T-189

PSYCHOLOGY AS A SINGLE VERSUS A CROSS LEVEL SCIENCE

Karl H. Pribram
Radford University

Introduction

It is 1995, and psychology in the United States is divided. Experimentalists and academics are dissatisfied with the growth of professionalism, and clinical and humanistic professionals cannot see the relevance of current experiments to the practical concerns of the field.

The ailment is of long standing, and seems intractable. But I believe that a new look is in order, and that the landscape before us heralds a remedy, if only we can accept what we see.

One of the major blocks to acceptance is judged to be the overwhelming concern of 20th century scientists with method and technique. According to this view we need search no further, and as long as we cannot change our methods by virtue of the interest we pursue, we cannot change what ails us. This concern with method has made psychology a respectable science but, as with every advance, some disadvantages have accrued. It is expressed in terms such as the "hard" and the"soft" parts of psychology (when in fact the soft often exceed the hard in the rigor of their experimental design), in the ambivalence of the clinical toward scientific psychology, and in the disdain of the experimentalist for the thought processes demanded in the clinic.

I believe this emphasis on the divisiveness of method to be false. In fact, method and technique have unified psychology. Differences in subject matter--instrumental behavior, social behavior, verbal reports of subjective experience, psychophysics, man-machine interfaces--have been considerably more divisive than method. We all share a faith in statistics and apply it whenever it is appropriate, and sometimes even when it is not. We all believe in multvariate analysis and in experimental design and apply them whenever feasible. If we are clinicians we accept or reject findings on the basis of a common belief in these methods.

It is the difference in regnant paradigms, not method, which differentiates the various divisions in psychology. At the core, I believe the problem is that experimental psychology's journey from behaviorism to cognitive psychology has been but a beginning. Until that journey is taken a step further, psychology will remain fragmented. I also believe the time is ripe for taking this next step and I want to make this an opportunity to outline the direction it will go.

Perspective

In the decade between 1955 and 1965, a paradigm shift took place in psychology. This shift, which has come to be known as the cognitive revolution, came about by virtue of a convergence of technological innovation, mathematical invention and a host of findings in the neurosciences. Among the remarkable accomplishments of the decade were: Information measurement in communication; servomechanisms in control systems; computers and programming techniques to analyze problem solving; studies of natural language grammars with the aid of symbolic logic; and the analysis of learning from the vantage of sampling and decision theories.

The neurosciences also made critical contributions. Neuropsychology, which had come into disrepute because of a failure to provide reliable data, was shown to be viable once the proper techniques were employed. More important, neurophysiology showed that feedbacks and feedforwards rather than reflex arcs were the elementary circuits in the nervous system. Thus the brain was shown to be capable of controlling its input and organisms were seen as actively operating on their environments. A simple stimulus-response chain, even with intervening variables and hypothetical constructs, did not reflect the actuality of how the organism was put

Pribram, K. H. (1995) Psychology as a Single Versus A Cross Level Science, APA Symposium "Unified Theory Perspectives-Cognitivism, Psychobiology, Psychometric Theory and Behaviorism", International Newsletter of Uninomic Psychology, Cyril M. Franks, Ph.D.(ed.), Volume 15.

together.

Finally, it was shown that dishabituation could occur whenever any aspect of a repetitive situation was altered, even when the alteration involved attenuation or absence of the stimulus. The brains of mammals at least, make "neuronal models" of their inputs, models against which subsequent inputs were processed. Such neuronal models act as representations, and processing constituted computations among representations.

Cognitive psychology centers on studying the active computations which guide organisms in solving problems. Computer programs are vehicles by which computations can be accomplished. Information measurement and other mathematical techniques aid in the construction of problem solving programs. Programs are languages. The revolution was underway.

Today

All of psychology has not become cognitive. Methodological behaviorists continue to demonstrate their strength in devising rigorous experiments. Radical behaviorists continue to decry the ambiguity of natural language and the resulting impossibility of a science of subjective experience. Existentialists and phenomentalists insist that behavior is not the essence of psychology--that subjective experience is what motivates all of us to enter the field. Clinicians have to deal with the verbal reports of introspections, but aspire to have reliable tests that will validate such reports.

Cognitive psychology has at least helped clinicians in their aspirations. The current surge of excellent work in clinical neuropsychology is but one example of linking an analysis of verbal reports with quantitative behavioral testing.

But cognitive psychology itself is beginning to feel its age. The vitality which characterized the revolution is ebbing. More and more experimentalists are concerned with refinements and, to outsiders, sometimes with trivia. There are suggestions of "burn-out"--that the revolution has come to an end, that activity will come to a standstill when it is realized that, after all, the radical behaviorists are right; language is too ambiguous to serve as the core of a science.

I see that matter differently. I, too, see the ending of the 1955-1965 revolution. But I also see the beginning of the next turn of the wheel, the coming revolution of 1995-2005 .

What is wrong with the current paradigm in cognitive psychology is that it is based solely on analogy with the serial processing computer and Von Neuman architecture. Serial programming is excellent for symbol manipulation, but fails to provide access to the richness of texture involved in image processing. And all thought is not imageless.

Thus, cognitive psychology concentrated on intentional problem-solving behavior. Plans as programs worked well; programs as image processors have, until recently, fared poorly. In the 1950s we were convinced that within a decade we would have machines that would produce finished hard copy manuscripts from verbal dictation, that translations from one natural language to another would soon follow. These expectations are as yet unfulfilled, due to the intractibility of image processing by current serial processing architecture and programming.

The deficiency is compounded by the view that, in the nervous system, serial architecture is represented by a hierarchical Euclidian system in which single elements, single neurons, serve as detectors of single features, single percepts, single cognitions. An otherwise excellent text in neuropsychology was based on the concept of a "cognon," a neuron which represents a cognition. In psychophysics, a channel is identified, implicitly or explicitly, with a neuron.

The identification of an idea with a neuron is not new. Bain, in the 19th century, held such a view. It is worth recalling on this 110th anniversary of the publication of Ebbinghaus' treatise on memory, his view of the matter. "The curious theory of Bain and others that each idea is lodged in a separate ganglion cell [is] an hypothesis impossible both psychologically and physiologically."

Why would this identification of percepts and cognitions with single neurons be of such importance to a pioneer psychologist? Why is it important today? The reason can be expressed in terms of mind/brain isomorphism. If indeed neurons correspond to our introspections, the entire cognitive enterprise is built on a faulty premise.

I am writing this essay on my word processor. According to the one neuron/one idea proposal there should be a single switch somewhere in my computer which represents the word computer and another which represents the word isomorphism. Or at least there should be a chip which constitutes such a *representation*. "Utter nonsense!" says the computer scientist. Then why does the cognitive scientist whose model is, and whose modelling uses, the computer and its programs so extensively, accept without question the current neurophysiological "dogma?"

In fact, the neurophysiological evidence is against the one idea/one neuron concept. Each neuron, even in the primary sensory cortices, is selective of several features, not one. Sets of neurons display different conjunctions of feature selectivity. This suggests that spatially arranged patterns of neurons, not single neurons, read out specific features to the next stage of processing. Occasionally in a network of such spatial patterns a node forms which responds more vigorously to a particular conjunction of features under investigation; thus the pontifical "grandmother" cell of which so much as been made in text books. But close inquiry in the laboratory shows that such pontifical cells also respond to other properties of the stimulus, albeit not as vigorously as to one specific conjunction. I would not be surprised if one could occasionally identify a chip or even a switch in the hardware of a computer which responds more vigorously when some feature in assembly language was being processed.

Another aspect of the current malaise in cognitive psychology is its relationship to AI, artificial intelligence. Much of what goes on under this label aims at enhancing problem solution and surpassing human capability. But a respectable group within AI is interested in how humans solve problems. Often this group simply introspects and attempts to use our notoriously ambiguous natural language to construct computer simulations. These often have the appearance of rigor, but the basic premises upon which they are formulated are never examined.

Already, however, there is a fresh wind blowing. The impetus comes (1) from the construction of parallel processing architectures, which allow content-addressable rather than location-addressable programming. These architectures resemble those of the brain much more than today's serially operating devices. (2) The ambiguity of natural language is being replaced by more precise linguistic formulations. This allows a diminution of the ambiguity inherent in verbal reports of problem-solving and perceptual experience. (3) The ambiguity is further reduced by clinical applications, especially in those where brain damage is being examined. (4) Further, mathematical descriptions which can be implemented more readily in parallel networks are becoming influential. Already convolution and matrix models are pitted against each other as explanatory of pattern perception, categorizing, and serial position effects in memory. These models fit neuroanatomical, neurophysiological and neuro-behavioral data much more closely than do less sophisticated feature hierarchy models. And, what is most important, there is room in these models for precise descriptions of processes which lead to intuitions, affects, attention and intention.

These mathematical models range well beyond the statistics which have proved so useful in the social sciences. The mathematical developments in the 19th century can be dated from Fourier's discovery, which showed that every pattern, no matter how complex, can be analyzed into simple component regular wave forms that differ only in amplitude, frequency and relationship to one another.

The Fourier process yields a dimensionality, a "space," in which information becomes distributed and thus enfolded in every portion of the "space." Thus space and time, as we perceive them, become distributed and enfolded, and are no longer the dimensions being processed. In the absence of explicit space and time dimensions, as noted by Gabor in his pioneering paper published in 1946, causality also disappears.

Gabor's analysis of acoustic and visual processing led to his mathematical invention of holography and current engineering techniques of visual processing. Holographic representations represent this distributed, enfolded domain, and there is now an imposing body of evidence that the microprocesses taking place in receptive fields of neurons in the primary sensory systems can be modelled by Gabor mathematics.

Another major mathematical formulation of the late 19th century was the formation by Boltzman of the second law of thermodynamics. This formation has more recently been developed by Prigogine into the mathematics of dissipative structures, structures which dissipate entropy (disorder) by establishing temporary stabilities far from equilibrium. Life, biochemical and neuronal, is characterized by such self-organizing processes.

These distributed and dissipative mathematical formations are much more readily, implemented in the parallel computer architectures currently under development. Psychologists are utilizing these formulations to describe facets of memory and other cognitive processes. Thus James Anderson and his colleagues at Brown University have been engaged in modelling the categorical aspects of memory by matrix models. Ben Murdock at the University of Toronto has implemented convolutional mathematics to handle serial position effects. I have provided neurophysiological evidence suggesting that the brain can work in either the matrix or the convolutional mode, depending on processing demands. Geoffrey Hinton and his group have developed Boltzman "machines" to characterize still other aspects of problem solution such as hill climbing. Cognition, yes, but as yet little in the way of understanding the influence of intuition and affect.

Tomorrow: Affordances and Consequences

In *The American Psychologist*, January, 1989, p. 18, Skinner wrote: "There are two unavoidable gaps in any behavioral account: one between the stimulating action of the environment and the response of the organism and one between consequences and the resulting change in behavior. <u>Only brain science can fill those gaps</u>. In doing so it completes the account; it does not give a different account of the same thing." [Underlining mine.]

At no time in my long association with Skinner did he ever disparage the utility of the brain sciences for an understanding of behavior. What he decried was the practice (as e.g., by Pavlov and Hebb) of neurologizing concepts derived exclusively from the experimental analysis of behavior. He was convinced that putting in neurological language what should properly be behavioral constructs gave such constructs unearned validity which often proved ephemeral.

Why then the empty organism approach? Skinner (1976) was clear on this point: We first need a behavioral science that can stand on its feet without recourse to biology. Once established, behavioral science can again turn to biology for filling "the unavoidable gaps" in the behavioral account.

In short, with respect to the brain sciences, Skinner's philosophy abhorred the identity stance. The behavioral and the brain sciences were at different levels of inquiry; each had its place in explanation, and to mix levels operationally was a cardinal sin.

Much of science has initially proceeded in this fashion. In their early stages, physics was physics and chemistry was chemistry. But at a somewhat later epoch, an explanation of the periodic table of chemical elements was found to come from atomic physics and even, in the case of radioactive elements, from quantum physics. Today the boundary areas among the natural sciences form sciences in their own right: physical chemistry, thermodynamics, biochemistry, for example.

The brain/behavioral science interface is also spawning its own set of boundary sciences: neuropsychology, psychobiology, cognitive neuroscience, etc. The question that needs to be answered is whether the gaps in the behavioral account can be filled by the brain sciences working solely at the biological-brain level of inquiry, or whether gap-filling, the province of these boundary sciences, is critical.

To answer this question, let us look in detail at the two gaps in the behavioral account. The first is between "the stimulating action of the environment and the response of the organism. " Gibson has given the issues concerning this gap a name. He calls them "affordances": Certain aspects of the environment allow the organism to perceive what it perceives (Gibson, 1979). Originally, the concept was established the other way around: Certain characteristics of organisms afford the selection of aspects of the environment in order to perceive them (Gibson, personal communication). The change was made in order to facilitate an experimental program designed to find out just which environmental configurations, in fact, afford particular perceptions. This program called ecological psychology, has been successfully engaged by Turvey, Shaw, and Kugler (see e.g., Kugler, Shaw, Vincente, and Kinsella-Shaw, 1990; Shaw and Kinsella-Shaw, 1988; Turvey, Shaw, Reed and Mace, 1981).

However, the earlier definition of affordances has merit as well. What are the characteristics of organisms that select just those aspects of the environment uncovered by ecological psychologists? In one set of experiments performed in my laboratory, we found that single neurons in the visual and auditory brain systems show their selective orientation and frequency responses (as determined by presenting a range of specific orientations and frequencies) even when the environmental stimulus consists of visual or auditory white noise. (For an extensive, in depth review of these and other experimental results bearing on the question of brain organization in perception, see Pribram, 1991.)

In such experiments, both brain variables and those describing the stimulating action of the environment were taken into account. Affordances are constituted by <u>both</u> their biological and their environmental determinants. With regard to affordances, therefore, my answer is that the gap in the behavioral account between the stimulating action of the environment and the resulting behavior cannot be filled by studies restricted to the brain level alone, any more than it can be answered at the environmental level alone. Answers are provided by boundary science inquiries which extend the ecological stance into the organism and do not stop short at the receptor surface (Pribram, 1982, 1991).

Next, let us examine the gap between consequences and the resulting change in behavior. The easy answer here might tempt one to conclude that consequences leave traces in the brain, and that the problem to be addressed is neuronal plasticity and "memory" storage. These are fascinating biological problems in their own right. But solving how plasticity leads to storage in the brain will not by itself fill the gap between consequences and the resulting change in the <u>organization</u> of behavior.

What we need to know is how behavioral organizations produce storage in such a way that they can <u>configure</u> changes in response. At a simpler level, how do brain processes configure at all? The problem is to account for figural equivalence in response as well as for figural change.

For Skinner, the figured consequences of behavior are the environmental resultants of that behavior. By his own statement, these consequences are the "cumulative records" he took home to analyze. According to his view, when I write in a notebook or type onto a word processor, the consequences of my behavior are in the environmental record. Storage is in my files, and once published, in bookcases of my colleagues. I hope these environmentally stored consequences of my behavior will influence (change) my future behavior and that of my colleagues. Bruner is correct; much of what configures and influences my behavior is stored in an ever-evolving culture (Bruner, 1990).

But the question remains as to how such cultural configurations are produced? Production does not rest on the particulars of the movements that produce them; a document can be constructed on a keyboard, with a right or left hand, or even on sand or blackboard with toes or teeth in an emergency. Further, the mode of expression does not unduly alter what one wants to express. There must be some brain process that directly codes what is expressed, what is written (the cumulative record).

I have elsewhere reviewed in detail (Pribram, 1971, 1991; Pribram, Sharafat & Beekman, 1984) experiments by Bernstein and his collaborators (Bernstein, 1967), by Brooks (1986), by Evarts (1967) and those performed in my laboratory that show how and where such a brain process occurs. Bernstein introduces the issue as follows:

> There is considerable reason to suppose that in the higher motor centers of the brain (it is very probably that these are in the cortical hemispheres) the localization pattern is none other than some form of projection of external space in the form present for the subject in the motor field. This projection, from all that has been said above, must be congruent with external space, but only topologically and in no sense metrically. All danger of considering the possibility of compensation for the inversion of projection at the retina ... and many other possibilities of the same sort are completely avoided by these considerations. It seems to me that although it is not now possible to specify the ways in which such a topological representation of space in the central nervous system may be achieved, this is only a question of time for physiology. It is only necessary to reiterate that the topological properties of the projection of space in the C.N.S. may prove to be very strange and unexpected; we must not expect to find in the cortex some sort of photographic space, even an extremely deformed one. Still, the hypothesis that there exist in the higher levels of the C.N.S. projections of space, and not projections of joints and muscles, seems to me to be at present more probable than any other. (Bernstein, 1984, p. 109)

With these insights Bernstein set the problem which neurophysiologists must address if they are to relate the anatomical configuration of the central motor process to the configuration of the consequence of behavior. Neuroanatomists have demonstrated a somatotopic representation of muscles onto the cerebral cortex. But as Bernstein points out it is the topological representation of external space not of projections of joints and muscles, that is needed if patterns of behavioral acts, the consequence of movements, and not just patterns of movements per se are to be explained. Bernstein, in his experiments, used Fourier analysis to specify the topology of such behavioral actions and his specifications were sufficiently accurate to allow prediction of the patterns of continuing action.

Experiments were undertaken in my laboratory to test the hypothesis that the Fourier approach might also be as useful in analyzing the physiology of single neurons in the motor regions of the brain as it was for analyzing patterns of behavioral actions. Support for such an approach came from its success when applied to the analyses of the functions of the sensory systems.

Neural Encoding in the Spectral Domain

Experiments were undertaken to find out whether there are cells in the motor system which respond selectively to a band width of frequencies of a cyclic up-down passive movement of a forelimb. The results of the experiment showed that a 20% portion of a total of 306 cells sampled were tuned (i.e., increase or decrease their activity at least 25% over baseline spontaneous activity) to a narrow (1/2 octave) band of the frequency spectrum.

Tuning could be due to a spurious convergence of factors relating to the basic properties of muscle: metric displacement and tonicity or stiffness. An examination was therefore undertaken of variables related to these basic properties, variables such as velocity, change in velocity (acceleration), as well as tension, and change in tension. These factors in isolation were found not to account for the frequency selective effects. This does not mean that other cells in the motor system are not selectively sensitive to velocity and changes in tension. But it does mean that the frequency selectivity of the cells described is dependent on some higher order computation of the metric and tonic resultants imposed on the foreleg musculature by the external load.

In addition to controlling for selectivity to velocity and acceleration, position in the cycle of movement was investigated. Position was found to be encoded by cortical cells (but not by caudate nucleus cells), but only at the site of phase shift and specific to a particular frequency. This result supports the hypothesis that the cortical cells are in fact frequency selective in that any sensitivity to phase shift presupposes an encoding of phase and therefore of frequency. Furthermore, the fact that the cortical cells respond to position suggests that they are directly involved in the computation of the vector space coordinates within which actions are achieved.

There is thus no question that an approach to analysis of the functions of the motor system in Fourier terms has proven useful not only in studying the overall behavior of the organism as initiated by Bernstein but also in studying the neural motor process. That some such an approach is required is amply documented in a review of the field, initiated by R. B. Stein in an article entitled "What muscle variable(s) does the nervous system control in limb movements?" which became available in the December issue (1982, Vol. 5, No. 4) of *The Behavioral and Brain Sciences*.

Psychology: The Interface Between Biology and the Social Sciences

With respect to consequences, therefore, the gap in the behavioral account is filled by a process that reciprocally transforms, i.e., correlatively codes configured brain with environmental events including such cultural artifacts as writing. Thus, the data that describe this process are not obtained solely at the biological-brain level of inquiry. As in the case of affordances, both environmental and brain variables are critical to understanding. With respect to consequences in a human setting, the environmental variables are, to a large extent, cultural.

The gaps in the behavioral account that are due to the processes that determine affordances and consequences are therefore filled by boundary, not by same-level science.

This analysis calls into question a program of research which aims to make psychology a purely same-level behavioral science devoid of its biological and social relations. I do not question the immense contribution technical behaviorism has made to our understanding of psychological processes. However, the yield in understanding harvested by this same level science in psychology has been disappointing in one respect to many of us. Psychological science has been unable to put its house in order. Instead, a welter of languages has developed to address identical issues (e.g., in attention and in short term memory research) and many issues go unexplored (e.g., learning through imitation) due to a failure to find applicable same-level science tools.

Biology did not come of age until boundary sciences were established. Genera and species were identified by recourse not only to anatomical morphology (analogy) but also to functional changes in morphology (homology). Classification led ultimately to the theory of selective evolution (as in horticulture and animal breeding) and to molecular genetics, all the results of explorations in interface sciences.

Psychology, the science of mental processes, may well depend for its maturity on the development of its interfaces with the social and biological sciences. As I have indicated, studying plasticity in the brain is not enough. The resultants of plasticity are configured by environments-- in the case of humans largely by culture. But studying culture alone is equally barren; culture is constructed by behaving humans whose brains generate the multiforms encoded as cultures. Behavior is central, but behavior, whether verbal or instrumental, is only an expression of mind--the generative psychological process.

What's in a Name?

These, then, are the developments which I believe will make psychology whole again. Once George Miller, Eugene Galanter and I found ourselves in transit from behaviorism to cognitive psychology, we noted that we were really "subjective behaviorists" and laughed at the paradox which, at that time, that term seemed to imply. We also noted that, to cope with the subjective portion of our agenda, we would have to develop a new set of scientific procedures. We suggested that enactment of subjective experience by computer simulation would serve this purpose. I view computer simulation as akin to the "in vitro" experiments performed in biochemistry, the computer serving as the test tube.

In the 19th century, cognition was joined to conation and affect to compose psychology. I doubt that the current changes in psychology will be termed conative or affective, but the changes portend in this direction. Emotions and motivations as root dispositions, thus far, have been inaccessible to enactment. Such dispositional variables may well become accessible to simulation when the parallel architecture and quantum distributed and thermodynamic dissipative mathematics are applied. To make psychology whole, its regnant paradigm must truly reflect the totality of subjective behaviorism, not a paradigm that is limited to perception and cognition, to problem solving and "information processing." The structuring of "redundancy" in terms of familiarity and novelty has been neglected except for a few pioneers such as Tex Garner (1962), George Miller (1956) and Herb Simon (1974) (on chunking and the magical number). It is the dimension of familiarity/novelty, not amount of information, which influences arousal. Modelling the apparatus by which chunking occurs, and the modelling of graph structures in general, is bound to benefit greatly from the availability of parallel processing architectures and programs based on matrix and convolutional mathematics.

What will be the name of this next turn in the development of scientific and professional psychology? It is hard to predict. I would like to see the label "holistic" become respectable. For not only is the whole greater than and different from the sum of its parts, as the Gestalt psychologists were wont to point out, but the whole can under certain conditions also become enfolded in all its "parts." Thus each "part" represents the whole, as in a hologram. Convolutional and matrix mathematics, the distributed and dissipative structures we are coming to know, allow holistic descriptions to be as rigorously scientific and precise as any that have been used in physics, chemistry and biology.

At the same time, these developments in mathematics and computer architecture allow us to model psychological processes as diverse as imaging and intuition, as respectable as sensory psychophysics and as non-sensical (non-sensory) as mystical experience. For a half-century, quantum physicists such as Niels Bohr, Schrodinger, Einstein and Heisenberg shared their insights with us by pointing out the similarity of their findings with those of the Veda and Upanishids and other spiritual disciplines. Is it not time that psychology listens, places the Newtonian cosmology in perspective and comes to grips, where relevant, with the models developed in 19th and 20th century mathematics and physics?

The transition from behaviorism, especially stimulus-response behaviorism, to cognitive psychology was characterized by an increasing difficulty with operationalizing such concepts as drive, and an increasing ability to operationalize such concepts as effort and attention. I believe that the next revolutionary turn in psychology will, in a similar way, be characterized by an increasing difficulty in operationalizing concepts we now hold dear, such as information processing, and by and increasing ability to operationlize such concepts as meaning and intuition. The 21st century is beckoning, and I predict advances in psychology, both as a science and in practice, which will rival those in the biology, the chemistry and the physics of the 20th. This is my faith.

REFERENCES

Bernstein, N. (1965) *The Co-ordination and Regulation of Movements.* New York:McGraw Hill.

Bernstein, N. (1967) *The Co-orindation and Regulation of Movements.* New York: Pergamon Press

Boltzann (1897/1974) *Theoretical physics and philosophical problems.* Boston, MA: Reidel (Translated to English by Paul Foulkes)

Brooks, V.B. (1986) How does the limbic system assist motor learning? A limbic comparator hypothesis. *Brain and Behavioral Evolution, 29,* 29-53

Bruner, J.S. (1990) *Acts of Meaning.* Cambridge, MA: Harvard University Press.

Evarts, E.V. (1967) Representation of movements and muscles by pyramidal tract neurons of the precentral motor cortex. In M.D. Yahr & D.P. Purpura (eds.), *Neurophysiological basis of normal and abnormal motor activities* (pp.215-254). Hewlett, NY: Raven

Gabor, D. (1946) Theory of communication. *Journal of the Institute of Electrical Engineers, 93,* 429-441.

Garner, W.R. (1962) *Uncertainty and structure as psychological concepts.* New York: Wiley.

Gibson, J.J. (1979) *The senses considered as perceptual systems.* Boston: Houghton Mifflin.

HUMAN MOTOR ACTIONS - BERNSTEIN REASSESSED (1984) H.T.A. Whiting (ed.) North Holland:Elsevier Science Publishers B.V.

Kugler, P.N., Shaw, R.E., Vincente, K.J., & Kinsella-Shaw, J. (1990) Inquiry into intentional systems. I: Issues in ecological physics. *Psychological Research,* 52, 98-121.

Miller, G.A. (1956) The magical number seven, plus or minus two, or some limits on our capacity for processing information. *Psychological Review,* 63, 81-97.

Miller, G. A., Galanter, E. & Pribram K. H. (1960) *Plans and the Structure of Behavior.* New York: Henry

Holt, 1960. (Russian trans; also in Japanese, German, Spanish, Italian.)

Pribram, K.H. (1971) *Languages of the Brain: Experimental Paradoxes and Principles in Neuropsychology.* Englewood Cliffs, NJ: Prentice-Hall; Monterey, CA: Brooks/Cole, 1977; New York: Brandon House, 1982. (Translations in Russian, Japanese, Italian, Spanish)

Pribram, K.H. (1982) Brain and ecology of mind. In: W. S. Weimer & D. S. Palermo (Eds.), *Cognition and the Symbolic Process,* (pp. 361-3 81). Hillsdale, NJ: Lawrence Erlbaum Associates.

Pribram, K.H. (1985) Holism' could close the cognition era. *APA Monitor, 16,* pp. 5-6.

Pribram, K.H. (In publication) Inter-facing Complexity at a Boundary Between the Natural and Social Sciences. *Social and Natural Complexity.*

Pribram, K.H. (1991) *Brain and Perception: Holonomy and Structure in Figural Processing* New Jersey: Lawrence Erlbaum Associates, Inc.

Pribram, K.H., Sharafat, A., & Beekman, G.J. (1984) Frequency encoding in motor systems. In H.T.A. Whiting (Ed.), *Human motor actions—Bernstein reassessed* (pp .121-156). North-Holland: Elsevier.

Prigogine, I. (1980) *From Being to Becoming - Time and Complexity in the Physical Sciences.* San Francisco, CA: Freeman

Shaw, R. E., & Kinsella-Shaw, J. M. (1988) Ecological mechanics: A physical geometry for intentional constraints. *Human Movement Science*, 7, 155-200.

Simon, H. (1974) How big is a chunk? *Science*, 183, 482-488.

Skinner, B.F. & Yingling, C.D. (1976) Regulation of slow potential shifts in nucleus retincularis thalami by the mesencephalic reticular formation and the frontal granular cortex. *Electroencephalogr. & Clin. Neurophysiol.*, 40, 288-296.

Skinner, B.F. (1989) The origins of cognitive thought. *American Psychologists*, 44(1), 13-18.

Stein, R.B. (1982) What muscle variable(s) does the nervous system control in limb movements? *The Behavioral and Brain Sciences*, vol. 5, no. 4

Turvey, M.T., Shaw, R.E., Reed, E.S., & Mace, W.M. (1981) Ecological laws of perceiving and acting: In reply to Fodor and Pylyshyn. *Cognition*, 9, 237-304.