2$. However, the wording is so vague that, as when bad handwriting combines with bad spelling, it is hard to know what one is trying to check. I left the section on community ecology with the feeling that his "sufficient parameters" may have some meaning of the kinds the author supposes but that they are probably by no means the best available. I do not understand in what sense they are supposed to be "sufficient."

A similar passage of inadequate explanation seems to lead into a definite error about genetic polymorphism, and this result is repeatedly referred to. Levins claims to demonstrate that when an environment is patchy a genetic polymorphism may be maintained even though there is no kind of heterosis. This seemed to contradict the findings of classical selection theory which had defined fitness as averages taken over all environments with their frequencies of occurrence. On referring to Levins's original paper I found a somewhat clearer presentation in which the idea of nonheterotic equilibria was worked out in the context of *a single environment varying in time*. This might be considered a kind of temporal "patchiness," but it is by no means obvious that it affects selection in the same way as contemporaneous spatial patches. Indeed I believe it does not and that something has been glossed over in the new presentation of the idea of a multiplicative adaptive function (p. 18). It

is claimed that the generalization to spatial "coarse grained" patchiness is supported in an earlier note by Howard Levene (*Amer. Natur.* **87**, 33 [1953]), and in a paper by Levins and MacArthur (*ibid.* **100**, 585 [1966]) which extended Levene's approach. But Levene worked from carefully stated assumptions that imply (although he did not emphasize this) that survival in the different patches observes an extreme density dependence. A fixed quota were supposed to survive in each particular patch irrespective of whether the initial population contained genotypes mostly well or badly adapted to that patch. Yet Levins and MacArthur state that the fitness of each genotype does not depend on population density; and hence apparently came Levin's present view that under "coarse-grained" selection, whether temporal *or* spatial, "a concave fitness set can still permit a mixed strategy polymorphism provided the concavity is not too extreme." To make the classical argument for a spatially patchy environment *not* work, and so to permit such equilibria, the assumption of severe density dependence must be brought in, and this makes the selective advantages of genotypes take a mainly competitive interpretation. This in turn suggests behavioral adaptation in a highly social species, to which the simple maximization criteria are unlikely to apply. In short I can see no very realistic situation to which Levene's model, and the developments from it, could apply.

This criticism perhaps advises caution in accepting some of the other too-briefly explained ideas. It is the most definite I have to make, and having mentioned it I must close what is already a longish review for a very short book. If the author had written more at length, and with more care, I think he would have done more justice to his bold design and the importance of his subject.

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## Known and Little-Known Pollutants

**Biological Effects of Pesticides in Mammalian Systems.** A conference, New York, May 1967. HERMAN F. KRAYBILL, Ed. New York Academy of Sciences, New York, 1969. Illus. Paper, $23. Annals of the New York Academy of Sciences, vol. 160, art. 1, pp. 1-422.

**Chemical Fallout.** Current Research on Persistent Pesticides. Proceedings of a Rochester Conference on Toxicity, Rochester, N.Y. MORTON W. MILLER and GEORGE G. BERG, Eds. Thomas, Springfield, Ill., 1969. xxii + 532 pp., illus. $22.50.

*Biological Effects of Pesticides in Mammalian Systems* is a series of 35 papers, together with four discussions and seven summations, that made up a conference held by the New York Academy on 2–5 May 1967. The seven main sections of this monograph cover general pesticide use, analytical instrumentation and methodology, metabolism of pesticides, and effects along biochemical, pathological, toxicological, and physiological lines. The bulk of the volume consists of review papers, with research reports occupying much of the last quarter. The organizers of the conference seem to have succeeded reasonably well in presenting in a single monograph the remarkable progress in epidemiologic and toxicologic research on chemical pesticides. The result is inevitably somewhat dated, and its utility is impaired by inadequate summaries and the lack of an index. This is not a book for those who read only abstracts, however. It contains four papers that together constitute the best summary we have seen of pesticide decomposition in the environment, a convenient compilation of residues in the atmosphere, a tight review of residues in wild animals, a cogent analysis of residues in American foods, and a good summary of residues in man. About 1200 references are cited, and an introductory chapter on the chemistry of pesticides will help the uninitiated.

The strength and weakness of this symposium are to some extent the result of its ambitious attempt to cover an enormous field that is now well fragmented into highly specialized research categories. Research on the effects of DDT and DDE has, of course, been highlighted by important breakthroughs since the New York conference took place, but most of the older data on these compounds are still solid, and the present volume also covers organophosphorous insecticides, fungicides, herbicides, and a fascinating array of sublethal and little-understood biochemical effects. In a thoughtful summary of the conference, M. B. Shimkin notes that the chronic effects of these chemicals on man are yet to be defined, that the release of pesticides for individual use would appear to require the same specific safety procedures we now impose on pharmaceuticals, and that research in preclinical toxicology and pharmacology still needs to be emphasized.

*Chemical Fallout* reproduces 25 papers and the discussion at a University of Rochester conference on toxicity held 4–6 June 1968. Although essentially slanted toward the primary goal of public health, this meeting paid considerable attention to effects on natural populations, and it involved more ecologists than did the New York Academy symposium. The results are also more helpfully put together by the editors: each paper has its conventional abstract, and the volume closes with an author and a subject index.

Except for overlapping papers on steroid metabolism, these two volumes do not duplicate each other. *Chemical Fallout* splurges on mercury with six papers from Sweden and one from Japan. Here for the first time in English are good descriptions of widespread mercury pollution that will surely stimulate research and monitoring of this chemical in North America. (An important summary of alkyl mercury poisoning in terrestrial Swedish wildlife have more recently appeared in *Viltrevy* **6**, 301–78 [1969]). According to Westöö, sale of fish from about 40 water areas in Sweden is now prohibited as the result of methylmercury contamination. In marine species caught commercially near the shore, mercury runs as high as 10 milligrams per kilogram. Aquatic pollution by mercury is reported to result from slimicides used in the paper-pulp industry and from mercury electrodes in chlorine-alkali factories.

These two volumes happily complement each other in a variety of ways, although one wishes that fish populations could have received more attention. Together, these books impress one with our present lack of information on worldwide or even continental patterns of chemical fallout. In the Academy monograph, Jegier's interesting paper cites only 24 references on pesticides in the atmosphere and emphasizes the extreme difficulty of quantifying residues in the air. In the Rochester book, Risebrough tackles the fallout phenomenon indirectly by addressing his attention to marine ecosystems. He