EDITOR'S NOTE

The editor is of the opinion that readers of the Journal may find the following report on behavioral and biological issues in the learning paradigm of interest. It summarizes the views of a number of experts in both behavioral and biological aspects of learning on general issues of control and definition, as expressed in a conference held at the Social Science Research Council in December 1973.

The reader will find the report remarkably free of data content—this was the express intent of the organizers. However, it is not free of substance. In the editor's opinion, it seems to summarize the current state of the field, both in areas of agreement and areas where behaviorists and biologists may not yet be in complete accord.

Readers who wish to express their own views on these issues may do so in letters to the editor.

Richard F. Thompson

Behavioral and biological issues in the learning paradigm

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The following is a report of the activities of the workshop on Behavioral and Physiological Definitions of Learning held in New York City in December of 1973. The participants are listed below. The workshop was conducted under the auspices of the Committee on Biological Bases of Social Behavior of the Social Science Research Council and was made possible by grants from the National Science Foundation and the National Institute of Mental Health. Workshop organizers were Richard F. Thompson, Harvard University and David Jenness, Social Science Research Council.

The workshop was concerned with several fundamental problems and issues surrounding the learning paradigm. Learning is one of the most widely familiar and fundamentally important topics in the area of behavioral study. Scientists in many disciplines, ranging through physics, chemistry, and physiology to psychology and the social sciences all must deal with the concept of learning.

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Learning has no specific definition. Broadly construed, it means change in behavior as a result of experience. However, particular forms or procedures of learning, such as classical and instrumental conditioning, have operational definitions involving the use of appropriate control conditions. Herein lies a critical issue, and an issue on which much of the activity of this workshop was focused. Considerable time was spent on problems relating to definitions of learning, both in terms of the operations that give rise to the phenomena and in terms of the necessary and sufficient controls for identifying the phenomena.

The workshop brought together prominent scientists from two related areas, the psychology of learning and the biological bases of learning. The workshop emphasized problems of definition, control, and interpretation, rather than more substantive matters relating to the vast data on learning and the biology of learning.

The questions to which the workshop participants addressed themselves were many and diverse. Among them were issues relating to the possibility of constructing a small set of paradigms that characterize learning and a discussion of the proper controls for each of these paradigms. Discussion was devoted to the

relations between the various paradigms and to what extent these definitions or relations might be modified, given more information about the biology of learning. Each major participant led a discussion on particular areas of his concern and made a summary statement at the end of the workshop. In this report, we have summarized the major points made by each of the participants. This is not an exhaustive discussion of all of the points raised by all of the participants. Rather, we have attempted to incorporate the flavor and the import of each participant's statements.

Dr. I. Gormezano
University of Iowa

Gormezano, starting with a consideration of Pavlov, briefly reviewed the development of law of effect accounts of classical conditioning. Gormezano stated that the adaptiveness of behavior did not occupy an explanatory role in Pavlov's analysis of conditioned responses (CRs), since Pavlov saw his task as being one in which the adaptiveness of behavior was to be explained. For Pavlov, the apparent adaptiveness of CRs were not determined by their consequences for the individual organism, but were the consequence of cerebral mechanisms that evolved from the organizational structure of the hemispheres determined by evolutionary selection processes. Unfortunately, Pavlov often employed a system of classification of "alimentary" and "defensive" salivary reflexes based upon what would appear to be their local adaptive consequences for the organism. Yet, the classification was but a *descriptive* shorthand for his belief that these behaviors were the product of cerebral mechanisms determined by phylogenetic selection. Nevertheless, many American psychologists took from Pavlov only his scattered descriptive comments to support "law of effect" formulations which placed the adaptive consequences of conditioned responses in a central *explanatory* role. In so doing, these formulations ignored Pavlov's effort to provide, via his contiguity principles of conditioning, a mechanistic (neural) model for the pervasive biological phenomenon of adaptiveness. In its most general form, law of effect accounts hold that CR-UCS overlap provides the source of reinforcement of CRs through its attenuation of the noxiousness of an aversive UCS or enhancement of the "attractiveness" of an appetitive UCS. Gormezano then discussed several approaches he has employed to assess law of effect accounts. Some of these means included: development of the rabbit nictitating membrane preparation to circumvent the notion of "voluntary responses" (with its implication of local adaptive consequences) in human eyelid conditioning, use of UCS intensity increments or decrements dependent upon CR occurrence, and yoked-control designs to assess the consequences of

CR-contingent omission of the UCS.

Gormezano then proceeded to a consideration of the appropriateness of yoked-control designs in the assessment of law of effect and contiguity accounts of classical conditioning. Gormezano noted that Church elaborated a general criticism of yoked controls on the grounds that individual differences in reactivity to the reinforcing event can produce a bias which favors the experimental (contingency) group over the yoked control, even when there is no effect *per se* of the response-reinforcement contingency. Gormezano pointed out that while this argument is true when there is a positive contingency between the response and reinforcing event, it does not hold for yoked-control designs in which the response precludes the occurrence of the reinforcing UCS event (i.e., omission paradigms). Nevertheless, Gormezano indicated that Church's schema can lead to the recognition that yoked-control designs do not provide an unambiguous means for assessing law of effect or contiguity accounts of avoidance conditioning. The interpretive problem is occasioned by the fact that not only is the UCS omitted whenever the contingent subject makes a CR (i.e., the contingency of interest to law of effect accounts), but by the fact that the UCS is presented whenever the contingent subject fails to make a CR (i.e., the contingency of interest to law of effect accounts). Accordingly, since both aspects define the contingency differential holding between avoidance and yoked-classical control subjects, either one of these factors (or both) may be responsible for the superiority of avoidance subjects. Gormezano indicated that when he has employed the yoked-control design, he has interpreted the avoidance superiority as due to the contingency differential favoring contiguity accounts. Moreover, Gormezano indicated that it is rarely the case that rejection of the null hypothesis provides unique support for only one alternative hypothesis. Hence, the yoked-control design is not unique in that limitation since one's theoretical viewpoint must be taken into account in considering the methodological adequacy of any control procedure. It was generally agreed that interpretation is the important part of any design and that theoretical biases and limitations must be understood.

In response to Kandel's query as to whether behaviorists would view long-lasting habituation as interesting or not, Gormezano stated that the retentiveness of the behavior is a very important consideration in such an evaluation. He stated his belief in a "Botany of Plasticity" which progresses from simple behavioral changes involving "limited" retention to more complex behaviors involving relatively "permanent" retention. Accordingly, in such a classification some judgment must be made regarding the time parameters of retention which, in turn, are governed by one's

conception of learning. For Gormezano, relatively "permanent" retention is one of the hallmarks for designating the presence of an associative process. Such considerations are not to say, however, that the simpler forms of plasticity (i.e., those behaviors demonstrating "limited" retention) are not important, since they may well serve as the substrate of the more complex plasticity which psychologists designate as learning.

Dr. E. R. Kandel
Columbia University

Kandel began his talk by reviewing three types of studies that have examined synaptic plasticity in simple systems. The first type of study examined the cellular alterations produced by simple stimulus patterns such as use or disuse. This work led to finding of differences in the plastic capabilities of chemical and electrical synapses and showed that the relationship between usage and synaptic efficacy is complex with some chemical synapses showing facilitation and others depression. The second type of study used more complex stimulus sequences derived from learning paradigms such as habituation, classical or operant conditioning and applied them to simple nervous systems (or simplified parts of complex nervous systems). The strategy here was to determine if these stimulus patterns produce cellular alterations (neural analogs) that parallel the behavioral alteration produced by the learning paradigms in intact animals. The approach has led to the finding of synaptic decrement as an analog of habituation and heterosynaptic facilitation as an analog of sensitization. Both the first and second approach select neural systems for conveniences without concern for the role of a given cell or a synaptic connection may have in actual behavior. In most cases that role is not known. The third type of study attempts to look at the neural elements that actually control a behavioral response and to examine those elements during the development of a behavioral modification to see what cellular and synaptic change causally mediate the behavioral modification. This approach has provided insights into the actual cellular mechanisms of simple instances of habituation and sensitization.

It was Kandel's contention, not shared by Thompson, that in all three cases the vocabulary used to describe the alterations seen at a cellular level should be distinguished from the vocabulary used to describe alterations in the behavior of an intact organism. Even if a cellular response showed all the features associated with behavioral habituation, Kandel argued the cellular alteration is not habituation and should not be called that. Rather the cellular change should be referred to by its mechanisms or its properties: excitatory synaptic decrement, inhibitory recruitment, etc. This would avoid the difficulties inherent in (incorrectly) applying

behavioral terms to physiological processes. Habituation is an animal process much like walking and talking. One would certainly not say that a cellular change is walking. Why then should it be habituating? The fact that we sometimes speak *colloquially* about the "behavior" of cells should not mislead us into thinking that this is logically equivalent to the "behavior" of an organism. Unless the definition of behavior is to become meaningless, it should be restricted to the movement of muscles, secretion of glands, etc.

The tendency to apply behavioral terms to cellular processes is unfortunate even when the behavioral function of the cell is known. It is worse, as is usually the case, when there is no demonstrable relationship between overt behavior and cellular processes being examined. Here one could be in the position of calling a synaptic change "habituation at the cellular level" without knowing what the cell actually contributes (if anything) to the behavior or its habituation. Thus, it is possible that a change called habituation at the cellular level might ultimately be shown to be directly involved in producing reflex sensitization! It was Kandel's contention that the rational development of behavioral psychology and neurobiology and their merger require that critical distinctions at different levels of discourse be maintained.

Dr. R. Rescorla
Yale University

Rescorla emphasized the need to distinguish between operational and theoretical levels of discourse in discussing learning. He illustrated the need to keep that elementary distinction in mind by reference to two aspects of learning experiments: control procedures and behavioral outcomes from which learning is inferred.

He noted that we ask control procedures to accomplish two quite different things. One goal is operational: to identify a particular operation as responsible for a particular change in behavior. For instance, in Pavlovian conditioning, we may ask whether pairing two events is responsible for change. A second goal is theoretical: to identify the presence of particular theoretical processes, such as "learning" or "associations." The first goal is relatively easily accomplished, and there are available various control procedures for any of various operations of interest. The second goal is more difficult and involves the commitment to a particular theory in which terms like "association" or "habit strength" are embedded. Within any particular theory, one can control for the presence of such processes; but a control procedure appropriate to one theory may not apply to another. Thus, Rescorla agreed with Gormezano that one's theoretical position will determine his choice of control procedures.

Failure to keep the operational and theoretical levels of discourse separate has generated considerable confusion about the selection of appropriate controls for learning. For instance, the truly random control procedure aids in evaluating the operation of CS/US contingency. Within some theories it also controls for the occurrence of "associations" but within other theories the application of that "control" procedure is hypothesized to generate "learning." One can never attain the perfect control for all possible theories; he can hope to determine the importance of an operation.

The level of discourse chosen is also important in deciding which behavior changes qualify as evidence of learning. If the question is whether a particular operation differentially affects the organism, then any differences in behavior should be sufficient to conclude positively. Any way in which the animal who has been subjected to an operation such as CS/US pairing differs from one not so treated is evidence that the operation modified the organism. There is no *a priori* reason to restrict the inference to cases where the CR is like the UR or even cases of behavior observed during the presence of the CS. Indeed, from an operational viewpoint, one could argue that if a dog's tail grew every time a CS was paired with food but not upon the administration of control procedures, we would have evidence of his sensitivity to that pairing operation. Precisely such changes in neural tissue constitute the data of many interested in the neurophysiology of learning.

Where we might want to place more restrictions upon usable experimental outcomes is in the step of imposing a theory upon the behavior. For instance, a particular theory of conditioning may identify "learning" as the attachment of the UR to the CS. Within such a theory, "conditioning" would occur only to the extent that the response to the CS looked like that to the US. Other behaviors would not reflect "conditioning," although they might reflect other "learning" entities within the theory. But again notice that the presence of "learning" is a decision to be made within a theory.

Rescorla emphasized that one level of discourse was not necessarily to be preferred to the other. Rather, the point is that the levels must be kept separate if we are to avoid confusion and the posing of meaningless questions.

Dr. C. D. Woody
University of California, Los Angeles

Woody began his remarks by saying that he agreed with Rescorla's statements regarding the use of controls in testing operations and in testing theories as being two different things. In fact, Woody has come to much the same conclusion independently. Woody felt that it is very difficult to form independent definitions for various psychological terms, and thus it is often

dangerous to try to work with a theoretical construct such as learning *a priori* due to the multiple definitions for the term. As a physiologist, Woody feels he is interested in interactions between cells, synaptic activities, and so forth. It is possible to talk about physiological minutiae of cellular operations just as it is to talk about psychological minutiae. In fact, there are several sets of such problems which can be discussed in relation to learning. However, as a physiologist interested in learning, one must look at each set of operational and/or observable variables and try to make correlations between sets of input and output variables. In fact, such sets must be constructed for the total input, intervening, and output variables and these are the important tasks remaining to be done. In this regard, for example, it must be recognized that most stimuli will produce responses in motor output regions of the brain; an auditory stimulus will produce neural responses in the facial nucleus of the brain; although no behavioral output. Thus, the neutral stimulus comes very close to producing a behavioral response. We must also realize that the response, when it is elicited behaviorally is in a very rich context, a total movement, and is simply the end of a line of nerve potentials, cell body responses, and so forth. The response then has many different levels in the total organizing system. One can thus look at the response in any of several ways, depending upon where in the system one observes. To a physiologist then it becomes dangerous to try to classify behavior in terms of psychological concepts. It may be best to realize that one must be able to define the stimuli and the responses and look at the consequences of the stimuli on the system. Woody also discussed his own work and the relationship of his paradigm to alpha responses.

Dr. R. M. Church
Brown University

Church focused his remarks on the question of what constitutes appropriate control procedures to demonstrate that a change in performance should be called "learning." He argued that no one control procedure can insure against artifactual results.

As an example, Church criticized the use of a yoked control in experiments on avoidance in the headless roach. He pointed out that a model based on purely reactive processes could account for the basic results. One need only assume that the leg of a headless roach withdraws when shocked and that the withdrawn leg eventually lowers again after some variable interval of time. It will sometimes happen that the yoked control animal will lower its leg before the experimental animal, and will leave its leg down until the experimental animal lowers its leg and both are shocked. This effect is sufficient to predict that the experimental animal will

have its leg raised more of the time than the yoked animal.

This model is too simple to account for gradual development of the "avoidance." Church noted that adding a fatigue-like factor would accomplish this, because the model predicts that the experimental animal would raise its leg more frequently than the yoked animal. If other effects need to be accounted for, more factors can be added to the model. The yoked control, therefore, cannot rule out a purely reactive model of the behavioral change. Indeed, in no experiment can the yoked control *by itself* answer such an argument.

Church went on to show that, logically, no control procedure exists that controls for the distribution of one and only one temporal interval. In experiments on classical conditioning, one would wish to vary only the distribution of CS-US intervals, holding the distributions of CS-US, US-US, and US-CS intervals constant. In experiments on instrumental conditioning, one would wish to vary only the distribution of response-reinforcer intervals, holding the distributions of response-response, reinforcer-reinforcer, and reinforcer-response intervals constant. In both types of experiment, however, the distributions of the two events in time is completely specified by any two of the distributions of component intervals—i.e., there are only two degrees of freedom. Since the number of degrees of freedom is always fewer than the number of component parameters, it is impossible to vary only one component parameter. Since at least two parameters must vary from experimental to control procedure, the number of control procedures necessary to isolate the effect of one parameter must always be greater than one.

Church agreed with Herrnstein that the potential number of control procedures is infinite. He concluded that experimenters must abandon the search for an ultimate control procedure that answers all objections. Instead, they should take a more pragmatic attitude that will allow them to do the controls that seem appropriate according to reasonable suppositions.

Dr. J. L. McGaugh
University of California, Irvine

McGaugh explained that the "double-lens model" advanced by Brunswik and Tolman might be applied to an analysis of input to and output from the nervous system. On the side of input, reliable percepts, such as distance of an object, derive from several cues, each one of which fails to be reliable by itself. The aggregate of unreliable determinants combines within the organism to produce a reliable judgment or percept. Symmetrically, on the side of output, unreliable means lead to a reliable end. The motor accomplishment, be it depression of a lever, movement through space, or simply extension of a limb, may be achieved by various combinations of units

(neural and muscular) on various occasions. Although the accomplishment is reliable, the units that participate in it may be unreliable. The further one's instruments move from recording the accomplishment into the nervous system to recording the participating units, the less reliable may the correlations between nervous action and accomplishment become.

In conclusion, McGaugh suggested that the psychologists abandon the term "learning," because it implies a reliability of neural output that can rarely be observed in practice. It might be replaced with the term "learnings," in analogy to "genetics," to refer to an interesting set of neurobiological problems. The neurophysiologist might then feel freer to pursue neural correlates of various treatments without having to struggle to relate those correlates to the "thing" that is learned. Instead, the neural correlates of treatments would be related to the motor accomplishments of the treatment.

Dr. R. J. Herrnstein
Harvard University

Herrnstein began with the premise that performance reflects two factors: what the organism knows and what the organism wants. The first, knowledge, he supposed to be composed of associations, some acquired and some inborn. The second he thought of as drive or deprivation. To give more substance to the two sets of phenomena, he described some recent work with positive and negative reinforcement.

For positive reinforcement, orderly relations have appeared when response rate is considered as a function of rate of reinforcement. For negative reinforcement, it has proven sufficient to arrange that presses on a lever reduce a rate of randomly occurring electric shocks for leverpressing to be maintained. Again, rates of occurrence seem essential: the rate of leverpressing depends on the reduction in rate of shock that it produces.

Responding in both types of situation has been found to conform to the following equation:

$$P_1 = \frac{k R_1}{\sum_{i=1}^n R_i}$$

where P_1 is the rate of responding, R_1 is either rate of food reinforcement or reduction in rate of electric shock, k is a constant that depends on the nature of the response, and the denominator equals the total of reinforcement (positive and negative) available in the situation. For simplicity, the equation can be rewritten with the denominator broken into two parts:

$$P_1 = \frac{k R_1}{R_1 + R_e}$$

where R_e is the total reinforcement in the situation other than R_1 . The equation indicates that response rate (P_1) varies directly with the reinforcement it produces (R_1) and inversely with the other reinforcement in the situation (R_e).

In terms of the initial dichotomy between knowing and wanting, the equation suggests that the organism has an association between R_1 and P_1 (knowledge), and that the more it values the reinforcer (wants it), the smaller will be R_e relative to R_1 . The association permits the response to occur; the relative size of R_e determines its level of occurrence.

The equation has at least two major implications for neurophysiologists interested in behavior. First, it suggests that rate of occurrence is a fundamental dimension of the events that control behavior. This implies that higher organisms integrate events over time, because a rate of occurrence cannot be assessed at a moment, but only over an interval. Second, it suggests that all behavior involves choice among possible alternative actions, this choice dependent on the values of all the alternatives.

On the subject of appropriate control groups for distinguishing learning from other behavioral changes, Herrnstein pointed out that one cannot experiment without some sort of a theory. Since the number of potential independent variables in any situation is always infinite, and only certain of these are selected as relevant, one always begins with presuppositions about what sort of operations are likely to have effects. By the same token, the number of possible control groups is infinite. An experimenter, therefore, must choose his controls according to his theory. Baum added that the experimenter should try to be explicit about his theory. This would make the logic of the controls apparent.

Dr. W. A. Spencer
Columbia University

Spencer raised the question of whether paradigms related to enduring behavioral modifications can be arranged in a true hierarchy; and if so, whether there is a sufficient gap at any one point in such hierarchy, so that one is justified in making a firm and fundamental distinction between "learning" and "sublearning" paradigms. Spencer suggested that one may arrange paradigms in a hierarchy based on complexity, but that any distinctions between learning and other paradigms is essentially arbitrary and a matter of collective agreement on a classificatory convention.

Spencer made the observation that in the past several decades psychologists conveyed a very important

message to those individuals more biologically oriented. They told them that the phenomena that they were studying were very interesting, but most of them, virtually all of them, exhibited a very short time course—a matter of milliseconds. The psychologists told the biologists that the behavioral changes that were most interesting, and perhaps most meaningful, last considerably longer than this, and the physiologists might be well advised to look at these relatively long-term changes. Spencer sees this as an important suggestion for two reasons: (1) it got physiologists interested in neuronal events showing a longer time course, such as posttetanic potentiation and habituation; and (2) it encouraged the development of a methodology for dealing with long-time-course events. This latter development opened the exploration of pathophysiological changes attending recovery of function following lesions, and developmental questions. Spencer's point was that the salutary impact of the message was related to its simplicity—"just think long-term." In relation to this simple message, the current state of dialogue between learning psychology and behavioral biology has conveyed no single clear-cut message. Behavioral biologists interested in the phenomenon of learning are confronted with considerable confusion regarding categories of learning-like phenomena, as well as a plethora of control procedures that have been developed over the years. The control procedures are generally regarded as being of two types—one simply empirical in describing relevant variables, and the other related to explicit theoretical positions regarding learning.

Spencer felt the need for agreement on a list of "learning criteria" for behavioral biologists interested in studying learning-like phenomena. While this workshop did not provide such a list, a consensus emerged that the simpler paradigms, such as habituation and sensitization, have become increasingly of interest as advances in the cellular physiology of the simpler forms of enduring behavioral alteration have been forthcoming. Spencer feels this trend will continue, and is likely to be extended to more complex associative paradigms. At this time, the behavioral biologist would benefit greatly from a clear statement, however provisional, of "learning criteria," as well as the theoretical and practical applicability of various control procedures.

Spencer presented a possible hierarchy of behaviors ranging from trivial to interesting in terms of complexity and meaning to the organism. He then contrasted the efforts required by learning psychologists and by behavioral biologists in understanding the behaviors located along this continuum. While psychological investigations have had some success in dealing with "interesting" behaviors, the behavioral biologists, using the methods of cellular physiology and biochemistry, have had success in dealing with behaviors that are

located closer to the "trivial" end of the continuum. Spencer hoped that in the future behavioral biologists will attempt to ascend the continuum in their search for the mechanisms underlying behavioral change.

Dr. R. F. Thompson
Harvard University

Thompson felt that the approach as exemplified by Gormezano offers the behavioral biologist clear definitions of what is meant by the term learning, and a clear exposition of the appropriate controls to run. The behavioral biologist interested in understanding the brain mechanisms underlying this behavior finds it very useful to know precisely what controls to run. On the other hand, Thompson supported the view of McGaugh, who stated that we must look at "learnings" rather than "learning." Since learning is not a unitary concept, it is unreasonable to expect that there is a unitary control procedure that would satisfy all forms of behavioral change. For the behavioral biologist interested in inferring the underlying mechanisms of the behavioral change, the choice of appropriate controls is thus determined both by the preparation and by the theoretical vantage point. The critical message that comes out of this conference is that the control procedures one employs are determined by the particular vantage point of the processes under study. Given this kind of an interpretation, there are essentially an infinite number of controls possible. One simple conclusion would be not to run any controls at all. Thompson stated that this, of course, was an extreme position and untenable as such. However, of the infinite number of controls possible to be included in an experiment, some will be very important to run and others will not. A decision must be based both on the empirical properties of the learning and the theoretical vantage point. Given this viewpoint, Thompson felt that there was no compelling need for behavioral biologists to be overly concerned about the utilization of the "one best control procedure." Rather the choice of control procedures will depend upon the phenomenon being examined and questions being asked of the preparation.

In the case of simple forms of behavioral plasticity like habituation, controls seem relatively obvious. However, even for habituation, the particular theoretical approach may influence experiments. There is a common tendency to "explain" other aspects of behavior in terms of habituation. Such usage is in fact theoretical—i.e., "Type 2." Unfortunately, the theoretical properties of inferred Type 2 habituation are seldom described by those who use it as an explanatory construct. Habituation has been defined at the operational or Type 1 level by a set of parametric characteristics. This definition offers the necessary

controls for determining that any form of response decrement, whether behavioral or neuronal, "is" habituation (Type 1). However, to go beyond this and state that a particular neuronal or behavioral process is a mechanism of habituation requires an explicit theoretical formulation of what is meant by inferred habituation.

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