ADAPTIVE BEHAVIOR FROM RANDOM RESPONSE

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Darwin has shown how the seemingly purposeful process of evolution could be explained by the piling up of random variations properly selected. Applied to learning this principle bridges the gap between the ideas based on mechanical principles of behavior and those based on "intelligence" or "purposefulness," much the same way as this gap is being bridged in servo-technology. It is suggested that all existing learning theories contain explicit or implicit assumptions about some selective principle operating on initially random responses, assumptions which Ashby has carefully spelled out and utilized in the construction of his Homeostat.

C AN *habit* be effectively treated as a semi-isolated stable system, à la Ashby (1), or as a low entropy high energy steady state, à la Bertalanffy (2), or as a homeostatic system, after Cannon (5)? Such questions are timely, in that the influence of cybernetics, servo-mechanism models, and general systems theory are suggesting such reformulations to a number of psychologists. But such timeliness is not sufficient to justify the enterprise. In seeking new models in psychology we should be motivated by something other than the desire to pour old wine into new jugs. And in particular when we come to deal with the concept of *habit* special justification seems necessary, inasmuch as we are dealing with the oldest, most respectable, and most adequately handled of psychological concepts. What are some of the difficulties with our present conceptualization of *habit* which tempt us into this new family of models?

Most important for the present discussion has been the difficulty in the definition of the character of the acquired response. Two principal operational alternatives have offered themselves: One of these is to define the response in terms of the body's own coordinates, in terms of muscle contractions or movements. The second alternative, equally

operationally specifiable, has been to define the learned response in terms of a change in the relationship between organism and environment. Responses defined in these terms become objects moved, places reached, regions entered, etc. It is characteristic of the great bulk of research on habit that these two alternative definitions of the learned response have been confounded. That is, no operations have been provided in the experimental situation which would distinguish between these two interpretations, since learning is continually tested in the identical situation in which the habit was acquired. This has given learning theorists the possibility of overlooking the existence of the alternatives, and of arbitrarily assuming one of the definitions in the process of building their theories. In the few experimental studies in which these two interpretations have been operationally opposed, in which through transposition experiments two interpretations have been unconfounded, the evidence is predominantly but not entirely in favor of the second interpretation (4, 18). That is, the learned response is to be essentially defined in terms of a shift in the organism-environment relationship rather than a motor response defined in terms of organism or body parameters alone. The most popular and best developed learning theories have ignored this fact and have gone ahead assuming the adequacy of

defining response in isolated motor or muscle terms, even when using molar data collection units.

A major reason for this resistance is that accepting the validity of the second definition of response presents the learning theorist with a much more difficult problem, a problem which cannot readily be encompassed by the anatomically reinforced conceptualization of the simple reflex arc, with discrete afferent and efferent neural transmission pathways. A second reason for resistance lies in the fact that those who, like Tolman (23) and Lashley (14), have pointed out the adaptive, object-consistent nature of learned responses, have not presented plausible mechanistic theories of how such responses could be achieved, and have used language implying purpose, thus raising the ghost of metaphysical issues which had been important at the time when psychology was freeing itself from philosophy. Recent attention to the engineers' achievement of self-regulating machines, the behavior of which is consistent to uniform ends achieved through variable intermediate steps (as in the thermostat and governor at the most simple level and as in the aiming and tracking devices for anti-aircraft artillery at a more complicated level), has supplied a mechanistic model of adaptive, habitual response mechanisms. While we cannot say that adopting a steady state or homeostatic model will solve at the psychological or physiological level the problem of how such responses are achieved by the organism, it does at least give a deterministic model for facing squarely one of the toughest aspects of the actual phenomena of habit.

In a recent book, Ashby (1) has proposed the minimum elements for a machine which would show adaptive behavior. As indicated in his title *Design for a Brain* it is his explicit intent to develop such a model for the furtherance of the understanding of the adaptive behavior of living organisms. While it is not the purpose of this paper to evaluate the over-all success of his achievement or its total relevance for psychology, the assignment which he gives himself is an important one, and directly pertinent to our problem.

How does the brain produce adaptive behaviour? In attempting to answer the question, scientists have discovered two sets of facts and have had some difficulty in reconciling them. The physiologists have shown in a variety of ways how closely the brain resembles a machine: in its dependence upon chemical reactions, in its dependence on the integrity of anatomical paths, and in many other ways. At the same time psychologists have established beyond a doubt that the living organism, whether human or lower, can produce behaviour of a type called 'purposeful' or 'intelligent' or 'adaptive'; for though these words are difficult to define with precision, no one doubts that they refer to a real characteristic of behaviour. These two characteristics of the brain's behaviour have proved difficult to reconcile and some workers have gone so far as to declare them incompatible.

Such a point of view will not be taken here. I hope to show that a system can be both mechanistic in nature and yet produce behaviour that is adaptive. I hope to show that the essential difference between the brain and any machine yet made is that the brain makes extensive use of a principle hitherto little used in machines. I hope to show that by the use of this principle a machine's behaviour may be made as adaptive as we please, and that the principle may be capable of explaining even the adaptiveness of Man (1, p. 1).

As a first part of his enterprise, Ashby has built a machine which he calls the "homeostat." While this machine is but a fragment of his total model it nonetheless manifests certain adaptive or problem-solving characteristics, constituting what Ashby calls an ultrastable system. It is one feature of this system, repeated in his full model, to which attention is turned in the present paper.

When these parameters are given a definite set of values, the magnets show some definite pattern of behaviour; for the parameters determine the field and thus the lines of behaviour. If the field is stable, the four magnets move to the central position, where they actively resist any attempt to displace them. If displaced, a co-ordinated activity brings them back to the centre. Other parameter-settings may, however, give instability; in which case a 'runaway' occurs and the magnets diverge from the central position with increasing velocity ... when, and only when, the magnet M diverges far from the central position ... the coil G of each uniselector is energised ...; for only at extreme divergence does the output-current reach a value sufficient to energise the relay F which closes the coil-circuit. A separate device ... interrupts the coil-circuit regularly, making the uniselector move from position to position as long as F is energised. ... The uni-

selector (or 'stepping-switch') U . . . [controls the parameter settings]. . . . The values of the components in U were deliberately randomised by taking the actual numerical values from Fisher and Yates' Table of Random Numbers. Once built on to the uniselectors, the values of these parameters are determined at any moment by the positions of the uniselectors. Twenty-five positions on each of four uniselectors (one to each unit) provide 390,625 combinations of parameter-values. [Thus eventually a setting is achieved which returns the magnet to its central position, inactivating relay F, and the uniselector is therefore left at that setting.] (1, p. 96).

It is not anticipated that this brief sample from Ashby will give the reader an understanding of his machine and the requirements he sets for it. It is doubtful that such understanding can be provided more economically than Ashby himself has done. But the quotation may be adequate to convey to the reader the essential gimmick in the machine which makes it adaptive. When the machine is under stress, this stress sets into operation a mechanism which emits randomly various trial settings or behaviors until a setting occurs which removes the stress and thereby interrupts the emitting of further settings.

To one who has picked up Ashby because of his promise to deal with "purposeful" or "intelligent" or "adaptive" behavior, encountering this arbitrary and random little gadget may come as something of a letdown. One might be tempted to apply Kurt Lewin's criticism of G. E. Muller's associationism:

The explanation, following closely Darwinistic ideas of directed actions as the result of factors which do not contain the concept of directedness, was considered a particularly important achievement because at the time the controversy between the teleological and causal explanations of behavior was acute. The outstanding characteristic of a scientific causal explanation was incorrectly seen as requiring the avoidance of the concept of direction. This view was held in spite of the fact that one of the basic concepts in physics, the concept of physical force, refers to a directed entity (a vector, in terms of mathematics) (16, p. 66).

But because Lewin may have underestimated the confusion remaining in psychology on issues of teleology and conscious purpose, and because the Darwinian conceptual achievement in dealing with adaptation on the phylum and species level is not one to be despised, we may be justified giving Ashby further consideration. He himself recognizes the Darwinian roots of his model:

The work also in a sense develops a theory of the 'natural selection' of behaviour-pattern. Just as, in the species, the truism that the dead cannot breed implies that there is a fundamental tendency for the successful to replace the unsuccessful, so in the nervous system does the truism that the unstable tends to destroy itself imply that there is a fundamental tendency for the stable to replace the unstable. Just as the gene-pattern, in its encounters with the environment, tends towards ever better adaptation of the inherited form and function, so does a system of step- and part-functions tend toward ever better adaptation of learned behaviour (1, p. vi).

If indeed the engineer attacking the problem Ashby has set himself has no alternative solution, and Ashby would claim that this is essentially so, then the device in question and its psychological parallels seem worthy of being taken quite seriously by the psychologist. Furthermore, it turns out upon inspection that most major psychological theories embody such a mechanism, and that Ashby's approach, if nothing else, may serve to point out one major point of agreement among our competing schools of learning. To make this assertion it is necessary to modify somewhat the demand for perfect randomness, and indeed, this is not essential to Ashby's argument. The essential mechanism becomes one of continual variation of behavior in the face of continued unresolved motivation. Randomness, or better, blindness, remains in that although the responses may have biased initial likelihoods of being used, the successive efforts or trials do *not* have the character of "corrections" of previous trials. Thus while eventually a more accurate or adaptive response is achieved by chance, there is no relationship between the direction of miss on one trial and the character of the next.

Thorndike (20) and Hull (11) enter their organism into the situation with an initial

family of divergent responses associated to the situation in varying strength.

$$S_1 \begin{cases} \dfrac{p. = .05}{} R_a \\ \dfrac{p. = .45}{} R_b \\ \dfrac{p. = .15}{} R_c \\ \dfrac{p. = .20}{} R_d \end{cases}$$

If R_b does not result in reinforcement, its likelihood of occurrence is successively diminished until the next strongest response, R_d, occurs, and so on until a reinforced response (possibly, though not necessarily, one which eliminates S_1,) occurs. The overt behavior described is compatible with that of Ashby. In the limited sense described, the responses vary at random, or blindly. The conditions of termination of response alternation are essentially the same. The differences are worth noting too. There is no S_1 term in Ashby's model, and no unequal prior response likelihoods—although the latter could get built into the uniselector without changing the essential mode of operation. Furthermore, the retroactive effect of reinforcement in strengthening the bond is not used by Ashby.

Skinner's theory (19) of learning is still more similar. His concept of emitted responses, bound by no prior association to specific external stimuli, is quite reminiscent of Ashby's gadget. However, Skinner's use of reinforcement as increasing reflex reserve departs along lines similar to those of Hull and Thorndike.

Guthrie's (7, 8, 9) description of puzzle box behavior most closely parallels Ashby's machine—both as to the random emitting of responses, and as to the mode of selection of the adaptive response. For Guthrie, the response that removes the animal from the stimulus situation that instigated the random floundering is strengthened because it is allowed to remain unoverlayed with the traces of other responses.

It is harder to point to random response-emitting mechanisms in those theories influenced by Gestalt psychology. In general, where response variability in the face of a continued unsolved problem is discovered, there is an emphasis upon the adaptive,

intelligent, corrective nature of these responses, not their random character. (This distinction is made clear in Hilgard's contrasting "trial and error" concepts with those of the "provisional try" and "approximation and correction" (10, pp. 335–339).) However, it is felt that where Gestalt theories conscientiously cover learning in entirely novel situations, random behavior enters the description at some point. In 1925 and 1926 Tolman (21, 22, 24), in partial agreement with earlier statements by Perry and McDougall, was describing purposive behavior in terms of the same essentials that Ashby is now using. In Tolman's terms the essential character of such behavior is "persistent . . . random trial and error . . . until" (22, 24, p. 50). While the randomness of the trial and error has not been a central emphasis in Tolman's over-all theory, it is nonetheless clearly present in a form which Ashby has closely paralleled (21, 22, 23 pp. 339–341). N.R.F. Maier (17) specifies a natural random variability as precursor to learning. The "discontinuity" theories of Krechevsky (13) and Lashley (15) imply a succession of problem solution efforts which are independent of each other, and hence do not manifest the cumulative character of successive corrections. Discontinuity would seem to imply randomness or independence between successive "hypotheses." Certainly in the original "hypotheses in rats" study (12), where blind alleys were assigned at random on successive trials, the hypotheses that were emitted by the rats were randomly or blindly emitted, in the sense of being independent of previous hypotheses: in the sense of not being "corrections" of previous hypotheses, nor systematically varying from previous hypotheses in the direction of greater appropriateness.

There is quite a difference, of course, between a trial and error of muscle movements à la Guthrie and a trial and error of hypotheses each of which programs a series of choice-point responses. However, this difference would not in particular disturb Ashby, for in his more elaborate model, the multistable system, the activated uniselector under some conditions would switch in and out whole subsystems, and under

other conditions would emit variant settings within a subsystem, as described above.

But Ashby's respectable company on this point leaves many problems yet unsolved.

For one thing, his Homeostat cannot learn. It can solve problems, as described, but it starts from scratch on any new problem, it cannot store old solutions for reuse when the same problem repeats itself. Ashby recognizes this limitation (1, p. 133) although he claims that in showing persistent change in the face of experience, the rudiments of learning and memory are present (1, p. 19). But as W. Grey Walter (25) has pointed out, Homeostat is more plant than animal, and a plant can be claimed to show memory at this level. To quote Ashby:

If the homeostat adapts to an environment A and then to an environment B, and is then returned to A again, it has no adaptation immediately ready, for its old adaptation was destroyed in the readjustments to B; it does not even start with a tendency to adapt more quickly than before; its second adaptation to A takes place as though its first adaptation had never occurred (1, p. 133).

Later he suggests that the model can be made to fit better.

It is convenient for simplicity, to assume here that the first reaction is no longer able to be disrupted by subsequent events. The assumption demands little . . . and there is, in fact, some evidence to suggest that, in the mammalian brain, step-functions that were once labile may become fixed (1, pp. 186–187).

This device, coupled with a multiplicity of subsystems, is in part to describe learning. To the present writer the effort fails because Homeostat or Multistat has no way of recognizing environment A or environment B, or distinguishing between them. And such recognition is essential to learning. Homeostat and Multistat are blind. They have no sense receptors. The only input is the disturbance that initiates their blind floundering.

Another difficulty. Even were we to grant that Ashby's machine showed consistent purpose in continuing trial and error behavior until a need was reduced, it would still not be showing the full character of adaptive behavior as we know it. For adaptive, pur-

posive behavior demonstrates itself not only in the learning of habits, but also in the execution of habits. It is characteristic of the learned response in many if not all habits that the response is executed in consistency with environmental objects and places, that the executed response has to be described in terms of minor ends or minor purposes (e.g., 4, 14, 23). The typical response in the execution of a habit is a getting to a choice point, a getting off of a grid, a touching of a lever, a picking up of a food bit. Such responses do not run off blindly, but are flexibly guided in terms of the location of environmental objects and places. And if there is error, the next response or portion of response represents a correction of the error, it has a directionality based upon the character of the error. Were this not so, our habits would have little use for us—there would be only one locus for each habit from which it could be effectively applied. But such guided, aimed, goaled responses contradict Ashby's basic gimmick for adaptation—that of the selective survival of random emissions. At this impasse, one might be tempted to place Ashby's book on the shelf. However, a possibility of resolution remains.

Ashby's animal is, as we have said, blind, deaf, anosmic, insensate. It is all motoric. Its only feedback from the environment is frustration. We can imagine that the first primitive protozoa were thus—perhaps Jennings' *Stentor*, which plays such an important role in Ashby's book. Such an organism learns of an obstacle by not being able to go any further. It distinguishes food from non-food by being nourished or poisoned when it ingests things. Its motor efforts provide its only sensory input. We can imagine a course of evolution for such an organism in which special appendages would evolve for—or some legs be used for—explorations of the environment prior to a total body commitment. Such "feelers" would reduce the energy of exploration by total body trial and error, through substituting an abbreviated, truncated, vicarious trial and error. In this fashion, sense organs would evolve as "trial" motor organs—as scouts and pathfinders radiating

out around the main body and eliminating the expensive waste of involving the main body in trial and error. For the guided, adaptive responses characterizing the habits of complicated animals, sense organs are needed. Ashby's model of adaptation can be sustained if we locate in the operation of these sense organs the random response emission, and provide a mechanism by which the major motor efforts of the organism can profit from the substituting sensory trial and error.

Aiming machines are characteristically gifted with sensory processes that can be interpreted as substitute motor exploration. The radar screen searches the sky with a random or unguided pattern. When the beam hits a plane, a gun is then appropriately aimed. The random trial and error of inexpensive electromagnetic wave impulses has been substituted for a trial and error of expensive projectiles. Note that the automata of Walter (25) and others, though less elegantly derived, have blindly scanning photo cells, which, combined with locomotor devices, give them a free and purposive lifelike character lacking in Homeostat.

If we can accept random sensory exploration as a guiding substitute for random motor exploration, Ashby's general mechanism of adaptation may perhaps still hold, and in addition we may have generated a fruitful way of conceptualizing the role of sensory processes in the execution of learned behavior. The implications seem manifold and challenging. The "Stimulus-Response" symmetry in our thinking is abandoned, much as Dewey predicted it would be (6), and "S" and "R" become parallel, intersubstitutable aspects of the same process. Thought can be seen as developing from perception as a still more vicarious substitute trial and error, in which even the supporting presence of environmental objects is no longer needed. And the Pavlovian model for learning appears as most appropriate for a blind organism, neglecting the major capacities of the perceptual mechanisms.

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