Comment on Dalbir Bindra (1978). How adaptive behavior is produced: a perceptual motivational alternative to response reinforcement. *Behavioral and Brain Sciences* 1, 41-91. doi: 10.150147/S014052T5HXE00_0B5E9H42A2

A long stride towards sense in psychology. *Behavioral and Brain Sciences* (1978) 1, 54-55.

***by* D. A. Booth**
Department of Psychology, University of Birmingham, P O Box 363, Birmingham B15 2TT, England
Email address from 2013: D.A.Booth@sussex ac.uk

Behavior is the pattern of responses to an interpreted complex of current stimuli. Behavior is acquired by classical conditioning, that is, the building of representations of the contingencies amongst stimuli. Bindra's advocacy of these two theses, central to his position, is an important contribution to the current development of behavioral and brain sciences. Moreover, these themes now appear more widely and are pushed further than Bindra's account implies. The commonsense realistic view of mind and behavior has been crippled for two centuries or more by fallacies from physicalist and dualist metaphysics, but scientific theorising and experimental design are at last returning to reality. Thus, Bindra has my general support - while he also has my specific criticisms for reflecting the new mood in only a limited way. He adds physiologising and antimentalism, which, in my view, are otiose. He retains unnecessary elements of drive theory. He does not attempt to defend conditioning theory against charges that it is cumbersome and slippery. I take these limitations briefly in turn.

Bindra's analysis is behavior theory Yet, even more in his book (1976 *op. cit.)* than in this article, he presents his view as brain theory. Dressing one up as the other helps neither psychology nor neuroscience, nor does it advance the final defeat of dualism The neurologising beloved of McGill psychologists and Russian conditioners typically neither adds to the behavioral predictions from the theory nor provides neuroscientists with manageable experimental issues The help that the neuroscientist currently needs from the psychologist is accurate detailing of the brain's input-output relationships - the mental processes within behavioral organisation. Furthermore, psychology will not be reduced to neurophysiology; the two will be married. In my specialty at least (motivation), no such marriage has yet even been consummated (Booth, 1976, pp 453-458; Friedman & Stricker, 1976).

Decades after the later Wittgenstein (1953), Bindra (not alone) regards percepts, thoughts, concepts, desires, and the like as occult entities or ghostly machinations At least his terminology suffers in consequence. Words like "pexgo," "gnostic assembly," and "central motive state" mean what percept, belief, and motivational or emotional state have ordinarily meant, despite what bad philosophy and its manifestations in introspectionist and environmentalist psychologies have tried to twist them to mean. Thinking or other mental activity does not subsist in some unobservable nonbehavioral world. Of course, the detailed organisation of someone's thoughtful silence and immobility is not usually evident from the instantaneous physical appearance of that person in the situation - but then neither is the nature of almost any piece of behavior. Bindra's account of action seems weakened by this residual physicalism, notwithstanding his appreciation for Lashley's and Skinner's awareness of the problem of response equivalence. There can be no burking the fact that action is intentional - that is, directed by the perceived consequences of physical movement or nonmovement of limb, larynx, or whatever. Giving an account of such behavioral structures is in the first instance a problem in psychology, and not in neurology, as Bindra takes it. Furthermore, the mere bundling of involuntary reactions may not create intentionality. In his concern to refute response-reinforcement learning, Bindra appears to regard the notion that a reinforced response can contribute to a conditioned incentive stimulus complex as more of a problem than learning to understand conventional symbols, for example. Where I am in accord with Bindra is in his program of working out the results of assuming that intentional aspects of behavior are based on the same learning mechanism as involuntary aspects of behavior. Uttley (1976), for example, has illustrated this approach in

mathematical brain theory, relating it to both neurophysiology and the Rescorla-Wagner conditioning theory. Bolles's (1976 and this Commentary) new edition of *Theory of motivation* is remarkable for its persistence in advocacy of the R-S "reinforcement" view of learning in the face of repeated admissions that the S-S "associative" analysis does at least as well. Motivations and emotions, operants and respondents, could both be selected into the stream of behavior by biases resulting from conditioned expectancies.

Bindra follows the tradition that bodily states of the organism set the effectiveness or otherwise of external stimuli. I have suggested that we could take the S-S view even more radically and consider the possibility that any perceptible bodily state can also be a conditioned or unconditioned stimulus that elicits behavior in conjunction with other eliciting stimuli from the external environment (Booth, 1977a,b). Extreme drive states and their energising of behavior or limiting of learning have been studied, but the more relevant exploration of the normal range of need states, or hormonal or drugged conditions, as stimuli for selecting behavior has hardly begun.

Bindra's account of conditioned motivation seems to assume that conditioning is stimulus substitution, not adaptive expectancy formation (his Figure 1). Conditioned responses are different from unconditioned responses, not only because the eliciting stimuli are presented in different contexts, as Bindra rightly emphasizes, but also because the appropriate reaction inbuilt to a predictive stimulus is not necessarily the same as the reaction inbuilt to the predicted stimulus. Bindra's view that "what an animal does depends on what it perceives" and on what perception it has acquired, is being applied much more widely than Bindra indicates, even in the much broader treatment in his book (1976 *op cit*). Miller, Galanter, and Pribram (1960) overprecipitately rejected all forms of conditioning theory as incompatible with the hierarchy that appeared necessary to action and cognition. The motivational processes generally treated hierarchically by ethologists can be accounted for by parallel S-R connections in interaction, e.g. by reciprocal inhibition (Ludlow, 1976). A wide range of human problem-solving behavior can be described in terms of minimally structured collections of situation-act connections (Newell & Simon, 1972). Such an organisation is highly flexible and adaptive because the particular connection that is operative at any instant is the one that is most closely specified by the momentary situation that has been created by the environment and the previous operation of other connections.

Such a theoretical approach to cognition has the great advantage that it can be operationalised in a way that can be checked directly against experimental results (including computer simulation if desired). Like Harrison (1972), I have urged (against the psycholinguistic establishment) that mechanisms of language reception and production should be treated in similar terms, not just at the early stage when the infant's language is obviously S-R, but even in adulthood (Booth, 1978). With language, as with other cognitive behavior, theories based on complex hierarchical fixtures may have more to do with logic than with mental processes.

A realistic theory of human behavior - even of the rat's spontaneous behavior - may have to be cumbersome. Collation of myriad facts and execution of routine calculations within a theory may have to be delegated to computer programs. This can cause antagonism. Yet simplistic theory is delusion. Minds and brains are highly engineered universes. Nevertheless, there is a further issue of whether conditioning theory is unnecessarily cumbersome. Suppes's (1969) reduction of finite automata (and hence all specifiable behavior) to a theory of S-S conditioning and S-R processing has been countered by Arbib (1969) as implausibly time-consuming because of a highly constrained learning process. Suppes and Scandura have their own ripostes (Scandura, 1976) and it would be interesting to have Bindra's.

Clearly, Bindra would not permit us to compare cognition with a discrete approach to a single conditioned incentive stimulus. It would be foolish to compare motor control with elicitation of a reflex or perception

with the firing of a bug detector, and yet motor and perceptual integration are readily treated as combinations of reflex-mediating mechanisms and of detector-type neural connections, respectively [cf Kupfermann and Weiss, this issue]. Two areas in which the complexity of coordinating such theorising to experiment seems to be adequately tackled would be applications of Newell and Simon's approach in developmental psychology (e g , Young, 1975) and the editor of the *Journal of Experimental Psychology - Animal Behavior Processes* presiding over experimental rivalries between his own and other variants of conditioning theory.

This last example may also be helping to refute the notion that "cognitive" behavior theory is too slippery to be scientifically useful. Nevertheless, Bindra's approach, and any liberal expectancy conditioning or causal perception theory of behavior, should be recognised as in part a paradigm, not a specific set of hypotheses. Furthermore, the conditioning paradigm can explain anything, just like the Skinnerian radical environmentalist language, because each is a paradigm centred on a tautology. For Skinner, an operant is reinforceable behavior and a reinforcement is what maintains behavior (and al!lbehavior is apparently prejudged operant until proved otherwise). In conditioning theory, the circular definition would be that the conditioning stimulus (or US) is whatever consequence of a stimulus (the CS) changes the response to that CS, and the changed response (or CR) is whatever behavior the CS comes to elicit when the CS-US contingency is imposed. In both cases, the tautology is an apparatus for getting a grip on the particular facts.

Bindra has plenty of good criticisms of particular response-reinforcement hypotheses. However, in the end neither his approach nor his rival will be refuted, I suspect. Rather, one will prove more effective to elaborate than the other.

REFERENCES

Arbib, M. A. Memory limitations of stimulus-response models. *Psychological Review.* 76:507-10. 1969.

Bolles, R C. *Theory of Motivation,* 2nd ed Harper & Row, New York, 1976.

Booth, D A. Approaches to feeding control. In: T. Silverstone (ed.), *Appetite and Food Intake* Abakon, West Berlin, 1976.

Booth, D.A. Satiety and appetite are conditioned reactions. *Psychosomatic Medicine.* 39:76-81. 1977 (a).

Booth, D.A. Appetite and satiety as metabolic expectancies In: Y Katsuki, M Sato, S Takagi, and Y. Oomura (eds.), *Food Intake and Chemical Senses.* University Park Press, Baltimore, 1977 (b).

Booth, D.A. Language acquisition as the addition of verbal routines. In: P. T. Smith and R. N. Campbell (eds.), *Psychology of language.* Plenum, New York, 1978.

Friedman, M. I., and Stricker, E. M. The physiological psychology of hunger: A physiological perspective. *Psychological Review.* 83:409-31. 1976.

Harrison, B. *Meaning and Structure: An Essay in the Philosophy of Language.* Harper & Row, New York, 1972.

Ludlow, A. R. The behaviour of a model animal. *Behaviour.* 58:131-72. 1976.

Miller, G. A., Galanter, E., and Pribram, K. *Plans and the Structure of Behavior.* Holt, Rinehart & Winston, New York, 1960.

Newell, A. & Simon, H A. *Human problem solving.* Prentice-Hall, Englewood Cliffs, N.J , 1972.

Scandura, J M. (ed.). *Structural learning, II Issues and approaches.* Gordon & Breach, New York, 1976.

Suppes, P. Stimulus-response theory of finite automata. *Journal of Mathematical Psychology.* 6:327-55. 1969.

Uttley, A. M. Neurophysiological predictions of a two-pathway informon theory of neural conditioning. *Brain Research.* 102:55-70. 1976.

Wittgenstein, L. *Philosophical Investigations.* Blackwell's, Oxford, 1953.