the complex songs of thrushes. Analysis is at several levels, and he breaks away from the more conventional approaches. In reading the article I worried a bit that I was being led up the garden path. His manipulation of data stirs memories of circadian rhythms in unicorns, but I would like to believe his analysis. Nelson goes on, building more general input-output models, and attempts to relate this to arguments about whether behavior is hierarchially or distributionally organized.

The article most in the mainstream of ethological thought on the organization of behavior is that of J. C. Fentress. He brings together a divergent literature on the problems of "action-specific" control versus "nonspecific" activation. The result is an esoteric collage, at times stimulating, at times recondite, and always difficult to decode. I suspect that the use of verbal models in motivation studies may have seen its day. More clarity might have been achieved here through the application of explicit mathematical models to cut through the mass of seemingly contradictory findings, as has been done in some other motivational studies in recent years.

The experiential dimension emerges in two of the more interesting chapters. One, by J. Garcia, J. C. Clarke, and W. G. Hankins, reexamines stimulus-response theory in relation to ethology and to paradoxical findings in experimental psychology. This article is something of a culmination of a growing dissatisfaction manifested in the psychological literature with the simplistic assumptions of the S-R model. The authors propose as an alternative an "information theory" of learning (an unfortunate choice of terms since information theory is so widely understood to refer to communication). This essay is as remarkable for its sophisticated arguments about learning as it is for its uncritical acceptance of ethological theory. The other chapter on experience, by J. M. Davis, is a useful review of imitation, establishing the importance of the phenomenon and revealing, by contrast, how remarkably little it is studied.

I was surprised that only one chapter deals directly with what must be ethology's most rapidly growing and potentially most important area: behavior as a mechanism in ecology. J. R. Krebs has written a lucid and parsimonious review of behavioral aspects of predation. The focus, however, is more on the strategy of searching than on overt behavior. Decisive treatment of various models points the way to further profitable research.

Almost as if to remind one that ethology also includes orientation, a timely brief article is included on magnetic orientation by birds. There is a growing body of evidence that birds use geomagnetic clues in migration. The author, S. J. Freedman, is a physicist who understands geomagnetic fields. He describes in a few words how birds *could* use such information for navigation. The problem of receptors, however, remains.

This book is most welcome. I applaud the goal of the editors, and I am delighted to see that they hope this will be just the first such book of a series. They intend to maintain a forum for ideas, including unpopular and heretical ones. To that end they solicit and welcome manuscripts. I should like to remind them, however, that editorial intervention can be done without the suppression of ideas, and indeed is often called for. For it is as reprehensible to publish an article that will not be read because it is poorly written as it is not to publish the article at all. The chapters in the second half of the book, for example, are altogether too long, and the first of them would have benefited from constructive editing.

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Neuroethological Approach to Animal Learning

Constraints on Learning. Limitations and Predispositions. Proceedings of a conference, Cambridge, England, Apr. 1972. R. A. HINDE and J. STEVENSON-HINDE, Eds. Academic Press, New York, 1973. xvi, 488 pp. $18.50.

Robert Hinde's introductory chapter to this book sets the stage by presenting two very different approaches to animal learning. One view, embodied in the work of psychologists such as Tolman, Skinner, and Hull, holds that there exist "laws of learning" of considerable generality and precision. The data base for these general laws is derived from a few intensively studied species learning arbitrary tasks under rigidly controlled conditions. The countervailing view of ethologists such as Tinbergen and Lorenz is that learning ability, like any other biological characteristic, has been shaped and sharpened over evolutionary time to serve in many cases quite specific ends. A particular species may learn one task very well and show no sign of learning in other situations, or two species in the same genus may show opposite results when presented with the same task to be learned. Hinde's introduction indicates the variety of factors that can operate to determine what is learned.

The remaining chapters of the book provide numerous examples of specialized learning mechanisms and factors that constrain what is learned in vertebrate species. Baerends and Kruijt analyze the responses of the herring gull to its eggs, especially the physical aspects of the stimulus controlling egg retrieval. They show quite clearly that different stimulus properties of the egg are used in triggering different responses. In their words, "physically identical stimuli may be evaluated differently by an animal, according to the activity they are controlling."

C. Blakemore summarizes recent work on the effects of visual experience on the trigger features of visual cortical neurons. In the kitten as little as 33 hours of selective visual experience can markedly alter the response properties of cortical cells, which then constrain the range of stimuli to which the animal subsequently attends. These considerations lead naturally to a discussion of imprinting in birds by P. P. G. Bateson. The effectiveness of visual stimuli both in altering the response properties of kitten cortical neurons and in promoting approach responses and social attachments by precocial birds is limited to a critical period very early in development. Bateson explores in detail how newly hatched chicks learn to recognize food, documenting the existence of critical periods and long-delay associations in this type of learning.

Taste-aversion learning in mammals and birds is one of the most striking examples of a specialized learning mechanism which is at variance with the psychological "laws of learning." This type of learning, discussed by D. J. McFarland, can occur with delays of reinforcement of several hours, is resistant to disruption by electroconvulsive shock, and can occur when the animal is given the reinforcement while in deep surgical anesthesia. Mc-

Farland reviews this literature in the context of feeding behavior viewed as a component of the mechanism for metabolic homeostasis.

Another attack on the generality of currently accepted categorizations of learning comes from psychologists working on the phenomenon of autoshaping. A hungry pigeon will come to peck a response key if the key is illuminated just prior to food presentation, even though food presentation is not contingent on pecking. B. R. Moore and H. M. Jenkins present their extensive experiments on this phenomenon, which blurs the distinction between Pavlovian and operant conditioning. S. J. Shettleworth and P. Sevenster present examples, using hamsters and sticklebacks, of the differential effectiveness of certain reinforcers for certain behaviors. The notion that any arbitrary response can be associated with any reinforcer is simply no longer tenable.

The final section of the book documents a variety of situations in which specialized learning mechanisms appear to operate in man. The formulations of Piaget are considered by A. S. Etienne and H. Sinclair, and Etienne indicates how one of the signposts of cognitive development in infants—the object concept—can be studied comparatively in several animal species. The analysis of developmental constraints and predispositions in human learning is also considered by S. J. Hutt and the special characteristics of language learning by J. C. Marshall and J. Ryan.

The relationship between psychology and ethology has passed through several phases, which Niko Tinbergen recently (*Psychology Today*, March 1974) characterized as follows:

> At first you ignore the other approach, because it is uncomfortable to contemplate. Then you criticize. That is a good sign, because it means that you have an interest in each other. Then you begin to collaborate. In the final phase, you say "It's all so self-evident that we don't need to talk about it any more."

As the studies in this volume witness, the study of animal learning is in stage three, and represents one of the most fruitful syntheses of the approaches of psychology and behavioral biology.

The book presents a very important and useful set of insights into the factors governing the expression of learning in animals. Physiologists on safari in the interneuron jungle searching for the elusive engram would do well to consider what tasks their animal may have evolved specialized mechanisms to learn. This strategy recently made it possible to demonstrate one-trial food aversion learning in a terrestrial mollusk whose central nervous system is amenable to detailed cellular analysis. Anyone working on the behavioral or physiological analysis of learning should consider this book—perhaps in conjunction with the treatment of the same matters in *Biological Boundaries of Learning*, edited by Seligman and Hager (Appleton, Century, Crofts, 1972)—required reading.

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On the Meaning of Brain Size

Evolution of the Brain and Intelligence. HARRY J. JERISON. Academic Press, New York, 1973. xiv, 482 pp., illus. $25.

The first step is to measure whatever can be easily measured. This is okay as far as it goes. The second step is to disregard that which can't be measured or give it an arbitrary quantitative value. This is artificial and misleading. The third step is to presume that what can't be measured easily isn't very important. This is blindness. The fourth step is to say what can't be easily measured really doesn't exist. This is suicide.—DANIEL YANKELOVICH (*1*)

This description of what its author calls the "McNamara fallacy" might, I believe, characterize a good bit of past and present biological research, and particularly some of the arguments presented in this interesting, but highly debatable, book by Jerison. I'm not sure the fourth step is actually taken, but a case can surely be made that the first three have been, and the beginnings of four.

This is a book about brain size more than brains per se. It deals with the evolution of brain size in almost all vertebrate taxa and its relationship to behavior ("biological intelligence") and to other variables such as body weight, neuron size and density, neuron numbers, glial/neural ratios, and dendritic branching. Those who have followed some of Jerison's earlier writings (*2*) will find nothing new in his philosophy here, but instead an expanded and more detailed approach to the use of brain weights and volumes to predict other, "more interesting" variables (or parameters) such as the numbers of "vital" and "extra" neurons in fossil animals.

One of the claims made on the dust jacket is that this book "is quite possibly a landmark publication." I will agree here, for since Edinger's two marvelous publications (*3*) nothing has appeared in the English language which brings together so much of the paleoneurological evidence and which offers a number of theoretical positions that might be debated and tested. Whatever this reviewer's personal opinions of the merits of this book, it clearly deserves the most careful attention, thought, and additional research.

We are also told on the dust jacket (by T. Melnechuk) that the author is "stubborn about preferring his own methods of analysis and interpretation; honest about admitting his biases; generous in trying to perceive the good points in the work of friendly rivals." As a rival I would like to take special exception to the last comment, particularly as Jerison claims this book will be of particular interest to the anthropologist. Jerison has done a remarkable job of avoiding my own past criticism of his work, particularly my 1966 article (*4*). That he must have read it is clear from the discussions, but he does not cite it. In that article, I pointed out that the neuroanatomical evidence clearly indicated that during primate evolution there have been quantitative changes in the three major types of cortical tissue—agranular, eulaminate, and koniocortex—and that the evidence also indicated different neural densities for each of the three major regions. This would make the use of *one* density figure erroneous when applied within evolving lineages, such as the fossil hominids. In other words, I was concerned in that critique with the generation of fictional numbers. That concern has only been made stronger with the publication of this book, and