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ON GOING BACK TO NATURE: A REVIEW OF SELIGMAN AND HAGER'S BIOLOGICAL BOUNDARIES OF LEARNING[1]

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The history of behaviorism has been marked by numerous challenges, both from within psychology and from without. Of late, these challenges have been directed primarily at Skinnerian behaviorism, known to its practitioners as the "experimental analysis of behavior", perhaps because it has been the most successful behavioristic approach or perhaps because it has been the most ambitious (Skinner, 1953; 1957; 1971). One of the most significant and persistent challenges has come from students of ethology (Lorenz, 1965). The points of conflict between ethology and behaviorism are numerous: first, ethology views its subject matter as primarily the result of native endowment (Lorenz, 1965), and criticizes behaviorism for its overemphasis on the role of experience in determining behavior. Second, largely as a result of its emphasis on native endowment, ethology emphasizes the species-specific character of behavior and is skeptical of the trans-species generalizations made by behaviorists. Third, ethology is essentially a study of structure (Eibl-Eibesfeldt, 1970; Tinbergen, 1951). It is aimed at understanding the ways in which behaviors are ordered, and the nature of the internal structures that allow such order to be expressed. Ethology criticizes behaviorism for its overemphasis of the *functional* analysis of behavior without apparent regard for the nature of the internal structures.

In addition to these fundamental differences in orientation and underlying epistemology, methodological differences have made it virtually impossible for behaviorism and ethology to have mutually positive influences. Research in ethology centers around the systematic observation of naturally occurring phenomena, while research in the experimental analysis of behavior centers around the institution of somewhat artificial ones. In short, the two disciplines collect and interpret very different kinds of data. Furthermore, the hallmark of experimentation that does occur in ethology—the so-called "deprivation" or "isolation" experiment (Lorenz, 1965)—yields strikingly different conclusions when viewed by experimental psychologists rather than by ethologists (Lehrman, 1953; 1970; Lorenz, 1965). Thus, the two disciplines have different underlying assumptions, which lead to different research methods, which yield different sets of data, and ultimately, different conclusions. Furthermore, there have been no obvious points at which these chains of difference could be bridged, permitting the two disciplines to communicate positively with one another. Instead, each of the disciplines has developed and elaborated itself quite successfully without attempting to incorporate the other (*e.g.*, Eibl-Eibesfeldt, 1970; Honig, 1966).

This is not an optimal state of affairs. Ethology and the experimental analysis of behavior are both fundamentally concerned with the origins of adaptive behavior, and they should be able to contribute to each other's development. Fortunately, a rapidly growing set of laboratory observations over the last few years may provide the basis for a new dialogue between ethologists and psychologists. These observations suggest a significant contribution by species-specific behavioral characteristics to the phenomena obtained within the context of the experimental analysis, and help bridge the methodological gap by providing a substantial data base for the interconnection of ethological principles with the principles de-

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[1] Seligman, M. E. P. and Hager, J. L. *Biological boundaries of learning*. New York: Appleton-Century-Crofts, 1972. Pp. XI +480, $12.95.

[2] I wish to thank Myrna Schwartz for illuminating discussions and criticisms of earlier drafts of this manuscript, and the ideas contained therein. Reprints may be obtained from the author, Department of Psychology, Swarthmore College, Swarthmore, Pennsylvania 19081.

rived from the experimental analysis. These observations, which may be characterized as indicators of biological boundaries or constraints on learning, are the fundamental concerns of this paper. A number of recent papers (Bolles, 1970; Rozin and Kalat, 1971; Seligman, 1970; Shettleworth, 1972; Staddon and Simmelhag, 1971) have focussed on the relation between organismic predispositions and experimental impositions in the determination of laws of behavior. Each of these papers, in keeping with the ethological tradition, suggests that the search for general laws of learning, independent of naturalistic or species-specific constraints, may be in vain. Some of the papers (Bolles, 1970; Seligman, 1970; Staddon and Simmelhag, 1971) suggest that the principles of behavior already obtained in the artificial laboratory setting are contaminated by species-specific contributions. Others (Rozin and Kalat, 1971; Shettleworth, 1972) suggest that the laboratory may not be an appropriate place to discover such general laws as do exist. All of the papers, however, and others as well, provide a clear and concrete vehicle with which the ethological tradition and the behaviorist one can articulate with each other.

Much of the recent experimental and theoretical work that led to consideration of how the organism's built-in characteristics determine the laws of behavior has been brought together by Seligman and Hager. Their book is organized around Seligman's (1970) concept of "preparedness", and elaborates preparedness by example rather than by logical argument. This review has the following objectives: first, it briefly outlines the theory of preparedness, and relates the contents of the book to the theory. Second, it discusses some problems with the theory and with the manner in which the theory and its exemplars are related. Finally, it discusses the relation between the phenomena that suggest the concept of preparedness and the more familiar phenomena observed in conditioning laboratories, and discusses the implications of biological constraints on learning for the basic assumptions and strategies that have characterized the experimental analysis of behavior.

Preparedness and its Exemplars

Research in learning has been guided by the assumption that the particular stimuli, responses, and reinforcers one chooses to study will not have a determining influence on the outcomes one obtains. Said another way, it has been assumed that the specific elements of an experiment may be chosen arbitrarily, and may be interchanged, without seriously influencing experimental results. It is this very premise, called by Seligman and Hager the "equipotentiality premise", that allows one to be confident that particular experimental results are instances of general laws:

> Psychologists had hoped that in the simple, controlled world of levers and mechanical feeders, of metronomes and meat powder, something quite general would emerge. If we took an arbitrary action such as pressing a lever, and an arbitrary organism such as an albino rat, and set it to work pressing the lever for an arbitrary foodstuff, *by virtue of* the very arbitrariness of this contingency, we would find features of the rat's behavior general to real life instrumental learning . . . the very arbitrariness and unnaturalness of the experiment(s) was assumed to guarantee generality, since the situation would be uncontaminated by past experience the organisms might have had and by special biological propensities he might bring to it. (Seligman and Hager, p. 2, italics theirs)

While they acknowledge that the strategy of studying arbitrary events has in fact resulted in substantial progress, in the form of empirical laws and generalizations, Seligman and Hager introduce an important *caveat* that is the foundation for the book: "Inherent in the emphasis on arbitrary events, however, is a danger: *the laws may not be general, but peculiar to arbitrary events*" (Seligman and Hager, p. 3, italics theirs). They suggest the notion of *preparedness of association* as an alternative to the equipotentiality premise. Organisms come biologically equipped to make some (prepared) associations, and biologically very ill-equipped to make others (contraprepared). In the middle are the arbitrary (unprepared) associations, which have been studied traditionally in learning laboratories. The concept of preparedness is thus a label for an ease of learning continuum, and Seligman and Hager suggest that the very laws of learning may vary with the place on the preparedness continuum that particular experiments sample. The concept of preparedness is very much in keeping with the persistent concerns of ethology (Lorenz, 1965) that histories of behavioral acquisition must be evaluated in their appropriate evolutionary contexts, and that the biological characteristics of a species must play a significant, and often decisive, role in determining the influence of experience. Preparedness also has the virtue of establishing criteria for evaluating species-specific contributions to learning phenomena that make sense *within* the context of the laboratory. Let us examine, then, some of those criteria.

First, there must be no ambiguity about what the authors endeavor to capture with the concept of preparedness. The "things" which are more or less prepared are *associations,* or contingency learning, not the reception of particular stimuli (the appropriate concern of sensory psychology) or the emission of certain responses (which might well be described by the concept of operant level). This point is extremely important in assessing both the theory of preparedness and the evidence on which it is based, and is discussed in some detail below. Second, given that preparedness represents a continuum for the ease of learning of associations, how is the continuum to be defined? Seligman and Hager choose the following operational definition: "The relative preparedness of an animal for learning about a contingency is defined by how degraded the input can be before the output reliably occurs which means that learning has taken place" (Seligman and Hager, p. 4). Degradation of input may be reflected in a variety of ways: number of trials necessary for learning; the maximum delay of reinforcement that will still result in learning; the maximum CS-US interval that will still result in learning; the extent to which the events studied in a particular experiment can be literally degraded, physically, and still result in learning; and other ways limited only by the ingenuity of the experimenter. While no one of these operational criteria is sufficiently broad to be meaningful in evaluating all experimental results, it is hoped that the cluster of criteria will provide such breadth, and that the intercorrelations among criteria, where applicable, will be sufficiently high to inspire confidence that the different individual criteria reflect the same underlying process.

As outlined thus far, the preparedness continuum is nothing more than a convenient test for estimating and describing ease of learning. The authors intend it to be a good deal more. They endeavor to transform it into an explanatory and predictive theory by asserting that (1) laws of learning, (2) mediating physiological substrates, and (3) mediating cognitive mechanisms all vary with ease of conditionability, and that where any particular association may fall on the preparedness continuum can be predicted by examination of the evolutionary selective pressures that have influenced the development of a particular species.

These various features of the theory of preparedness can perhaps be best understood with reference to a set of concrete empirical findings. The phenomena of taste--aversion learning, or poisoning, which comprise a large segment of the Seligman and Hager book, provide a detailed illustration of the theory of preparedness at work. The hallmark experiment in this field, at least with respect to preparedness, is one by Garcia and Koelling (1966).[3] Garcia and Koelling demonstrated that rats differentially associate tastes as CSs with stomach illness as the US (prepared), and exteroceptive CSs with shock as the US (unprepared), but fail to associate taste with shock and exteroceptive stimuli with stomach illness (contraprepared). This experiment might be considered a model of the kinds of data necessary to substantiate the theory of preparedness. Garcia and Koelling clearly demonstrated that differential *associability,* rather than differential *sensitivity* to lights, sounds, shocks, tastes, or illness, is what determined their findings. This is a demonstration of what Thorndike called "belongingness" and captures the essence of what Seligman and Hager must mean when they assert that *associations* are prepared. In order to demonstrate unequivocally that an organism is prepared to associate A and X, but not B and X, one must either demonstrate that the organism can associate B and something other than X quite well, or be open to the argument that B is simply not as salient a stimulus as A. That

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[3] All of the studies on taste aversion learning referred to in this section are included in the Seligman and Hager book. In addition, all studies cited in the review without a year of publication are included in the book, and not published elsewhere.

taste-aversion learning is *prepared* (by the criterion of degradation of input) in addition to being selective is evidenced by the finding that delays between taste and poison of 75 minutes do not impair the acquisition of a learned aversion to the taste (Garcia, Ervin, and Koelling, 1966). This is in marked contrast to the maximum CS-US intervals that produce conditioning in unprepared situations (Beecroft, 1966).

A companion piece to the Garcia and Koelling experiment is one by Wilcoxin, Dragoin, and Kral (1971). They argue, in keeping with Seligman and Hager's view, that selectivity of taste-illness association is not a peculiar, isolated phenomenon, but an instance of a general principle: organisms are constructed to associate selectively with illness stimuli from the sense modality they use in recognizing food. In the case of rats, this is taste, and presumably smell. In a different species, however, it might be vision. By knowing some of the biological characteristics of the species in its intercourse with the natural environment, one could predict what class of stimuli would be differentially associable with stomach illness. Wilcoxin, *et al.*, reasoned thus to predict that quail, which identify food visually, selectively associate illness with the visual characteristic of the stimulus and not with its taste, a prediction that they confirmed experimentally.

Some other less descriptive and more theoretical aspects of the theory of preparedness are suggested by other studies in the book. There is evidence that the physiological mediation of taste-aversion learning is different from the mediation of arbitrary associations (Garcia, McGowan, and Green, 1972; Nachman, 1970; Roll and Smith). There is also evidence that taste-aversion learning, perhaps unlike other classical conditioning (*e.g.*, Kamin, 1969; Rescorla and Wagner, 1972) is fundamentally noncognitive (Roll and Smith; Kalat and Rozin). Indeed, the fit between taste-aversion learning and the theory of preparedness is so good that it is almost as if the theory of preparedness was constructed specifically with an eye toward organizing the literature on taste-aversion learning and reconciling it with more familiar learning phenomena. As we shall see, the relation of data to theory with regard to the other phenomena included in the book is considerably more tentative and ambiguous than in the case of taste-aversion learning.

Having illustrated the theory of preparedness with the phenomena of taste-aversion learning, let us examine more skeletally the other contents of the book. The readings are organized into five sections. Each reading is introduced by a short note relating it to the theory of preparedness. The first section is labelled "classical conditioning". It includes 10 papers, all of which focus on taste-aversion learning, or the related phenomena of specific hungers (*e.g.*, Rozin, 1967) in the rat. Some of those papers have already been described. The next section is a collection of examples of preparedness in instrumental learning. The papers on autoshaping (Brown and Jenkins, 1968; Williams and Williams, 1969) and the classic paper by the Brelands (1961) will be most familiar to readers of this Journal. Also included are an excerpt from the writings of Thorndike (1898), a paper describing some peculiar aspects of discrimination learning in the dog by Konorski and his colleagues (Dobrzecka, *et al.*, 1966), a study of punishment and escape by Bolles and Seelbach (1964), and an assessment of the difference between social and non-social stimuli in discrimination learning experiments by Stimbert (1970). The relation between these studies and the theory of preparedness is not as well worked out as in the case of taste-aversion learning. Rather, it seems that these examples are meant to convey the more general point that an organism's biological characteristics contribute to the phenomena of learning one observes, even within the methodological framework that was designed to ensure arbitrariness.

Next is a section on avoidance learning, including Bolles' (1970) significant reappraisal of the avoidance literature in terms of species-specific predispositions, amplified by Hineline and Rachlin's (1969) discussion of the difficulties involving in training pigeons to key peck to avoid shock. There is also a demonstration of belongingness in avoidance learning in chicks (Shettleworth), and a contrast between taste as an arbitrary cue in shock-avoidance learning and as a prepared cue in taste-aversion learning (Garcia, Kovner, and Green, 1970). Finally, Allison, Larsen, and Jensen (1967) demonstrate that Miller's (1948) classic avoidance experiments may have been contaminated by species-specific influences.

The final two sections depart from the familiar laboratory setting in presenting impressive examples of prepared learning from comparative psychology and ethology, more general theoretical accounts of the relation between species-specific and experiential determinants of behavior from the ethological perspective, and extensions of the notion of preparedness to human functioning. The comparative work includes a sample of Bitterman's research (1965), the study of imprinting (Gottlieb, 1965), the study of bird migration (Emlen, 1970), and some of Marler's (1970) work on bird-song learning. The more theoretical accounts are excerpts from the books of Tinbergen (1951) and Manning (1967). The section on human functioning focusses first on language learning in man and chimp (Gardner and Gardner, 1969; Lenneberg, 1969) then on Piaget's theory of development and its relation to studies of infant conditioning (Furth, 1969; Sameroff, 1971), and finally on the role of preparedness in behaviorally oriented psychotherapy (Seligman, 1971; Wilson and Davison, 1968).

This, then, is a brief outline of the contents of the book. Independently of how one evaluates the theory of preparedness and the relationship between the theory and the data, this collection of papers is superb. It bridges a gap between the American learning tradition and the European ethological tradition, it presents some of the most impressive and problematic studies of animal learning under naturalistic conditions, and it attempts to extend some of the key issues in learning to human functioning and development. In sum, the work presented in the book exemplifies what will probably be *the* problem in the study of animal behavior for the foreseeable future, all organized around a preliminary attempt (preparedness) to solve that problem.

Preparedness: a Critique

The concept of preparedness represents the first real effort from within the learning tradition to integrate general principles of learning with both naturalistic observation and laboratory anomalies. As with virtually all first efforts, it is less than perfect. In this section, I will first point out some difficulties with the logical status of the concept, and then discuss some problems with the relation between the concept and some of the phenomena it endeavors to capture. Finally, a modification in the operational criteria for preparedness will be suggested that appears to solve some of these problems.

Preparedness, as operationally defined, represents a label for an ease of learning continuum. However, Seligman and Hager wish the concept to be more than a descriptive label. Indeed, as Seligman and Hager recognize, unless preparedness can say something about mechanism, which will give it explanatory power, it is hopelessly circular. Associations learned rapidly are prepared, by definition. Associations learned slowly are unprepared. The label, preparedness, adds nothing to the measure of ease of learning. Bolles (1970) faced the same problems in his discussion of avoidance learning. An avoidance response is learned rapidly only if it is a species-specific defensive reaction (SSDR). If avoidance is learned rapidly, then the response is an SSDR. What breaks the circularity in Bolles' account of avoidance is his implication that there might be an independent means of assessing what behaviors are SSDRs—by observing the organism's responses to danger in nature. With this tool in hand, one can observe animals in the wild, and predict which responses will be learned easily and which with difficulty. Seligman and Hager aspire to a similar kind of predictive power by suggesting, as does Bolles, that examination of the organism's intercourse with its natural environment will yield predictions as to which associations are prepared and which are not. The Wilcoxin *et al.* (1971) follow-up of Garcia and Koelling's (1966) dramatic findings, mentioned above, is a clear example that such a strategy can be predictively fruitful.

So let us examine preparedness as both a descriptive continuum and a theoretical tool. The extent to which an association is prepared is determined by the degree of degradation of input an association can survive and still be learned. This operational definition immediately raises a problem. How does one assess degradation of input? The kinds of indices Seligman and Hager refer to (*e.g.*, number of trials, delay of reinforcement) are sensible and useful within the context of the well-studied experimental paradigms to which they refer, like Pavlovian and instrumental conditioning paradigms. Certainly Pavlovian conditioning procedures can be compared with respect to a large number of parameters that might reflect degradation of input. The problem is this: any assessment of degradation of input assumes that the paradigms on which the assessment is based are reliable measuring instruments. To compare taste-aversion learning to the laws of Pavlovian conditioning and conclude that taste-aversion learning is prepared is at the same time to invest Pavlovian conditioning with the status of a unitary, reliable process; that is, such a comparison assumes that all instances of Pavlovian conditioning have something in common aside from the procedure that produces them. Suppose one encounters a phenomenon that appears to be prepared. Presumably, it appears prepared because the procedure that produced it differs significantly (input is degraded) from past procedures that have produced other familiar phenomena. What then is the justification for comparing this new phenomenon with the familiar ones to assess its preparedness? Presumably, it is not similarity of procedure, since the procedures are different. If not, it must be similarity of some underlying process, which is assumed to characterize the familiar phenomena, and appears to characterize the new one. But what is the underlying process that uniquely characterizes Pavlovian conditioning? I think this question does not, as yet, have an answer. Despite the numerous characteristics that have been ascribed to Pavlovian conditioning phenomena in general (*e.g.*, Kimble, 1961), the only one that can apply to all phenomena commonly referred to as Pavlovian conditioning is the experimental paradigm that produced them. Thus, what Seligman and Hager appear to be doing in relating seemingly prepared phenomena to traditional categories of learning (*i.e.*, Pavlovian conditioning, instrumental conditioning, avoidance, discrimination) is treating differences in paradigm as differences in process. The learning categories we employ are extremely useful devices for simplifying and reducing the phenomena we observe into a manageable number of classes. However, they are inevitably a somewhat oversimplified and artificial set of distinctions. It may be that the various phenomena that gave rise to Seligman and Hager's book demand that we reformulate our most fundamental principles and categorizations. The concept of preparedness, as presently defined, does not facilitate a reformulation, and may even serve to fortify our current one.

This problem of dependence of an assessment of preparedness (degradation of input) on current labels for different types of learning is reflected in another, more concrete way. Seligman and Hager's theory seems to require that a new phenomenon be placed in its appropriate learning category unambiguously. For example, before we can assess whether taste-aversion learning is prepared Pavlovian conditioning, we need to be sure that it is Pavlovian conditioning, which is not obviously the case (see Rozin and Kalat in the Seligman and Hager volume). There are difficulties in specifying the unconditioned response (UR) and the conditioned response (CR) in taste-aversion learning, as well as in specifying the relation between the UR and the CR. Suppose one considered taste-aversion learning as an instance of discriminated avoidance learning (the rats do, in fact, avoid a flavor that has been paired with an aversive, internal stimulus). A number of characteristics of taste-aversion learning that make it stand out seem less surprising when it is viewed as avoidance learning. Rapid acquisition (Bolles, 1970) and resistance to extinction (Solomon and Wynne, 1954) characterize a good deal of avoidance learning. There is still, of course, the long delay between taste and poison to be accounted for, but the point here is not to suggest that taste-aversion learning is easily related to familiar phenomena. Rather, it is to show that how prepared taste-aversion learning is depends upon the paradigm it is evaluated against, and that there are no clear criteria, other than procedures themselves, for labelling phenomena as one kind of learning or another.

Autoshaping provides an even clearer example. A response key is briefly illuminated and food delivery follows. After a number of such pairings, pigeons come to peck the key (Brown and Jenkins, 1968) and continue to peck even when pecks prevent food delivery (Williams and Williams, 1969). Seligman and Hager include these papers in the section on instrumental learning. But what makes the phenomenon instrumental? Presumably, since the key peck has long been a prototypic instrumental response, the autoshaping phenomena are considered instrumental. However, the procedures themselves, as well as some of the findings, parallel exactly some rather standard Pavlovian findings (Sheffield, 1965). While the general significance of the issue may be purely academic, and no deeper than a labelling problem, for the notion of preparedness it is crucial. Considered as instrumental, autoshaping suggests that key pecking is prepared. Only a few (and often only one) pairings of peck and food are necessary for pecking to be fully established. This is true even when there is no contingency relation between pecking and food delivery (Brown and Jenkins, 1968; Schwartz and Williams, 1972 *a, b*). On the other hand, considered as Pavlovian, autoshaping suggests that the acquisition of key pecking is unprepared. A substantial number of keylight-food pairings are typically required before the first key peck occurs. Thus, the relative preparedness of key pecking depends upon which measuring tool (response-reinforcer pairings or stimulus-reinforcer pairings) one uses. Whether or not key pecking is prepared is, at present, unresolvable, not especially because of weaknesses in the notion of preparedness, but because of weaknesses in the distinctions we make among different kinds of learning. Indeed, current investigations of the autoshaping phenomenon (*e.g.*, Gamzu and Schwartz, 1973; Gamzu and Williams, 1971; Jenkins and Moore, 1973; Schwartz and Williams, 1972 *a, b*) have been directed at assessing the extent to which key pecking is influenced by Pavlovian and instrumental variables, and it seems clear that the key peck is not a simple example of either Pavlovian or instrumental conditioning alone.

The problem becomes even more apparent when we attempt to deal with more naturalistic learning phenomena that fit conveniently into no laboratory paradigm. Language acquisition is a dramatic case in point. Seligman and Hager suggest that language acquisition is prepared. The argument is based mainly upon the speed of acquisition in the absence of any explicit program of contingencies. Since vocalizations are only rarely systematically reinforced, and discriminations of closer and closer approximations to English are trained haphazardly at best, and pairings of word and object frequently suffer long delays, one could assert that whether one's model for language learning is operant or Pavlovian, the input is degraded and the acquisition is rapid; thus, the learning is prepared. However, we can ask whether, to a naive observer unfamiliar with laboratory learning paradigms, language learning appears unusually rapid. Language is acquired after some two or three years of linguistic input. The daily amount of input is enormous (and relevant, as Lenneberg's studies of children of deaf parents suggest). This can only be considered rapid acquisition with degraded input if one's model of language learning is based upon the deliberate acquisition of individual associations in accordance with traditional laboratory paradigms. The point, as in the case of the taste-aversion learning and autoshaping examples, is not to suggest that language learning poses no serious problem for traditional conditioning principles, for surely it does; rather, it is to suggest that the concept of preparedness, which depends, as it does, on the viability of traditional laboratory paradigm distinctions, may not be the best way to capture and emphasize these problems.

Thus, to summarize this point, a serious investigation of preparedness requires an implicit acceptance of traditional learning paradigm distinctions, which may reify distinctions that are not overly secure in their own right. To overcome this difficulty, it may be necessary to develop different operational criteria of preparedness.

Preparedness as a Continuum

Seligman and Hager assert that preparedness represents an ease of learning continuum, and that " 'learning' is continuous with 'instinct' " (p. 5). Indeed, since the operational criteria for preparedness are all continuous, there appears no recourse but to view the underlying conception as continuous as well. The idea that preparedness is a continuum probably does not appropriately capture the underlying concern with the interaction of biology and experience, which gave rise to the concept of preparedness in the first place. Skinner (1966) and Lorenz (1965), as leading spokesmen for their respective disciplines, have discussed the relation between phylogenetic and ontogenetic determinants of behavior. Both tried to distinguish clearly between these two sources of behavior. Both emphasized how easily one might confuse these two influences, and the chaos that might result from such a confusion. Each in his career consistently overemphasized one of these sources, effectively minimizing the significance of the other. Thus, both are guilty, to some extent, of distortion. However, neither is guilty of obscuring the distinction between phylogenetic and ontogenetic sources of behavior which concerned them both. Phylogenetic and ontogenetic influences on behavior are indeed quite different, and in principle, separable. While the important and appropriate point of Seligman and Hager's collection is to emphasize that one must study the *interaction* of these two influences, it should nevertheless be clear that the interaction results from two disparate influences. My concern with the notion of preparedness as a continuum is that it may obscure the distinction between the phylogenetic and ontogenetic contributions to it. Consider, for example, the problem posed by an organism's past history. When an experimenter places an organism in an artificial situation, at least two different variables that might influence an assessment of preparedness are unknown. First, to what extent does the situation capitalize on species-specific characteristics? This is the main focus of preparedness, and it is a phylogenetic question. Second, to what extent does the situation capitalize on the organism's past history? This is clearly not at the heart of preparedness, and it is an ontogenetic question. A demonstration of preparedness in a particular experimental setting may have either a phylogenetic origin, or an ontogenetic origin. Such an experiment may permit either the expression of species-specific behavior, or the positive transfer of early learning, or both. A simple determination that an association is prepared does not carry with it a determination of the origin of the preparedness. Numerous investigators (*e.g.*, Lehrman, 1953; 1970) have been sensitive to this problem for some time, but it is not an easy problem to solve. In the interest of avoiding this confusion, a criterion for preparedness that does not make learning continuous with instinct would be desirable. A discrete operational criterion for preparedness might better capture the discrete contributions of phylogeny and ontogeny.

What is Prepared

As I discussed above, the notion of preparedness means neither that certain responses are more likely to occur than others (a fact presumably captured by the notion of operant level), nor that certain stimuli are more likely to be detected than others (a matter for sensory psychology), but that certain associations are more likely than others. Certain contingencies, between a particular CS and US, or between a particular response and reinforcer, are more easily learned than others. This view focusses attention appropriately on that aspect of the interaction between biology and environment, which is especially relevant for the study of learning. However, the criterion for preparedness of degradation of input does not, by itself, allow one to distinguish phenomena resulting from prepared *associations* from those resulting from the choice of especially salient stimuli or especially probable responses. In order to show, unequivocally, that what is prepared in any particular situation is indeed an association, a number of demonstrations in addition to the degradation of input are necessary. Consider two CSs, A and B, and two USs, X and Y. It is necessary, but not sufficient, to show that an association of A and X is acquired with input degraded. As was argued above, it is also necessary to show that an association between A and Y is not prepared (indicating that preparedness is not simply the result of CS A), that an association between B and X is not prepared (indicating that preparedness is not simply the result of US X), and that an association between B and Y is prepared relative to an association between A and Y (indicating that the other results are not simply attributable to A being more salient than B). The experiments by Garcia and Koelling, Wilcoxin, *et al.*, and Shettleworth in the Seligman and Hager book are concrete examples of research that essentially fulfills these requirements.

This is not a minor point. An illustration of the difference between preparedness as indicated by differential associability and mere stimulus salience or operant level can be seen in Bolles' (1970) analysis of avoidance learning. Bolles argues that only Species-Specific Defensive Reactions (SSDRs) are rapidly learned as avoidance responses. This could mean at least two different things, one of which is trivial, while the other is quite significant. Bolles' arguments could be reduced to arguments about operant level. Behaviors that are not SSDRs simply do not occur; hence they cannot be reinforced. If one could somehow induce them, however, they would be learned as readily as SSDRs. But Bolles clearly intends something different. His argument is that only SSDRs can be *associated* with the alleviation of aversive stimuli. Even if one ensured the occurrence of a non-Species-Specific Defensive Reaction, it would not be rapidly acquired as an avoidance response. Some recent evidence with the pigeon supports Bolles' assertions (Schwartz and Coulter, 1973).

Thus, if Seligman and Hager's criteria for preparedness are applied stringently, to be certain that preparedness refers only to particular *associations,* the concept overlaps substantially with Thorndike's (1935) notion of "belongingness". While some studies in the book satisfy the belongingness criterion, others clearly do not. For example, Stimbert shows that conspecifics are more effective discriminative cues than non-social stimuli in an open field maze task for rats. While this finding is interesting, it does not really address the concept of preparedness. As it stands, it merely demonstrates that some stimuli are more salient than others. The complementary *situation specific* superiority of social stimuli still needs to be demonstrated. Similarly, Allison, Larson, and Jensen report that when rats run from white to black they learn shuttlebox avoidance far more rapidly than when they run from black to white. They accurately suggest that a lighted area may therefore be an unconditioned aversive stimulus to the rat. Again, however, without supporting evidence, this finding does not really speak to preparedness. Also, Gardner and Gardner impressively demonstrate that a chimp can show signs of language learning if a well-articulated response system (hands and arms) is the vehicle instead of a poorly articulated one (vocal apparatus). This demonstration seems more obviously related to operant level than to preparedness. It does not show that associations between particular digital manipulations and particular objects are more prepared than associations between particular vocalizations and those objects. Gardner and Gardner capitalized on the fact that chimps can manipulate fingers, though they cannot speak. It should be said that these studies were not designed to test the theory of preparedness, and that they are indeed very suggestive examples of where one should look to investigate preparedness thoroughly. But they do not, in themselves, offer support for the concept as it is defined.

Thus, the final criticism is that not all of the evidence marshalled in support of preparedness really supports it. One might argue that this is unfair criticism, because the criterion for preparedness it implies is overly stringent. On the contrary, modification of the criteria for preparedness to include belongingness is essential. If one adheres strictly to the belongingness criterion, then some of the problems with assessing degradation of input discussed above can be circumvented. Showing that A belongs with X (key pecks and food, jumping and shock, taste and illness, *etc.*) is sufficient to demonstrate that some associations are prepared. Moreover, it frees one from dependence upon the traditional paradigms of learning in assessing preparedness. It does not matter whether taste-aversion learning is Pavlovian conditioning or discriminated avoidance conditioning. In either case, it is prepared simply because the phenomenon depends critically on the relationship between the taste and the poison (as one can demonstrate by first substituting light for taste, then shock for poison, in the same paradigm). Nor does it matter whether autoshaping is considered Pavlovian or instrumental. Substitute a tone for a keylight (Schwartz, 1973), or a treadle-hop for a key peck, or a shock for food (Rachlin, 1969) and the phenomenon will be different though the paradigm is the same. An assessment of the degree to which input can be degraded and learning still occur, in conjunction with an assessment of belongingness, will in fact make the concept of preparedness paradigm free. Furthermore, an emphasis on belongingness changes slightly the thrust of the concept of preparedness. Preparedness is now a description of the degree to which particular stimuli, responses, and reinforcers are interdependent in producing particular phenomena. As we shall see below, to the extent that such interdependence is characteristic, it introduces some rather profound problems with respect to the set of definitions and premises on which the operant conditioning edifice is built.

Preparedness: implications

The review to this point has focussed on the concept of preparedness and some of its shortcomings. While these shortcomings exist, they must not obscure the overriding significance of the problem that preparedness endeavors to solve. The fact is that psychologists of learning have essentially ignored biological contributions to learning phenomena. The studies in the Seligman and Hager book, and others as well, have made it clear that we can ignore biological contributions to learning no longer. Skinner (1938) rejected a methology that entailed what he called "botanizing", and appropriately so. He chose, instead, to make some simplifying assumptions, which resulted in a set of powerful empirical generalizations. However, what we must assess are the implications of findings like those included in the Seligman and Hager book for the status of those generalizations. Skinner asserted that he was studying the "behavior of organisms". Some might argue that he was, in fact, studying only the bar pressing of white rats. The truth, undoubtedly, is that he was studying some of the behavior of some organisms. Perhaps in this section we can come to a clearer understanding of just how generalizable general laws of learning really are. This section will address three questions: first, what are appropriate units of behavior? This entails an assessment of the implications of biological constraints on learning for our strategies of identification and definition of behavioral units. Second, is the "box" really arbitrary? This entails an analysis of learning phenomena in the artificial environment; it is necessary to reevaluate familiar procedures in terms of possible biological contributions to the phenomena observed. Finally, is learning that occurs under naturalistic conditions ever arbitrary? This entails an assessment of learning phenomena that occur in nature; if learning does not conform to laboratory principles, it is a clear indication that the arbitrary situation does not yield general principles. Each of these questions will be taken up in turn, not to determine their answers so much as to illustrate their significance.

On Units of Behavior

Throughout this review, we have contrasted learning that occurs in the artificial laboratory setting with learning that occurs in the natural setting. However, the meaning of this distinction between artificial and natural has not been precisely formulated. Skinner (1969) has explicitly criticized the ethologists' position that the laboratory is not like real life: "In any case, behavior in a natural habitat would have no special claim to genuineness. What an organism does is a fact about that organism regardless of the conditions under which it does it. A behavioral process is none the less real for being exhibited in an arbitrary setting." (Skinner, 1969, p. 191).

Of course, Skinner is correct. The laboratory is very real for the individuals involved. The major issue is not the reality status of the laboratory. Rather, it centers on precisely what it is that the organism *does* in the laboratory. And what the organism does is determined by the experimenter. The experimenter defines a behavioral unit (*e.g.,* bar press), and typically it is a unit that does not already exist in the organism's repertoire. Subsequently, the factors that contribute to the acquisition and maintenance of this behavioral unit are analyzed experimentally. How are behavioral units defined?

To avoid botanizing, Skinner defined the behavioral unit—the operant—with an eye toward functional utility. In particular, Skinner saw the difficulty in attempting to identify behavioral units independent of the environmental context, and he thus emphasized the functional relations between behavior and environment (Skinner, 1935). A response class, or operant, was defined on the basis of those properties on which reinforcement was dependent. Such a functional definition was successful if "the entity which it describes gives smooth curves for the dynamic laws of the reflex," (Skinner, 1938, p. 37).

Schick (1971) discussed some logical problems in Skinner's definition of operants. He has shown that the definitions of operants and reinforcers are interdependent, and has suggested that they can be defined only relatively, in pairs (*e.g.,* key peck for food, bar press for water, *etc.*). The way to make definitions less context dependent, according to Schick, is to require that a class of responses be labelled an operant only if it satisfies the criterion of orderliness (Skinner, 1938) in combination with any reinforcer. Thus, a demonstration of transituationality is necessary to eliminate a logical problem with the definition of operants (*cf.* Meehl, 1950, on the apparent circularity of the law of effect. It is worth noting that Meehl reached a similar conclusion in his discussion of a different, but closely related, matter—the identification of reinforcers). It is just this concern for transituationality that is behind the "equipotentiality premise"—the point of departure in Seligman and Hager's book. It amounts to identifying classes of events labelled stimuli, operants, and reinforcers, and showing that any member of a class can be interchanged with another member without dramatically affecting the phenomena one observes. This interchangeability is presumably what we mean when we call our experimental situation "arbitrary", and it is just this assumption that is challenged by the concept of preparedness and the phenomena that give rise to it. Stimuli effective in one situation will not necessarily be effective in others. Reinforcers that control one operant will not necessarily control others. Interchangeability may be the exception rather than the rule. Thus, the very definitional system that specifies the rules for the synthesis of behavioral units rests on somewhat uncertain ground.

Despite this logical problem, Skinner's definitional system has been enormously successful. Many different operants have been studied in the laboratory, and have satisfied the criterion of orderliness. However, it must be emphasized that before any experimental analysis can occur, there must first occur experimental synthesis. Our methods first create the behavior we study, and only second, analyze it. One can observe the synthesis of an operant as it develops. An organism enters the experimental situation with a certain genetic and experiential character. Pre-organized (either by the genes or by experience) units of behavior already exist. The arbitrary designation of an operant (*e.g.*, bar press) cuts across pre-established behavioral units, and creates a new one. Bar pressing is extremely variable in topography early in training. It seems apparent that many bar presses are accidental consequences of the active organism moving in tune to its own drummer. Gradually, as the law of effect exerts its influence, the variability decreases, an efficient topography develops, experimental control is established, and with it, a new unit of behavior. Indeed, some investigators (Staddon and Simmelhag, 1971) have recently argued that the fundamental characteristic of reinforcement is not that it strengthens behavior, but that it reduces variability—it selects certain features of behavior, molding them into a new behavioral unit, and excludes others. Said another way, contingent reinforcement produces smooth curves by its very nature, no matter how the operant is defined.

Thus, the law of effect may create new units of behavior in the laboratory. If so, it is not surprising that ethologists, who are interested primarily in understanding the structure of behavior, should balk at methods that impose a new structure rather than analyze the already existing one. Does this reflect a valid criticism of our methodology? I think not. It seems possible that while some behaviors in the natural environment are intrinsically organized, others are organized on the basis of experience, and more specifically, by the very nature of their operant relation to reinforcement. The study of arbitrary operants in the laboratory may yield general principles that underlie the organization of behavior by experience. The operant is thus an artificial synthesis that in fact reflects a naturalistic one, and one which is especially critical to the understanding of human behavior. The study of the law of effect captures the important possibility that behaviors are organized, or defined, in natural circumstances, on the basis of those properties on which reinforcement depends. The reinforcing agents of a culture may have a determining influence (just as the experimenter does) in the manner in which the behavior of members of that culture will be organized. Thus, it turns out that ethology and the experimental analysis are both studies of the structure of behavior: the former is concerned with genetically determined structures; the latter, with experientially determined structures. The key problem lies in understanding how genetically and experientially determined structures interact. Experimental institutions of structure sometimes interfere with genetically determined ones. When this occurs, the resulting output does not yield smooth curves. Thus, one observes "instinctive drift" in unexpected situations (Breland and Breland, 1961), key pecking in the face of negative consequences (Williams and Williams, 1969), failure to maintain rigid postures for contingent reinforcement (Blough, 1958) and so on. Indeed, a number of the studies included in the Seligman and Hager book may be viewed as instances of intrusion of genetic structures in the effort to create experimental ones. However, there are undoubtedly situations in which the two types of structure facilitate one another and situations in which they do not interact at all. The concept of preparedness may ultimately be an heuristic for organizing the relations between genetic and experiential behavioral structures that future research uncovers.

Is the Box Arbitrary?

One of the messages of the Seligman and Hager book is that there are instances of prepared learning even in experimental situations that were explicitly designed to be artificial and arbitrary. It is possible that researchers, guided by the "equipotentiality premise", have endeavored to create experimental situations that would neutralize species-specific contributions to learning, but that they have, in general, failed. The phenomena observed in the conditioning chamber may be as much an illustration of preparedness as phenomena observed in nature. Indeed, the major import of Breland and Breland's (1961) dramatic observations of "instinctive drift", which overrode environmental contingencies is not that species-specific behavior patterns exist, but that they intrude whenever they are allowed to, even in the most artificial situations.

The papers in the Seligman and Hager book that speak most directly to the question of whether the box is arbitrary are the discussions of avoidance by Bolles (1970), and the demonstrations of autoshaping and negative automaintenance by Brown and Jenkins (1968) and Williams and Williams (1969). Bolles argues that the effects of the many variables that have been explored and determined to influence avoidance learning are tempered by the relation of the required response to the organism's built-in defensive repertoire. This suggests a model in which avoidance learning is the product of an interaction among the standard variables, the required response, and the biological propensities of the organism. Experimental manipulations will be effective in varying degrees as a function of the other terms in the interaction. The critical point is that all three terms are important in all avoidance situations, but the relative importance of each changes from one situation to the next. Thus, the variables that have been identified as critical in arbitrary situations will also be significan in non-arbitrary ones.

The need for an interactionist view is even more clear cut with regard to autoshaping. Research by Brown and Jenkins, Williams and Williams, and others not included in the Seligman and Hager book has clearly indicated that there is a relation between pecking, food, and visual stimuli which is well established in the pigeon before any experimental intervention. When conditions are arranged appropriately, one can observe this relation exert a greater influence on the control of key pecking than the law of effect. There is no question that the acquisition of key pecking for food is prepared. However, under most experimental conditions, the influence of the preparedness of the key peck on the phenomena one observes is very difficult to discern. Little distinguishes key pecking as an operant from bar pressing in rats under most schedules of reinforcement, or most discrimination learning procedures, despite the fact that key pecking is prepared and bar pressing is presumably unprepared. One might deal with these similarities between key pecking and bar pressing by asserting that the concept of preparedness refers only to processes of behavioral acquisition, and not to processes of behavioral maintenance. That two behaviors that are differently prepared for acquisition are similarly affected by maintenance conditions (*e.g.*, schedules of reinforcement) is interesting, but not particularly problematic. However, the matter cannot be resolved with a simple distinction between acquisition and maintenance. On some maintenance procedures, like the differential-reinforcement-of-low-rate (DRL) schedule, bar pressing and key pecking are not similarly affected (Hemmes, 1970; Schwartz and Williams, 1971). Similarly, on some discrimination learning procedures—those that produce behavioral contrast—bar pressing and key pecking are not similarly affected (Gamzu and Schwartz, 1973; Rachlin, 1973). Thus, the prepared key peck and the unprepared bar press can be treated neither as wholly distinct nor as wholly similar. Some variables influence them identically, while others influence them quite differently. Only an understanding of the interaction of biological and environmental contributions to key pecking and bar pressing will enable us to understand both the differences and the similarities.

It thus appears (though a good deal of additional research is needed to assert it with some force) that many of the seemingly arbitrary situations we have studied have not, in fact, been arbitrary—that any experimental situation has the potential to allow intrusion of species-specific behavior patterns. However, it also appears that the mere fact that a situation is non-arbitrary does not imply that the principles it yields will not generalize to other situations. In the case of both avoidance learning and key pecking for food, the central problem lies in understanding just which features of the phenomena are attributable to general principles and which are attributable to situation-specific ones. At this time, it seems virtually impossible to solve this problem, or more generally, to make *a priori* determination of which situations will generate which species-specific behaviors. We are forced either to make educated guesses, or to await the discovery of phenomena that do not fit our theories, and which demand an assessment of potential biological influences.

Is any Learning Arbitrary?

At the heart of Seligman and Hager's book is the notion that while laboratory procedures are designed to study arbitrary associations among events, learning under natural conbitions is rarely, if ever, arbitrary. The strategy of developing arbitrary situations was adopted expressly to neutralize the specific biological characteristics of the organism under investigation, and thus ensure generality. However, if the phenomena of learning are critically intertwined with the biology of organisms, then the study of arbitrary associations essentially creates a phenomenon, rather than analyzing one. Lorenz (1965) made the argument most forcefully. He suggested that if learning, like genetic mutation, occurred arbitrarily, the overwhelming majority of experiential influences would be maladaptive. Living organisms, like racing cars, are very finely tuned machines. If one were blindly to take a wrench and a screwdriver to a racing car engine, the resulting modification would be most unlikely to be an improvement. Similarly, if one were to "benefit" from experience blindly, the resulting modification would rarely be an improvement. Thus, Lorenz argues that learning must be non-arbitrary. The genome incorporates a kind of blueprint that very rigidly constrains when an organism will learn, what an organism will learn, and how an organism will learn. In this way, the chances that environmental modification of behavior will be adaptive are greatly enhanced. The implication of Lorenz's view is that the phenomena of learning *per se* are only the tip of the iceberg. The real key to understanding behavior comes from understanding the genetic blueprint. Furthermore, to the extent that learning *is* important, it must be studied in its natural context. Efforts to abstract learning processes must result in their distortion.

The Seligman and Hager book is full of examples of non-arbitrary learning in the natural environment, especially the impressive studies of bird migration (Emlen, 1970), bird song (Marler, 1970), and imprinting (Gottlieb, 1965). However, the question we are asking is not whether non-arbitrary learning ever occurs under naturalistic conditions, but whether it always occurs, as Lorenz suggests. To what extent is learning ever arbitrary in the natural environment?

It is by now a truism that organismal flexibility increases with phylogenetic complexity, and with development. Simpler organisms profit less from experience than more complex ones. Similarly, infantile behavior is far more rigid than adult behavior. What this suggests is that the significance of prepared learning may vary with the species and developmental stage of the organism under investigation. The capacity for and frequency of arbitrary learning may increase with increasing species complexity and developmental progression. It seems clear that most of the learning done by human adults is arbitrary, or at least largely independent of evolutionary constraints. Learning to drive a car, to swim, to use statistics, to play bridge, or chess, or golf, hardly seem closely related to any biological prewiring. On the other hand, the learning in infants of visually guided reaching, face recognition, social attachments, and language seem more obviously connected to a genetic program. Similarly, what pigeons, rats, or worms may learn seems a good deal less flexible than what chimpanzees and people may learn. Indeed, the phenomenon of play, which characterizes primarily the higher primates, has been considered an important influence on the development of what might be called creative intelligence (Bruner, 1972; Sutton-Smith, 1966). The opportunity play provides for seemingly unlimited arbitrary combinations and recombinations of behaviors and stimulus inputs may provide the source from which later, goal-directed behaviors arise. Therefore, it may be that the learning of associations among arbitrary events is characteristic of relatively adult, complex organisms, and that the concept of preparedness carries fewer and fewer implications as the species under consideration is increasingly advanced.

If this is true, it raises an interesting and paradoxical possibility. Researchers in general are ultimately interested in understanding human behavior. To do so, they study simple organisms in artificial (biologically neutral) situations. They develop the laws of unprepared learning. However, the learning that characterizes these organisms in the natural environment may well be largely prepared, and governed by different laws than those of unprepared learning. Thus, the general principles obtained in the laboratory may not apply to the species under study. However, these principles may well apply to man. It is odd, but perhaps reassuring, to think that by studying the behavior of pigeons, in arbitrary situations, one learns nothing about the principles that govern the behavior of pigeons in nature, but a good deal about the principles that govern the behavior of people.

The ideas put forth in this section may be summarized as follows:

1. Some behaviors are intrinsically organized while others are organized on the basis of experience. The synthesis of behavioral units in the operant conditioning chamber may mirror the synthesis of behavioral units under natural conditions.
2. The experimental chamber is sometimes free of species-specific influence and sometimes not. In any case, principles derived from arbitrary situations may have an influence in nonarbitrary ones.
3. The degree to which naturalistic learning is arbitrary probably increases directly with species complexity and developmental progression.
4. The study of lower organisms in *nonarbitrary* situations may yield principles that accurately describe the behavior of that species in nature but do not generalize to more complex species.
5. The study of lower organisms in *arbitrary* situations may yield principles that do not accurately describe the behavior of that species in nature, but that do generalize to more complex species.

Conclusion

The import of the theory of preparedness, and the phenomena on which it is based, is that they limit the generality of laboratory principles. They do, indeed, begin to set the biological boundaries of learning. They force researchers to examine the biological organization of behavior, especially as it constrains the experiential organization of behavior. These developments are wholly salutary. They will ultimately result in rules of induction and inclusion that will make the experimental analysis of behavior more comprehensive, and make attempts at extra-experimental extrapolation more meaningful, even as they are more restrained.

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