REVIEWS

Morphological differentiation and adaptation in the Galápagos finches.—Robert I. Bowman. 1961. University of California Publications in Zoölogy, vol. 58, vii + 326 pp., 22 pls., 74 text-figs., 63 tables.—In spite of Dr. David Lack's excellent study (Cambridge University Press, x + 208 pp., 1957) of Darwin's finches (the Geospizinae of the Fringillidae of Wetmore), a great gap existed in our understanding of this group in the lack of a careful investigation of the skull and jaw apparatus of its members. Bowman has filled part of this gap with his extensive study of the feeding habits and cranial morphology of these birds. The primary goal of his study "is a new attempt to explain some of the structural variations in the Galapagos finches as adaptations to food getting," the discussion being based upon a strong foundation of excellent descriptions of the feeding habits, jaw muscles, and skull structure. Indeed, the sections on feeding habits and the jaw muscles can serve as models for future studies. The only obvious error concerns the M. pterygoideus (see especially p. 123) where Bowman attempts to correct W. J. Beecher's (*Auk*, 70: 270–333, 1953) identifications of the subdivisions of this muscle but is incorrect himself (Bowman's treatment of the jaw muscles will be reviewed more fully elsewhere, Bock, MS.). A special word should be said about the illustrations. Few papers have appeared in recent years that can compare to this one in excellence and abundance of illustrations. The careful execution of the figures and the precise agreement between them and the text set a high standard that all workers in avian anatomy might endeavor to reach. Although the descriptions form the necessary foundation for this study, the discussions of function, the methods of comparison, and the theoretical considerations of evolutionary principles are of far greater importance to an evaluation of the general conclusions; hence inquiry into these parts will comprise the bulk of this review.

Bowman gives a brief description of the kinetics of the avian skull together with the actions of the jaw muscles in opening and closing the jaws (Table 35). These muscle functions would be better expressed as probable functions and not as definite facts as implied. Bowman has deduced the functions from the structural configurations and while these deductions are good, more detailed observations are needed to prove the actual action of the muscles. The separation of the jaw muscles into functional units (p. 126) is an excellent forward step; however, the functional units defined—depression of the upper jaw, elevation of the mandible, and so forth—could be further refined as these units are quite inclusive. Some important aspects of bone–muscle systems are omitted from consideration. No mention of ligaments is made in the analysis, yet the ligaments are absolutely essential for a proper understanding of the kinetic feature of the skull and of the exact action of some jaw muscles, as for example, the M. depressor mandibulae. The timing of contraction of the individual muscles during the cycle of opening and closing the jaws was not considered. Probably the adductor muscles, for example, do not act simultaneously and continuously during closing of the jaws. Most likely, the M. pseudotemporalis superficialis contracts first when the mandible is greatly depressed, and the anterior parts of the M. adductor mandibulae externus act when the jaws are almost closed. Hence, use of the term "adducting" (p. 128) is too broad; the precise function of the individual muscles varies. Some appear to be "speed" muscles and others to be "power" muscles. Thus the factors governing their size, arrangement of fibers, and attachments would be different. Bowman assumes that the factors controlling relative size and other aspects of the adductor muscles are the same in all cases, which appears to be too simple a solution.

In his analysis of the functional significance of the differences in the jaw muscles between species of geospizines (pp. 128–135), Bowman relies solely upon "index values" for the mass and hence power of the muscles; these were developed because the muscles are too small to be dissected out and measured directly. Yet it is not shown that these indices are closely correlated with the size and/or strength of the muscles. Further, it is questionable whether these indices are suitable for the purposes for which they were used. The comparison of species by ranking them according to their index values provides no real indication of the size of the differences between any two species, be they in juxtaposition or separated by several species in the ranking order. Moreover, mere size is perhaps the least important of the several factors influencing the force exerted by a muscle on the skeleton. Of greater significance are the angle at which the muscle attaches to the bone (change in this angle during contraction must not be forgotten), the length of the lever arms of the entire system, and the internal arrangement of the muscle fibers within the muscle—the degree of pinnateness. No mention of these factors was made by Bowman, and as they influence the strength of the muscle more than would the mass of the muscle, their omission may well nullify any conclusions reached on the function and comparative adaptation of the jaw muscles.

Bowman correlated the structure and function of the jaw muscles with the feeding habits in each species, with the hope of ascertaining the adaptive significance of the skull and the jaw muscles. Unfortunately, in spite of the great detail in the analysis of the feeding habits, this information is not adequate for determining the functional and adaptive significances of the cranial morphology. The problem arises because of the great discrepancy between the preciseness of the variables being correlated. Bowman wished to correlate his very precise morphological data with his information on feeding methods and stomach content analyses to obtain very exact conclusions about the various adaptations for feeding. Bowman reached highly exact conclusions on the morphological adaptations, yet a correlation can be no better than the most imprecisely measured variable, which in this case is the feeding methods. It is not enough, for the desired conclusions, to know only the hardness of the seed. One must also know the exact movements of the jaws while the seed is being cracked, the exact action of the muscles, where the seed is held between the jaws and exactly how the seed is oriented in the jaws. These questions have never been investigated, to my knowledge, yet they seem essential to the problem. Bowman was able to establish that a close correlation exists between the size of the bill and the hardness of the seed cracked by the bird. This conclusion substantiates Lack's earlier statement that the size of the bill in congeneric species of geospizines (e.g., *Geospiza*) is correlated with the size of the seeds eaten. Lack used the size of the seed as an index to the strength needed by a bird to crack it and correlated this index with the size of the bill. Bowman showed that the hardness of the seed coat, not only the size, must also be considered. Although Bowman argues at great length against Lack's conclusion (p. 71), his conclusions are essentially the same as Lack's and reached on largely the same type of reasoning.

From his functional analysis of the jaw muscles, Bowman reached one conclusion that I should like to quote in full (pp. 134–135): "In view of the magnitude of the differences in relative size and position of the adductor muscles between closely related species of *Geospiza* and *Camarhynchus*, it would seem that the suggestion made by Lack (1947: 63–64) attributing the differences in the bills of these species primarily to their taking foods of different size, is not substantiated by the myological evidence presented here. Rather, these differences in musculature reflect differences in adducting potentiality, which may be better correlated with differences in feeding habits and availability of food, as well as in morphology of bill and skull." Bowman's functional analysis does not support his conclusion, but more importantly, I fail to see any real difference between Lack's conclusion as given in the first of these two sentences and Bowman's conclusion given in the second—they say the same thing in different ways. Rather Bowman's evidence seems to confirm and refine Lack's earlier conclusion—it does not reject Lack's results.

In his functional analysis of the skull, Bowman concentrated on the curvature of the bill and on the pneumatization of the cranial roof. As extensive discussion of the curvature of the bill will be presented elsewhere (Bock, in press), suffice it to say that Bowman's analysis and conclusions appear to me to be incorrect. Although I have not studied the section on cranial pneumatization in great detail, the arguments and conclusions do not seem convincing.

The functional aspects of the skulls of the genera and species of geospizines are compared in great detail (pp. 217–251), the basic comparison being between *Geospiza magnirostris*, representing the extreme seed-cracking form, and *Certhidea olivacea*, representing the extreme insect-eater. The major differences in structure between these species are listed and their functional significances discussed. This method is excellent and indeed the one that will provide the most preliminary information, but it has several inherent shortcomings and difficulties, some not avoided by Bowman. The greatest problem is that a comparison of two extreme forms does not prove that the observed differences in structure between them are all correlated with the difference in their feeding habits. One cannot assume, on the basis of this comparison, that the observed structures in both forms are direct and different adaptations to their feeding habits. Bowman makes this assumption tacitly, and throughout assumes that when a structure in any species is more like that in *G. magnirostris*, it is an adaptation for seed-cracking, and, when more like that in *Certhidea*, is an adaptation for insect-eating. Because most geospizines take both animal and vegetable foods, some degree of structural compromise permitting the taking of both sorts of food must exist. Because the skull–jaw muscle system is so complex, comprised of many individual parts which can evolve semi-independently (i.e., mosaic evolution), the same functional goal may be reached in several ways. Each of these multiple pathways leading to the same functional goal is adaptive, but the morphological differences between them are nonadaptive in terms of that particular function. It is possible that, during the adaptive radiation of the geospizines, several adaptive pathways were utilized by the different genera and species while acquiring the necessary adaptations for seed and insect eating, and hence the observed morphological differences in the present-day species cannot all be explained directly on the basis of feeding habits. There is no reason to expect only one anatomical–functional trend in the cranial characters from the extreme seed-eating condition to the extreme insect-eating condition. The ranking of the 20 cranial characters in Table 60 indicates this problem clearly, yet there is no discussion of this most interesting topic. The comparisons of the different pairs of geospizines (pp. 238–251) are ingenious but difficult to evaluate because of the above-mentioned problems in the functional analysis of some cranial features, and because almost all of the comparisons are very subjective in spite of the careful measurements reported. It is impossible to check the comparisons against the tables of raw data, Tables 49–59, and with the ranking of the species in Table 60, without spending inordinate lengths of time. Bowman's comparisons are, in reality, qualitative, not quantitative as he claims and attempts to support by reporting (but not using) huge amounts of detailed, exact measurements.

It seems to me that the sections of this paper dealing with function are the weakest parts. I think that Bowman has not succeeded in establishing a correlation between the cranial features and the development of the jaw muscles, and in showing how these morphological characters are associated with the food and feeding habits of the individual species.

Bowman (pp. 136–141, 155–156) discusses the problem of the generic groupings within the Geospizinae, based upon his analysis of the curvature of the bill, and states (p. 156): "It is here concluded that the genera of Geospizinae as defined by Swarth (1931) mainly on the character of the bill, do represent distinct natural assemblages, for the reason that they show significant adaptational divergences." Bowman's demonstration of the adaptive divergences of Swarth's genera is based on his functional analysis of the bill-curvature, which is open to serious doubt. But this is the least of the problem. The main issue of generic definition is one of delimitation, not grouping. Lack simply defined the genus as a broader adaptive group as he recognized all of Swarth's genera as subgenera; both workers acknowledged the same natural groups. Other workers consider every species distinct enough for generic recognition, yet every species is not a genus! Most species groups represent distinct natural assemblages, but are not necessarily good genera. The only pertinent question is: are the groupings recognized as full genera by Swarth more meaningful than those of Lack? I think that Bowman's data do not permit a better answer to this question than those reached by earlier workers and would recommend that this problem remain closed until more relevant evidence is available.

A major reason for Bowman's challenge of Lack's conclusions is that Bowman embraces wholeheartedly the opinion of Andrewartha and Birch (*The distribution and abundance of animals*. Chicago, Univ. Chicago Press, 1954. Pp. 462–463.) that competition does not occur between animals, past or present, and that the ecological differences between sympatric species, even closely related ones, evolved fully while these forms were still allopatric as a result of each form becoming adapted to its environmental conditions. Careful reading of Andrewartha and Birch, and of Bowman, reveal that these authors have misquoted Lack and set up a "straw-man" to demolish. Andrewartha and Birch (*op. cit.*, p. 462) claim, without exact citation, that Lack said: "The only reasonable hypothesis is that these habitat differences are brought about by competition between species." Bowman's discussion is worded so as to give the impression that Lack claimed competition was responsible both for the origin and the complete evolution of the adaptive differences between sympatric species of geospizines. These statements attributed to Lack are simply not true. Lack always said very carefully that newly evolved species are rarely, if ever, identical in their adaptations at the time they reinvade each other's ranges. These differing adaptations evolved in response to the different environmental conditions of their geographically separated ranges. Competition simply reinforces these differences until the species no longer interact. Lack never claimed that competition was responsible for the origin of these differences. Birch (*Amer. Nat.*, 91: 5–18, 1957) rediscussed the entire problem, and concluded that competition can be applied where a shortage exists in the common ecological needs of two or more sympatric species—Lack's exact definition. Hence I would conclude that no essential difference exists between Lack and Birch on the role of competition in the ecological adaptation of two newly sympatric species.

Bowman, in agreeing with Andrewartha and Birch's earlier opinion, does not present any convincing arguments supporting his stand. He writes (p. 274): "The biological advantages of maintaining the bird population considerably below the maximum level that the food resources of an area can support at any given time are obvious," but does not offer any mechanism by which populations maintain this level. Then he says (p. 273): "Competition for food implies that there is an active or passive 'struggle' on the part of one species to gain certain foods sought by another species. . . . ," suggesting a belief in a Victorian concept of "fang and claw" selection. He claims that the ecological conditions of the various islands are sufficiently different to allow the observed anatomical differences between now sympatric species to evolve by adaptation to the local ecological conditions while these species were still allopatric, yet he never gives sufficient data to show the exact differences in the ecological conditions of the individual islands, and he does not correlate the observed morphological adaptations between the allopatric populations of a single species with the exact ecological conditions of their separate ranges. Instead he talks about vague differences in flora and how these account for the present-day variation in feeding adaptations (pp. 295–296). Bowman's comparison of the floras of the individual islands (pp. 11–18) is based upon species composition and suggests only that the concept of geographically variable polytypic species has not yet been extensively applied to the flora of the Galápagos. While it is perhaps interesting to learn that only a small percentage of the total flora of two islands is common to both, it would have been more useful had Bowman indicated the number of genera and species groups found on each island, rather than the species, and then determined the percentage shared by sets of islands. It seems likely that, so far as the finches are concerned, only a certain plant genus, or species group, need be present, such as *Opuntia*, *Cereus*, *Pisonia*, *Psidium*, *Miconia*, etc., with the exact species making little difference. Similarly, the exact composition of the total flora may make little difference to the finches; their distribution and their particular adaptation may depend upon a small number of plant species. This possibility was not explored by Bowman. Lastly, Bowman did not really determine the most important fact, to wit, whether there was a shortage of seeds of certain sizes. His data, which come from only a few years' observation, cannot prove or disprove possible shortages (see p. 274).

Summing up, I think that Lack's conclusion that the observed differences in the feeding adaptations between sympatric species of geospizines are the result of competition reinforcing those differences already present when the species came into contact is correct. The arguments of Andrewartha and Birch against competition are inconclusive and the facts given by Bowman to prove that specific cases in the geospizines could not have resulted from competition are insufficient. There is, thus, little basis for Bowman's sweeping assertion (p. 275): "The anatomical differences between closely related species of *Geospiza* living in the same locality may be thought of as biological adjustments (adaptations) that prevent these species from competing with each other. The mechanisms by which these adjustments [to prevent competition] have evolved is unknown."

In a final chapter on "Adaptive radiation in the Geospizinae" Bowman tabulates (pp. 287–289) the general and special adaptations for each genus and species, giving a clear picture of the range and types of adaptations within the group. It is to be regretted that this chapter lacks discussion, and omits consideration of the evolutionary factors responsible for adaptive radiation in general and for the Geospizinae in particular. I fail to understand Bowman's reluctance to speculate upon a possible ancestral form for this group (p. 287), when the evidence points so overwhelmingly to some member of the Fringillidae of Wetmore. Yet, Bowman states in his summary (p. 295) that the geospizines and the emberizines (of the Fringillidae) may have been derived from a common ancestor. There can be little doubt that the ancestral geospizine was a finch, probably one that fed on both seeds and insects. Since more and more evidence points to such finches as *Melanospiza* and *Tiaris* as possible ancestors of the Geospizinae, a frank discussion of this problem is more reasonable than evading it.

A major shortcoming of this section on adaptive radiation results from Bowman's failure to separate clearly his conclusions on the adaptations of the genera and species from his conclusions on the evolution and classification of the subfamily. When stating that certain characters of the skull, for example, show close adaptations to certain environmental factors, he does not go on to discuss the possibility of polyphyletic origin of these characters within the geospizines. The reluctance to seek possible ancestral groups for the Geospizinae prevents delving into the possible evolution and hence into the course of adaptive radiation of this group. His summary of adaptations (pp. 287–289) merely lists the broad ecological properties of each genus and species, but does not touch upon the problem of the origin of the ecological relationships and of the morphological adaptations to them. With this absence of any real discussion on the adaptive radiation of the Geospizinae, this study lacks a central theme about which the great mass of reported facts and deductions can be arranged. It is, therefore, my opinion that the main objective of this study (p. 1)—"to explain some of the structural variations in the Galápagos finches as adaptations to food getting"—has not been achieved.

The general reactions I have after studying this paper are certainly mixed. On one hand, Bowman has presented a wealth of new data about the Geospizinae, but on the other he fails to convince me that his interpretations of the feeding adaptations and of the adaptive radiation undergone by this group represent an advance over the ideas presented by Lack in *Darwin's finches.* Because Lack and Bowman have studied the same group and have attempted to solve the same problem, most workers will automatically compare these works; hence it seems proper to offer a few comparative remarks. The investigations are quite different in approach. Lack was more concerned with general principles and frequently included examples from other groups of birds, while Bowman emphasizes detailed problems within the Geospizinae. Although Bowman's investigation is the more ambitious and includes far more detail, Lack presents a more complete picture of adaptive radiation in the Geospizinae. Where Bowman differs most widely from Lack, his evidence and functional analyses are not sufficient to support his conclusions; these sections of Bowman's study include the shape of the bill, generic limits, and the role of competition. In other parts of his study, on the jaw muscles, food and feeding habits, and the structure of the skull, Bowman's evidence supports Lack's earlier results although Bowman reaches, at times, different conclusions from his more extensive data. It is with a feeling of real regret that I present these opinions, because of the great amount of work done by Bowman in this investigation. Yet I see no alternative. The gathering and quantifying of the great mass of detailed data as done by Bowman for the Geospizinae is most desirable, but unless backed by a critical understanding of the subject matter, no amount of detail will lead to sound generalizations.—WALTER J. BOCK.

Animal dispersion in relation to social behaviour.—V. C. Wynne-Edwards. Edinburgh & London, Oliver & Boyd, 1961. xi + 653 pp. 55 shillings ($7.93).—The central theme of Wynne-Edwards' book is that social behavior is the basis for numerical homeostasis of natural populations of animals. Social organization originally evolved to provide the feedback for the homeostatic machine (p. 14). A determined effort is made to relate all self-regulation of populations to social behavior and develop a new theory (not hypothesis) of "dispersion through conventional behaviour" that identifies a possible common purpose of sociality for the first time (pp. 21, 142, 493). Society is defined as an organization capable of providing conventional competition (innately ritualized or learned through tradition), and much of the book is devoted to evidence of what are coined as "epideictic" displays as expressions of conventions. Social conventions divert competition between members of a society away from food and into substitute channels to limit population density in an artificial manner and prevent over-exploitation of the food supply (p. 226). Conventional competition intensifies as population density rises and provides the signal in the feedback system that regulates the adjustment between population and food resources (p. 143).

Most of the material in Chapters 2 through 7 on social integration through various means of intercommunication seems superfluous to the theme. There are excellent descriptions of territorial behavior, ranging from arthropods to primitive man, in Chapters 9 and 10, and of communal nuptial displays in Chapter 11. The conventions involved and the patterns of dispersion that result are well established, and their role in limiting numbers seems obvious. One chapter is devoted to the natural selection of display patterns with particular reference to controlling the number of matings. The function of communal roosts as a dispersionary mechanism (Chapter 14) gives meaning to the phenomenon for the first time, according to Wynne-Edwards (p. 298). It is suggested in Chapter 17 that associations of ecologically similar species limit their total numbers to the available resources through interspecific behavioral conventions, contradicting Gause's exclusion principle that no two species can occupy the same ecological niche. The mechanisms of varying natality, socially induced mortality, and deferment of growth and maturity are considered in the last three chapters; and there the book ends abruptly.

Since Wynne-Edwards believes that social organization evolved to regulate populations, almost any aspect of animal behavior can be fitted into the "theory." However, on the one hand all social behavior cannot be related to population homeostasis, and on the other hand social behavior is probably only one of several mechanisms through which homeostasis is achieved. The extensive generalizations in his book are based on so little solid evidence that homeostasis through social behavior cannot as yet be accepted as a theory. It is still in the nature of a hypothesis, and the idea of self-regulation of populations through behavior, as the author's own documentation shows, has occurred to many researchers and dates back at least half a century. Two important references overlooked are Naumov's 1939 paper (Ecological characters in steppe mice and voles. *Zool. Zhur.*, 18: 711–732), in which the evolutionary trend towards social regulation was presented, and J. B. S. Haldane's succinct expression of "Natural regulation of numbers through ritualized contests" (*Ibis*, 97: 375–377, 1955). Therefore, it is difficult to accept Wynne-Edwards' statement that his "theory" had been published previously only by Carr-Saunders in reference to primitive man (p. 21).

Wynne-Edwards' main contributions have been (1) to present an exhaustive, well-documented, well-written review of epideictic conventions and (2) to relate these observations to a clear statement of the concept. He draws upon his rich knowledge of natural history over a broad spectrum of the animal kingdom, providing interesting reading but often losing himself in superficial and irrelevant detail. Although the collective observations on social conventions support the concept, they do not constitute proof. As N. Tinbergen once pointed out to me, there is little proof that even territorial behavior, presumably the most highly developed homeostatic mechanism, functions to regulate numbers. The two best studies seem to be those of H. N. Kluyver and L. Tinbergen (*Arch. néerl. Zool.*, 10: 265–289, 1953) on titmice in Holland and R. E. Stewart and J. W. Aldrich (*Auk*, 68: 471–482, 1951) on removal and repopulation of breeding birds in a spruce–fir forest in Maine.

Had he confined himself more strictly to the subject matter outlined in his first chapter, Wynne-Edwards could have produced a more effective and less controversial volume. Nevertheless, the book will provide a useful background of knowledge and orientation, as well as stimulation, for future research.—Helmut K. Buechner.

Silent spring.—Rachel Carson. Drawings by Lois and Louis Darling. Boston, Houghton Mifflin Company, 1962. 368 pp. $5.00.—For the members of the American Ornithologists' Union this book requires serious consideration. Because of its peculiar nature and especially its impact upon the public, it is not comparable to other new publications on biology. Rachel Carson is trying to awaken a largely indifferent public to consciousness of the implications of continued unrestrained use of plant and animal poisons, called pesticides.

Silent spring is in a prose style entirely different from what most of us are used to reading. Each paragraph is factual and seems to understate its message; but the author does use such flowery phrases as "slumbering volcano," and the impact of the sum of the parts is a tremendous foreboding of doom. Because this book is so partisan in its approach to the pesticide problem (seldom does Miss Carson acknowledge the usefulness of any chemical poisons), it may be questioned whether it will serve only to call down a violent, vitriolic counter-blast. But *Silent spring* had to be written, since it is now clear that the abundance of factual evidence of the effects of poisons on natural populations has not moved the American public or affected American business ethics. If this book had not "brewed up this tempest," its message would not have reached the huge audience it is reaching as a "best seller."

In those areas treated by the book where I have had some experience (i.e., geological processes of erosion and stream flow, vegetation analysis, and bird populations), the author's facts are correct. Her speculations, however (readily identified as such), are far from cautious; but they must be viewed in the context of the book and in the context of statements found in such propaganda pieces as *Open door to plenty, facts and fancy* by the National Agricultural Chemicals Association, and the published sophistry of the U. S. Department of Agriculture: "No one knows how many children have been killed by the fire ant's sting."

In the same way that the author introduced the mysteries of the sea to the dilettante public, she now introduces this same public to the intricacies of the biological community and how this is affected by poisons. She starts off with her real shocker —"A Fable for Tomorrow"—which Dr. George Decker, once strong proponent of a restrained use of pesticides while entomologist for the Illinois Natural History Survey, and now a consultant for the chemical industry, has used to justify condemnation of the whole book as "science fiction." In this chapter she has forecast what might happen if all the known repercussions of familiar and widespread pesticide misuses converged upon one community.

In the chapters that follow, Miss Carson defines and describes the major poisons: chlorinated hydrocarbons, organic phosphates, arsenicals—and briefly touches on the herbicides. Her statements of danger sound alarmist, but these are fantastic chemicals and the complacent public needs to be alarmed. Next she describes a number of spectacular local illustrations of how poisons have affected ground water supplies and the soil, how herbicides make hideous the vegetation bordering country roads, and then documents how sprays broadcast over the countryside have damaged fish, mammals, and birds. She points an accusing finger at all aerial spraying and especially that forced upon local residents over their opposition. Much in the book concerns the effect of these poisons upon public health, and it is unfortunate that more intensive research has not been directed to the central theme of our "pesticide" concern. This is the nub of the problem. I know little about this, but I have inquired of senior and world-famous local physicians how they feel about the book and its implications. Some felt that she overstated, but all felt that her facts are correct and her warning justified and needed. The dangers to men of these poisons are clearly greater than that of the much touted radioactive fallout, and neither can compare to the tragedy of continued uncontrolled human population growth.

Her short primer to inheritance and the biochemistry of energy transfer is well done. Again she prophesies that there is real danger from poisons because these basic chemical energy reactions of life are, in fact, just those which the new poisons are designed to block. Her chapter on cancer would have been improved by discussion of the other school—that viruses may cause cancer; but this omission does not negate her argument. The final chapters, "The Rumblings of an Avalanche," emphasize growing insect resistance to poisons and the intensification of dangerous chemical programs (reminiscent of the experience of physicians with "wonder" drugs in the decade 1945–55), and finally she suggests the areas of alternative control methods, finishing the book with a long list of references. She sounds rather hopeful that further research in biological control will bring success equal to that brought by the control program of the screw worm fly, the Annapolis Valley apple growers' combined-control action, and the male attractants in Gypsy Moth control. I doubt that we can hope for this soon, or that, necessarily, satisfactory solutions will come from biological research alone.

I would have felt much happier if her book, which presents this negative side so well, had also used a chapter each to point out first, the far-reaching benefits of insecticides used in villages, houses, and on people to control tropical diseases and, second, their use to double agricultural output and lower the level of insect contamination to a minimum. This in itself, however, raises a problem because we are only contributing to further human misery if we double food production and decimate disease yet remain gagged by medieval superstitions against teaching birth control.

If, as I am convinced, conservation is the practical application of biological laws, as engineering is the practical application of chemical and physical laws, we must seek to learn the laws and solve the equations of population dynamics. On this account the book exposes a serious and almost universal failing in academic communities. If modern field biology had been practiced in the last 20 years, we would not have to grope for evidence and could answer the industries' propaganda with facts of our own. Laboratory physiologists know many ways to poison the cytochrome system or to block acetylcholine, but field biologists know almost nothing of the population dynamics of common species. As a result, experimental biologists using that mystic figure LD_{50} presume to state categorically what the effects of pesticides are, without knowing anything more than what per cent of poison can kill

50 per cent of their laboratory animals. It is naive or misleading to suggest on this basis what will be the effect of a poison on a population replacement and reproductive potential of wild animals. Stuart and Aldrich's, and also Hensley and Cope's work in Maine (*Auk*, 1951) showed that censuses without marked individuals are totally unreliable to measure even the direct effects of pesticides.

The results of this attitude in the major universities have spread to agricultural schools, to the extension service technicians, and to the public. Most people readily bow to medical experts, engineers, and scientists (mathematicians, chemists, and physicists), but local chemists or fertilizer salesmen and especially, sportsmen, feel free to voice (with authority) contradictions to statements of population dynamics by recognized authorities. Until the universities recognize population biology and the study of intact organisms in their habitat as important fields of study, until they realize that the testing of multiple working hypotheses by circumstantial evidence is as valid as experiment, our best minds will be drawn from these fields. Because an experiment only answers the question asked, it has been convenient for the "scientific tests" of economically interested parties to ask the wrong question about the effects of pesticides and thus gather "incontrovertible" evidence that their poisons do not damage populations. Plenty of facts have already been published which show that many animal populations are being decimated but those who study in this area are so few that they are easily ignored or contradicted.

What evidence can be believed? It cannot be only coincidence that the American societies of Herpetologists and Ichthyologists, and of Mammalogists, and the American Ornithologists' Union have formally stated their concern for the consequences of indiscriminate use of poisons. These organizations are not economically involved in this controversy. The Audubon Societies and U. S. Fish and Wildlife Service and other conservation organizations depend for support upon a favorable public image and for them to be drawn into this public hassle and to be ridiculed by community leaders potentially damages their economic position. If the evidence allowed it, they would like to be out of the mess; but this can hardly be said for the $300 million segment of the chemical industry and its agents. Can there be doubt about whose statements, evidence, and motives are suspect?

Where does the problem lie? There is no doubt whatsoever that poisons have contributed Herculean labors to advances in health and food technology and thereby to the public welfare. Pesticide uses in disease control have resulted in great forward strides in reducing the incidence of malaria, yellow fever, and elephantiasis by spraying houses, cisterns, and towns in the tropics. We must, however, reject the use of these dramatic successes to justify uncontrolled sale of chlordane, dieldrin, and others over the counters of hardware stores to be used for every trivial insect pest. The great victories over tropical diseases have been used in unfair advertising. How can we measure the impact of a two-full-page colored picture of a hairy mosquito on a flesh-colored base, published in major circulation periodicals to advertise the program of the Shell Oil Company?

It doesn't seem to bother the chemical salesman to demand the absolute protection of the classical beauty of our stately elms (by spraying, ignoring sanitation) because of our obligation to pass beauty on to the coming generations, and in the same argument to demand the elimination of roadside brush 20 feet from the edge by spraying because it is cheaper than mowing.

The question is not pro or con poisons (pesticides, insecticides, herbicides, fungicides). That is the question shrewdly put by the salesmen. It is a question of (1) control of the salesmen and technicians pushing uncontrolled use, and (2) insistence upon honest warnings on labels and in advertising. Those who want to see sense put into this mess will recognize the advantages of dieldrin as they do those of morphine, but they can ask for similar guards and restrictions on its use. Along this line, in the Massachusetts Commission to study pesticides the point was made at every meeting that no controls are necessary, only public education.

But public education is not enough in Massachusetts at least. Pesticide users blocked year after year the establishment of a Massachusetts Pesticides Commission. The Dutch elm disease is declared a public nuisance by the General Court and towns are directed to appropriate money for the control of the disease in language that leads most towns to spray 5 pounds per acre of DDT on maples, ash, oaks, and by coincidence, elms. The State Department of Public Works did contract in 1961 for helicopters to spray all state highways for Dutch elm disease. The Department of Natural Resources in June, 1962, recommended to the public that they spray at once for nuisance inch-worms which would disappear in the next two weeks. The U. S. Department of Agriculture proposed the application of *30* pounds per acre of 10 per cent dieldrin on the shoulder of the airstrip, and *100* pounds per acre of 10 per cent chlordane over the buildings, including headquarters and apartments, to control Japanese beetle at Otis Air Force Base. These programs do not consider wildlife except by *post facto* rationalization based upon almost incredibly primitive techniques of analysis.

The only way to assess damage is to do adequate research and yet the proportions of funds for control program vs. funds for study of effects quoted by Rachel Carson for Illinois Japanese beetle control for an eight-year period are $375,000 federal funds for control and $6,000 on study of the effects. In spite of all the money spent upon mosquito control, justified in large part as encephalitis prevention, research on encephalitis in Massachusetts is perennially strangled by lack of funds. The U. S. Congress has appropriated less than 20 per cent of the money approved for the Branch of Research of the Bureau of Sport Fisheries and Wildlife to study the effects of pesticides. The fire ant boondoggle still rolls on because details of the evidence of damage at a new low level of dosage were not complete. This sort of hasty conclusion must be avoided. Anyone with the slightest knowledge of population phenomena knows that it was impossible to get any documentation at all of effects on population numbers or reproductive potential in the couple of years of the lowered dosage. To use the lack of evidence as support for "no damage" and support for the program must be dishonesty or incompetence. A representative of the University of Massachusetts, in May, 1961, stated that the Cooperative Wildlife Research Unit's three-year study had been totally unable to show evidence of damage by pesticides to "your birds." In fact, the funds (inadequate for a reliable study) were received *after* the breeding season in 1959 and the towhees, Yellow-throats, and Chestnut-sided Warblers being studied were not yet back on territory for the third year (1961) when the assertion was made.

On the other hand, the cause of reason is done at least as much damage by the extremists who say poisons are killing us all. These offer justification to claim that the conservationists are screaming to stop all use of pesticides. They, too, are irrational and do not take a considered look at all the evidence either. I think Rachel Carson's book comes dangerously close to that group.

As far as wildlife is concerned, we must sharply separate spray programs in cities, human living space, farmers' crops, etc. (monoculture or totally man-dominated areas, where natural controls are totally bypassed) from aerial programs affecting the ecological complex of numbers which we know is responsible for relative stability of natural populations. We must separate what a man does on his own property from what a community agent does to the countryside. Forest insect sprays and mosquito control are entirely different matters from crop dusting. But when pollution from crop pest control enters ponds and streams, we must demand vigorous restraining action. Locally serious situations can be corrected by intense local spraying (which will have less long-range effect on annual populations), but country-wide year-by-year attrition programs for the comfort of recent suburbanites or the short-term profits of forest product industries do by far the most harm with the least gain.

The pesticide controversy needs facts and men of integrity willing to make honest decisions by the facts which may result in some present human discomfort but a better result for humans in the end. We know that we can't cheat the laws of gravity but the vast majority of Americans is quite glad to try as hard as it can to cheat the laws of ecology. As long as that is true, emotion, economic pressure, self-indulgence, and politics will govern our use of poisons—not facts.

We have already seen that a well-financed, aggressive, outraged, and in some cases, blatantly dishonest program of discrediting this book is under way. If industry and the selfish public could be convinced by facts, that poisons although tremendously valuable are also seriously dangerous, the book would not be necessary. In fact, they cannot, and we must then welcome this book, and using its impact, try to teach the public some sense of responsibility for the environment and the habit-forming drugs now in use.—William H. Drury, Jr.

The study of birds made simple.—Hilda Simon. Garden City, New York, Doubleday, 1962. Pp. I–XI, [XII]–[XIV], 1–143, many black and white text-figs., vigns., etc. 10½ × 7½ in. $1.45.—This is one of many paper-backed works identified by the publisher as "made simple books." Likely one could be purchased at any newsstand. While perhaps no member of the American Ornithologists' Union would, or should, purchase one, neither need he chastise his child or offend his neighbor if he finds that either has done so.

In short, "bird books" of this nature being, too often, very bad, it is refreshing to find that this one is not. It is, in fact, clearly written, generally accurate in nomenclature and terminology, and betrays extensive reading of exemplary sources by the author, some of the material included having but recently appeared in text-books.

The study of birds, obviously, is not made simple; the drawings are crude; there are scattered obvious errors; few paragraphs are without some point for quibbling; yet withal the text seldom deviates widely from the broad essentials of what is currently taken for ornithological truth, and a very wide range of subjects is lightly covered. The sources recommended at the end will lead the hypothetical reader to many more. A small, quiet blow has here been struck, very near the grassroots, both for conservation and biological education.—Robert M. Mengel.