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INTERFACING COMPLEXITY AT A BOUNDARY BETWEEN THE NATURAL AND SOCIAL SCIENCES

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When I asked leading Berkeley astronomer and Nobel Laureate Charles Townes, "Which is more complex, the 100 billion stars in our galaxy or the 100 billion nerve cells in the 3-pound mass within our heads?" he answered without hesitation, "The brain. . . . For, after all, it is only the brain that can interpret our galaxy." And it is only the brain that can interpret its own cognizance.

Marian Cleeves Diamond, 1990

Science has only two ways of proceeding: it is either reductionist or structuralist. It is reductionist when it is possible to find out that very complex phenomena on one level can be reduced to simpler phenomena on other levels. For instance, there is a lot in life which can be reduced to physicochemical processes, which explain a part but not all. And when we are confronted with phenomena too complex to be reduced to phenomena of a lower order, then we can only approach them by looking to their relationships, that is, by trying to understand what kind of original system they make up.

Claude Lévi-Strauss, 1978, pp. 9 and 10

THE PARADOX OF COMPLEXITY

As noted in the quotation above, the brain has repeatedly been heralded as the most complex piece of matter in the universe. There are probably at least as many synapses, connections between nerve cells, as there are suns in the galaxies.

In the same vein, human social systems are the most complex on earth. There are also at least as many social connections as there are suns in the heavens. But human societies are made up of humans each blessed (or cursed) with a human brain. Which then is the more complex: The social system or the neural?

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For years it seemed to me that the concept of "complexity" was untenable because self-similarity at different levels as in the case of fractals, defied simple categorizing (see Pribram, 1985). Nonetheless, as the title to this volume attests, serious scholars continue to find the concept useful and therefore try to define it (see e.g., review by Seth Lloyd, 1990). Each definition appears to grasp part of the puzzle – but the answer to the puzzle: "Which is more complex – the brain, or the social system, or the universe?" continues to be elusive.

I have now come to a new view. There is a reason for the current failure of definition: Modern analytical logic fails to cope with complexity. Not until we come to realize that the concept is paradoxical, can we begin to understand it. The dictionary definition of complexity already hints at paradox: complex, (1) from the Latin *co* (together) *plectere* (weave, braid): entwined; intricate; (2) from the Latin *complexus* (embracing, surrounding): anything formed by the union of interconnected parts; an assemblage; a system. Note that definition (1) emphasizes the intricacy of internal structure while definition (2) embraces the "system" as a whole.

Complexity is thus shown to encompass two levels or scales – a multi-form interior or micro-level and a macro-level whole. Complexity, therefore, is a cross-scale, cross-level concept, and it must be measured accordingly. The paradox: an intricate diverse micro-level embraced within a simpler macro-level entity – unity in diversity.

In the brain/behavioral science interface, this paradoxical aspect of complexity has interesting consequences. Levels must be clearly discerned by an embracing relationship. A level or scale can be defined as a presentation, a description of an *entity* that is simpler than if it were made in terms of the collection of constituents of that scale or level. Thus the entity at each level can be characterized by a description that is a presentation. Components are described in some different fashion from the entity as a whole. Furthermore, there would be no need for a presentation of the entity as a whole were it not in some basic sense, simpler – that is, more efficient to process than that available to the components. For example, bytes are more efficient in use than the equivalent description in bits. A presentation of a program in Fortran is much more efficient than a presentation of the successive switch settings that form the hardware equivalent of the program. The question for us is whether psychological processes can, in the same manner, be considered to be simpler, more efficient, representations of functions of the brain.

In the sense of hierarchical levels of presentation, the analogy between computer software (programs) and hardware serves well. The psychological, mental level is described in presentations that are analogous to presentations at the program level. The "wetware" of the brain can be thought of as analogous to the hardware of the computer (Miller, Galanter, and Pribram, 1960; Pribram, 1986a). There is an equivalence between program

and successive hardware switch settings. Can we say, therefore, that in some real sense the switch settings are represented in the program? If this is so, then in the same sense, psychological processes represent brain function.

This leads to a most tantalizing question: To what extent are the represented entities configured in a fashion similar to the entities they represent? In other words, to what extent are presentation and "representation" isomorphic to one another? The answer to this question obviously depends on reaching some consensus on the definition of isomorphic. Processes that map into each other in such a way as to preserve structure can be said to be either geometrically or algebraically isomorphic. For instance, although the Gestalt psychologists thought that the electrical fields of the brain have geometric *shape* resembling that of perceived objects, evidence shows that perspective transformations display algebraic (i.e., secondary), not geometric isomorphism (Shepard and Chipman, 1970).

By contrast, the computer program-hardware analogy suggests that significant *transformations* can occur between levels of presentation – indeed that the utility of representations is derived from these transformations. The analogy helped make understandable the results of neuropsychological research which showed that the search for "pictures" in the brain was misplaced (see Pribram, 1971, Chapter 6 for review). Understanding comes when the neuropsychologist searches for algebraic algorithms, such as computable *transforms* of sensory input. This emphasis on transformation is the key to resolving the larger issue of how to deal with the paradox of complexity: Same-level science by itself is ineffective. The resolution to issues comes when transformations across interfaces are taken into account.

PSYCHOLOGY AS A SAME-LEVEL AND AS A CROSS-LEVEL SCIENCE

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. *Only brain science can fill those gaps.* In doing so it completes the account; it does not give a different account of the same thing." [Italics 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

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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.

AFFORDANCES AND CONSEQUENCES

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 is the province of these boundary sciences.

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, afforded 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 a 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 *both* 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 that it can be filled 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).

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 *organization* of behavior.

What we need to know is how behavioral organizations produce storage in such a way that they can configure 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 environmental storage 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, and Beekman, 1984) experiments by Bernstein and his collaborators (Bernstein, 1967), by Brooks (1986), by Evarts (1967) and those

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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 probable 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, 1967, 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. These analyses are reviewed here in some detail in order to provide a background of expectations and of problems faced when we attempt to relate the configuration of a neural process to the configuration of a behaviorally produced environmental consequence.

THE FOURIER APPROACH TO THE SENSORY SYSTEMS

The first suggestion that brain processing might involve a Fourier analysis was made over a century ago for the auditory system by Ohm (1843), the same Ohm who formulated Ohm's Law of Electricity. This suggestion was adopted by Herman von Helmholtz (1863) who performed a series of experiments which led to the place theory of hearing – essentially a view of the cochlea as a piano keyboard, whose keys, when struck by acoustic waves, would initiate nerve impulses to the brain where resonant neurons were activated. This view was modified in this century by George V. Bekesy (1959). His experiments showed the cochlea and peripheral neurosensory mechanism to operate more like a stringed instrument which is sensitive to the superposition of acoustic wave forms. This work led to the discovery that the initial stages of auditory processing can be described in terms of a Fourier transform of the acoustic input (Evans, 1974).

Bekesy went on to make a large-scale model of the cochlea composed of a row of five vibrators (1959). When the model was placed on the forearm and the phase of the vibrators adjusted manually, the phenomenal perception was that of a point source of stimulation which could be moved up and down the arm. When two such model "cochleas" were applied, one to each forearm, the point source appeared at first to jump alternately from one forearm to the other, and then suddenly to stabilize in the space between the two arms. The stimulus was "projected" away from the stimulating source and the receptive surface into the external world, much as sound is projected into the environment away from the source in audio speakers of a high fidelity stereophonic system.

Both macro- and micro-electrode studies performed in my laboratory have shown that multiple simultaneous vibratory stimulations of the skin also evoke only unitary responses in cortex (Dewson, 1964; Lynch, 1981). Just as in perception, the cortical electrical response does not reflect the actual physical dimensions of the stimulus. Bekesy noted that sensory inhibition, due to lateral inhibition in dendritic networks, might be the responsible agent in the transformations.

Evidence is therefore at hand to indicate that the input to the ear and skin becomes transformed into neural patterns that can be described by sets of convolutional integrals of the type that Gabor (1969) has suggested as stages in achieving a fully developed Fourier holographic-like process.

The manner in which such a stepwise process occurs is best worked out for the visual system. Recordings from units in the optic nerve (Rodieck, 1965) demonstrated that the moving retina decomposes the image produced by the lens of the eye into a "Mexican hat" organization which can be described as convolving retinal organization with sensory input. A second step in the process occurs at the lateral geniculate nucleus where each geniculate cell acts as a peephole "viewing" a part of the retinal

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mosaic. This is because each geniculate cell has converging upon it some 10,000 optic nerve fibers originating in the ganglion cells of the retina. The receptive field of the geniculate neuron is composed of a center surrounded by concentric rings of receptivity, each consecutive ring of sharply diminishing intensity and of a sign opposite to that of its neighbors (Hammond, 1972; Pribram, personal observation).

At the cortex the transformation into the Fourier domain becomes complete. Beginning with the work of Campbell and Robson (1968), Pollen, Lee, and Taylor (1971), Maffei and Fiorentini (1973), and Glezer, Ivanoff, and Tscherback (1973), investigators using gratings as stimuli (e.g., Schiller, Finlay, and Volman, 1976; DeValois, Albrecht, and Thorall, 1978; Movshon, Thompson, and Tolhurst, 1978; Pribram, Lassonde, and Pito, 1981) have repeatedly confirmed that the cells in visual cortex are selectively tuned to a limited band width of spatial frequency of approximately an octave ($\frac{1}{2}$ to $1\frac{1}{2}$ octaves). Ordinarily the term frequency implies a temporal dimension, but the spatial frequency (or wave number) of a grating reflects the width and spacings of the bars making up the grating. When such widths and spacing are narrow the spatial frequency is high; when widths and spacing are broad the spatial frequency is low.¹

The findings do not, however, mean that the visual system performs a global Fourier transform on the input to the retina (see also Julesz and Caelli, 1979). The spread function, as such transformations are called, does not encompass the entire retina: Rather it is limited to the receptive field of a retinal ganglion cell. Similarly at the cortex encoding is restricted to the receptive field of the cortical neuron.

This patchy organization of the Fourier domain (Robson, 1975) does not impair its functional characteristics. The technique of patching or stripping together Fourier transformed images has been utilized in radioastronomy by Bracewell (1965) to cover expanses which cannot be viewed with any single telescopic exposure. The technique has been further developed by Ross (see Leith, 1976) to produce a hologram by which three-dimensional *moving* images are constructed when the inverse transform is effected. Movement is produced when spatially adjacent Fourier encoded strips, which capture slightly different images are scanned (temporally) as, for instance, when frames of a motion picture are used as the image base for the Fourier transformation.

Such framed Fourier patches have come to be called Gabor elementary functions. Gabor (1946) noted that the Fourier transform relates the spectral domain to spacetime in an either/or fashion. For the purpose of determining the maximum efficiency of telephone communication across the Atlantic Cable, Gabor quantified Hartley's Law (1928) which dealt with the trade off between spectrum (frequency) and spacetime. Gabor therefore developed a phase (Hilbert) space in which both spectrum and spacetime were simultaneously represented and thus were correlated (to

describe the density of information transmission). Gabor used the same mathematics as had Heisenberg to develop the definition of a quantum in microphysics. Gabor therefore called his elementary function "a quantum of information". More on this presently.

NEURAL ENCODING OF THE CONSEQUENCES OF BEHAVIOR

In my laboratory (Spinelli and Pribram, 1967; Pribram, Lasselonde, and Pfito, 1981) we showed that the Gabor function could be altered to reflect either the spacetime or the spectral dimension - i.e., the Gabor function could be "pushed" to its either/or Fourier origins. This was accomplished by electrically stimulating the posterior cerebral convexity (to produce the spacetime configuration) or the frontolimbic forebrain (to produce a spectral, holographic-like domain).

For motor function the Fourier (and Gabor) principles of organization were found to hold. 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 per cent portion of a total of 306 cells sampled were tuned (i.e., increase or decrease their activity at least 25 per cent over baseline spontaneous activity) to a narrow ($\frac{1}{2}$ 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

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the motor system in frequency 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, 5 (4)) of *The Behavioral and Brain Sciences*.

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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. To know the resultants of 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.

TRANSCENDING THE MIND/BRAIN DUALITY

The considerations lead directly to the perennial question: Can such disparate entities as the material brain and our ineffable, private mental experiences be related by crass neuropsychological cross-scale endeavors? Whether we ought or not, the fact is that clinical neuropsychology is successfully pursuing just such a program, not only in enhancing knowledge, but also in the practical matters of alleviating suffering. How has this come about? Again some defining concepts are in order. In *Languages of the Brain* (1971), I stated the matter as follows:

Over the past half century subjective experience has rarely been admitted as a legitimate field for scientific inquiry. Instead, the focus of study has been instrumental or verbal behavior *per se*. This approach has been generally successful in quantitatively delineating environmental variables that influence behavior, but somewhat less than successful when variables within the organism codetermine what happens. In such circumstances the data make considerably more sense when physiological as well as environmental variables are monitored. It is important to emphasize that the behavioral approach cannot, however, be dispensed with: many clinical neurologists and brain physiologists have neglected specification of relevant environmental circumstance, uncritically asserting an identity between what they observe physiologically and some psychological function. Thus the study of psychological processes had become polarized, with behaviorists at one extreme and physiologists at the other. On the one hand, most early behaviorists declared that operations defining subjective statements were impossible and that scientific psychological language should, therefore, entirely exclude mental terms; meanwhile, medically trained scientists would loosely refer to psychological functions such as voluntary action, affective feeling, or imagination on the basis of uncontrolled subjective reports without specifying the defining operation of their language, thus making it difficult for other scientists to know what was being talked about.

During the 1960s behavioral psychology came to appreciate the dictum of Gestalt psychology that subjective awareness is an integral part of the biological and social universe and is too central to these operations to be ignored. Thus "respectable" psychologists began to work on problems such as cognition, thought, and attention. By the

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end of the decade even Imaging, our present subject, could be discussed openly at psychological meetings without undue risk.

This broadening of the base of psychological inquiry came about, of course, by a rigorous attempt to detail the defining operations that make possible scientific communication about subjective processes. My own procedure is to start with nonbehavioral means to describe categories of organismic and environmental circumstances. I then use behavior as a dependent variable to study interactions between the categories (which constitute the independent variables of the experiment). From data obtained in such experiments I *infer* psychological functions and examine their similarities and dissimilarities to verbal reports of subjective experience. When the fit appears right, I use mental language (Pribram, 1962; 1970).

This recourse to mental terms is not capricious. For one thing, much of clinical neurological analysis is based on the verbal reports of subjective experience when the brain is damaged or electrically stimulated. Further, I found the behaviorist jargon (with which I had been doing my thinking) replete with inconsistencies that couldn't be clarified until I admitted the relevance of the subjective mode. In other words, I had to come to terms with Gilbert Ryle's (1949) famous "Ghosts in the Machine". Images and feeling are ghosts – but they are ghosts that inhabit my own and my patients' subjective worlds. They are our constant companions and I want to explain them.

(Pribram, 1971, pp. 99–101)

Interest in the relationship between psychological function (mind) and brain has become further invigorated by the surge of activity in the neurosciences over the past two decades and in what has become called "cognitive science." The surge of interest in mind/brain issues has come in various guises. Cognitive scientists have argued whether "representations" or "computations" characterize the relationship (see e.g., Gardner, 1985; "Special issue" in *The Behavioral and Brain Sciences*, 1980). A philosopher and a neuroscientist have banded together only to find themselves maintaining an interactive separateness of mind and brain (Popper and Eccles, 1977). Further, a neuroscientist (Sperry, 1952, 1969, 1976, 1980), as well as a philosopher (Searle, 1979) have declared themselves solidly on the side of mind. Meanwhile, a psychologist (Skinner, 1971, 1976) has given up hope that a "science of mental life" as William James (1901), and more recently George Miller (1962), have dubbed it, is possible at all. Skinner bases his view on the premise that such a science would depend on verbal communications, which are notoriously ambiguous.

It is this variety in the attempts to deal with mind/brain relations that called forth my reevaluation. The time was therefore ripe to take a new look at this fascinating interface from the standpoint of the scientist as well

as from that of the philosopher (see Pribram, 1986a). I know most of the protagonists personally and have high regard for all of them, as I have for much of the philosophical discourse that bears on the issues. It seemed to me that these intelligent scholars cannot all be wrong despite the fact that their respective contributions are at variance with one another. Could it then be that they are all correct, in some nontrivial sense? If so, how?

My suggestion was that each of these espoused philosophical positions has captured a part of the domain of issues, and that what is necessary is to determine the database on which the position rests. The failure of philosophy to resolve the issues comes when a position is maintained beyond the confines of its relevant database to a point where another position is more appropriate.

The danger of such an eclectic approach is that one may end up with an "any worlds" or at least with a "many worlds" relativist viewpoint, which is fine if one wishes to show merely that there are many different answers to the questions posed. But I am not satisfied with such a result. I was able to show that the several databased theoretical frames fit different epistemological agendas in philosophy, but that a unified ontological view can be constructed out of the diversity of theories.

A proposal made by Eccles provides a good starting point to explore this relationship between the different epistemological agendas and a unified ontological view. Eccles (1986) discerns elementary aggregates of dendro-synaptic structures he calls dendrons; being a dualist, he suggests corresponding mental entities, psychons. But what might a psychon be? Behavioral scientists have only rarely concerned themselves with "the unit entities of behavior," and when they have, they usually invoke the reflex (see Miller, Galanter, and Pribram, 1960). There is no conceivable correspondence between a reflex and a dendron.

The computer/program metaphor suggests that a search for a psychological "machine language" – Fodor (1980) has called it "mentalese" – might be fruitful. If so, its code must correspond to the neural code described in terms of dendrons. Holonomic brain processing theory, developed from the holographic metaphor, addresses this issue. The theory is based on the finding that the elementary dendritic process, a receptive field property of visual and auditory cortex neurons, is best described in terms of Gabor elementary functions (Gabor, 1946; see above and review by Pribram, 1991, Lecture 2). Instead of the binary code which described Shannon's measure of information, information processing in the dendritic microstructure of the brain proceeds in terms of *quanta* of information. As described earlier, Gabor developed this measure to assess limits on the efficiency, or the minimum uncertainty with which human communication across the Atlantic cable can proceed. This limit was found to be defined by the same mathematics that Heisenberg had used to define quanta in microphysics. The quanta of informa-

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tion, the Gabor elementary functions, are thus measures that apply equally to microphysics, the operations of the material wetware of the brain in terms of the operations of dendrons, and the operations of mental communication in terms of the operations of psychons among human actors.

Just as in classical programming hierarchies, brain/mind processing structures at each level are transformed into those at the next level. For instance, the Gabor quanta responsible for image processing are assembled into group structures that are involved in object perception, and these, in turn, become organized into neural systems that categorize objects (see Pribram, 1991, for details). Still, something remains invariant across these transformations, or the process would fail to work. There is, therefore, a difference between surface structures which become *trans*-formed and the deeper identity which *in*-forms the transformation. Transformations are necessary to material and mental realizations – Plato's particular appearances – of the ideal in-forms. The instantiation of Beethoven's 9th Symphony is transformed from composition (a mental operation), to score (a material embodiment), to performance (more mental than material), to recording on compact disc (more material than mental), to the sensory and brain processes (material) that make for appreciative listening (mental). But the symphony as symphony remains recognizable, "identical" to Beethoven's creative composition over the centuries of performances, recordings, and listenings.

Instantiations depend on transformations among orders. What remains invariant across all instantiations is "in-formation," the form within. Surprisingly, according to this analysis, it is a Platonic "idealism" that motivates the information revolution (e.g., "information processing" approaches in cognitive and neural science) and distinguishes it from the materialism of the industrial revolution. Further, as information is neither material nor mental, idealism displaces not only materialism but also mentalism and dualism as the center of concern. Alternatively, a new tension is developed between Platonic idealism and Aristotelian realism perhaps in the guise of a Pythagorean-like structural pragmatism (see Khalil, 1990; Lévi-Strauss, 1978; Pribram, 1965) which investigates the relations among levels of inquiry by specifying the transformations that characterize the differences among their forms.

Along these lines, an ontological origin neutral to the mind/brain for information was shown (Pribram, 1986a) to resolve the apparent paradox of (a) invariance in ideal informational structure and (b) a plurality of instantiations. It was shown that to identify invariance solely as mental leads to awkward interpretations such as those that would endow computers with "minds" and "feelings." Instead, a plausible case was made that what remains invariant across transformations is neutral to the mind/brain, mental-material duality and is captured by physicists' definitions of energy

and the amount of its structure – entropy (and its converse, negentropy) and information. Information can be instantiated mentally as well as materially, an idea captured by the aphorism that, on occasion, the pen can be mightier than the sword.

Central to the view expressed above are measures of process, i.e. change. The efficiency with which the change proceeds is ordinarily measured in terms of entropy. The relation between measures of efficiency and measures of information (i.e., entropy and negentropy) has been discussed at length by Shannon (Shannon and Weaver, 1949), Brillouin (1962), and Mackay (1969). However, these authors came to somewhat different conclusions: Shannon equated the amount of information with the amount of entropy, Mackay and Brillouin with the amount of negentropy. A conciliation of these views comes from a modification which results in a definition of entropy as potential information. The reasoning is similar to that which motivated Shannon, who called the structure within which information processing occurs “uncertainty.” Recall here that Gabor’s quantum of information is a measure of the *minimum* uncertainty, that is the maximum amount of potential information that can be processed at any moment.

In addition, Shannon (Shannon and Weaver, 1949) and Lila Gatlin (1972) have noted that the efficiency of information processing depends on the presence of redundancy and, by virtue of pattern matching, on actively structuring redundancies. George Miller (1956) called attention to the importance of such structuring, which he called “chunking.” Elsewhere (Pribram, 1991, Lectures 8 and 9), I have reviewed the evidence that the frontolimbic portions of the forebrain are critically involved in structuring redundancy and by this means enhancing the efficiency of information processing. Thus, initial conditions, measured as an amount of uncertainty, and a controlling context, measured in terms of chunking, constrain the efficiency with which information is processed.

CODA: THE PARADOX OF CHOICE

Einstein in his famous aphorism, “I don’t believe God is playing dice with the universe,” voiced his concern as to whether measured change in physics could remain determinate in the face of quantum probability. Monod (1971), in his book *Chance and Necessity*, posed this same concern as a paradox that faces biological scientists. Are biological processes determined, or are they subject to the vagaries of the unpredictable? In the brain/behavioral sciences, the issue of free will has filled volumes of discourse.

In keeping with the theme of this chapter, I maintain that we need not make an either/or choice between freedom and determinism. By accepting the paradox as such, we simply specify the constraints under which freedom is displayed. When this is done, the full measuring of the paradox

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unfolds. Freedom of an action path is dependent upon the presence of a determinate structure.

It is the determinate structure of Einstein's dice that makes possible the probabilistic path initiated by a throw. It is the determinate structure of orbits that allows the probabilistic path of placement of electrons in quantum mechanics. And, in biology, though it is the genetic given in strands of DNA that determines the development of every part of the body, every tissue, every organ, development also depends on a path dictated by operations of inductors, derepressors of the DNA's potential.

The path of process is ordinarily quasi-predictable: "degrees of freedom" are specified by the constraining structures. For the most part, degrees of freedom can be determined, and thus the determinist will claim that, though the boundaries of determinism may be more fuzzy than had been anticipated, in principle determinism remains intact. But this view is placed in jeopardy when the boundaries become stretched sufficiently to constitute a paradigm shift. For the cross-level endeavors that must relate the natural to the social sciences, such a shift has provided important insights.

Much of natural science has flourished by modeling processes under an umbrella of determinate linear invertible equations. The paths described by a variety of least action principles are examples. The three-body problem is, however, a counter-example. In the social sciences, models so conceived are more often inadequate. If this were not so, more of us could quickly become wealthy by playing the stock market; wars would not be so frequently undertaken through miscalculation; setting up a business would entail considerably less risk.

The recent emergence and popularity of nonlinear dynamics attests to how a chaotic state can result from a deterministic path of a process. The danger accompanying any enthusiasm is that it overreaches the bounds of applicability and becomes "the answer" to everything, and therefore risks becoming an answer to nothing. Much of established knowledge, even in the social sciences, does deal with the predictable and the quasi-predictably probable. Predictions through polls are an example. Controlling constraints are often present that keep a process from becoming completely unmanageable. At the same time, interactions among people are, as Toynbee (1972) noted, challenges, not causes (see also Khalil, 1990). There is as yet no plausible science that can predict the results of a challenge.

For me, the most important observation that has come from the study of nonlinear processes is the observation made by Prigogine (1980) that stabilities can be formed far from equilibrium. I very much want to know whether such stabilities can form as a result of processes hitherto modelled in terms of least action paths. If, in fact, entropy can be thought of as potential information, the path to minimum entropy – that is to maximum information – should be strewn with stabilities far from equilibrium. Such stabilities should be formed by way of the coaction of ensembles of quanta

of information when these ensembles are cooperatively engaged within a controlling context.

To use the language of behavioral psychology, stabilities far from equilibrium ought to characterize the reinforcement process. Reinforcement is defined as a process that enhances the likelihood of recurrence of a response. In turn, a response is defined as an environmental outcome, an environmental event, produced by the organism. In order for the event (dictionary definition: *l. ex-venire*; outcome) to have the effect of enhancing the likelihood of recurrence of a similar (or identical) event, a stability must be engendered in the brain process that controls the behavior. During discrimination learning, such temporary stabilities are displayed as plateaus in the acquisition curve as each element composing the discrimination is attended (see review by Pribram, 1986b).

Learning, in primates at least, is thus seen to be a process of self-organization. The ordinary view that a reinforcer produces its effect by way of drive reduction or drive induction is readily abandoned when one is conversant with testing monkeys (or educating children). A monkey rewarded for making a correct choice will put the reinforcing peanut in his cheek pouch. When, however, he next makes an error, he pops the peanut out of his pouch and chews and swallows it with obvious relish. Should the occasion demand, I have seen monkeys work for hundreds of trials to solve a problem when the "reward machine" was broken. Self-organization, not self-indulgence, is the overarching motive in learning.

Computational and mathematical models of the learning process in these terms are well within our reach. Given such models, we might begin to understand the constraints necessary to control cooperative processes among ensembles of quanta of information – the cross-scale processes that engender the paradox of complexity.

NOTES

I have gained much from listening to "tutorials" presented by Peter Kugler on the topics of cross-scale science and nonlinear dynamics. Thanks also are due to Debbie Akers, without whose assistance this chapter would not have been accomplished.

- 1 The temporal dimension can be evoked by successively scanning across the grating (as, for instance, by walking across the path of illumination of a projection of a slide of such a grating). Conversion to the temporal dimension is, however, not necessary. The grating is a filter whose characteristics can be understood either as a spatial or a temporal modulation of a spectral frequency.

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