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Reflections on the Place of
Brain in the Ecology of Mind

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There seems to be good evidence for the age-old belief that the brain has something to do with the mind. Or, to use less dualistic terms, when behavioral phenomena are carved at their joints, there will be some sense in which the analysis will correspond to the way the brain is put together. Psychological problems may not be solved by making measurements on the brain; but some more modest aim may be accomplished. A psychological analysis that can stand up to the neurological evidence is certainly better than one that can not. The catch, obviously, is in the phrase "stand up to," since considerable prejudice can be involved in its definition. In any case, each time there is a new idea in psychology, it suggests a corresponding insight in neurophysiology, and vice versa. The procedure of looking back and forth between the two fields is not only ancient and honorable—it is always fun and occasionally useful

[Miller, Galanter, & Pribram, 1960, p. 196].

INTRODUCTION

The issues I wish to address stem directly from the conference that generated this volume. During the conference, I was, and still am, disturbed by the almost complete disregard, in otherwise excellent presentations by psychologists, of the contributions the brain sciences can make to the problems under investigation. Some of this disturbance is purely personal: Why should my proposals be repeatedly and universally quoted when they appear devoid of their neurological skin, flesh, and bones in *Plans and the Structure of Behavior* (Miller et al., 1960), but be totally ignored when instantiated and substantiated in *Languages of*

the Brain (Pribram, 1971)? Why is it that in this group of scientists, where communication is relatively easy because of mutual respect and trust, only the *philosopher* addresses the issues before us without regard to race (neuro vs. psycho)? Is psychology still so insecure, even here and at this late date, that it must (as Skinner and Freud both argued in that distant past) build on its own level without recourse to physiology? If that is the case, I asked myself, what am I doing here? It seems to matter little what I say; no one is listening anyway.

The answer to this question is, of course, the major contribution James Gibson has made to my understanding of phenomenal reality. But I want to insist that explorations of the issues he has raised can be enriched considerably by investigations of the nervous system. The question is whether in the long run any science can ignore its relationships with others without suffering obsolescence. Has the descriptive functionalism so dear to Gibson and other behaviorists outlived its usefulness?

I wish here to share my conference experiences, both the frustrating and the rewarding, and shall try for once to keep references to the nervous system at a minimum without the detail that might otherwise bring down those defensive gates that psychologists are apparently so prone to use. But to the brain, I do and must refer, for whether you believe it or not, that's where the action is, as the younger generation is wont to remark.

REPRESENTATION

The central issue before the conference turned on the question of the existence of *brain* representations and their nature. Surprisingly, *everyone*—and I mean *everyone*—agreed that representational mechanisms exist, though not everyone wanted to label them as such, and there was considerable range in attributing their importance. Thus, for Gibson “Sensations triggered by light, sound, pressure and chemicals are merely incidental,—*information* is available to a perceptual system and—the qualities of the world in relation to the needs of the observer are experienced directly [this volume].” But then Gibson goes on to suggest

that when we view phenomena in terms of e.g., a special sense, the process of attention occurs at centers within the nervous system, whereas [when we view a phenomenon as a] case of a perceptual system attention pervades the whole input-output loop. In the first case attention is a consciousness that can be focused, in the second case it is a skill that can be educated. In the first case physiological metaphors are used such as the filtering of nervous impulses or the switching of impulses from one path to another. In the second case the metaphors used can be terms like resonating, extracting, optimizing or symmetricalizing, and acts like orienting, exploring, investigating or adjusting.”

Let us examine these quotations closely (as examples of a wider range of pronouncements) with respect to the issue of representation. In the first case

"sensations" appear although they are incidental. Just how are sensations constituted except by a match between the input to the senses (or even perceptual systems) and a brain mechanism ready to process this input? (See Pribram, [1971] Chaps. 3, 6, 12 for the gory—i.e., bloody—biological details.) Then, as an alternative comes "information" available to perceptual systems. Does "information" exist in the absence of an informed (uncertain) organism? Here I use the term in the strict (measurement) sense as well as in the more informal Gibsonian sense. To be informed is to be in-formed, literally formed within. Has Gibson really begged the question by equating external pattern, structure, and organization with information—much as those who insist that the perturbations produced by a falling tree make phenomenal sounds even in the absence of an acoustically sentient observer?

Figure 17.1 makes my point in brain terms. It diagrams the response of a cell in the visual cortex of a cat. The experiment was performed in my laboratory and reported by Erich Sutter (1975). The cell's responses were correlated with the appearance of points of light on an oscilloscope. However, the points of light were not arranged in an elongated fashion. They were displayed randomly with respect to place and time over the entire oscilloscope face. When the correlation encompassed approximately 30 msec, the elongated receptive field of the cell becomes apparent; at 40 msec of correlation, its inhibitory flank is demonstrated.

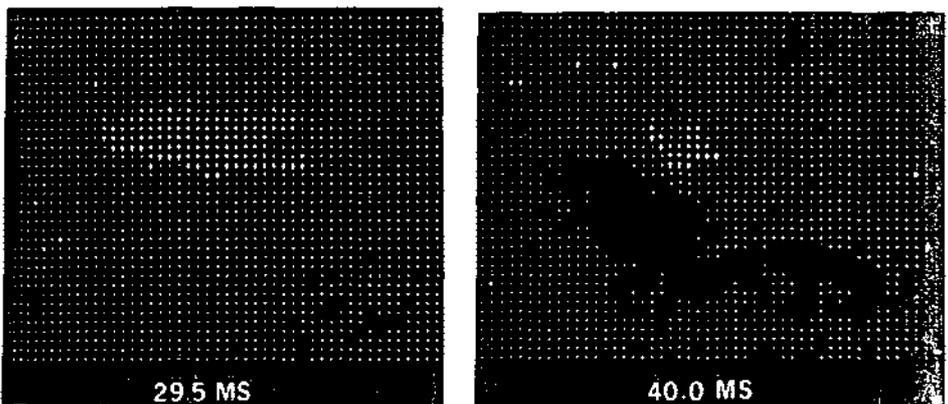


FIG. 17.1. Computer print-out of the shape of the receptive field of a cell in the visual cortex of a curarized cat responding to visual white noise on an oscilloscope face. Intensified spots indicate a correlation between the appearance of a spot on the oscilloscope face and the firing of the cell above baseline. Absent spots indicate that when a spot appeared on the oscilloscope face, the visual cortical cell's firing rate was depressed below baseline. A. The record obtained over 29.5 msec of correlation. B. The record obtained over 40 msec of correlation. Note that the generation of the inhibitory flank takes approximately 10 msec longer than the generation of the excitatory field, suggesting that inhibition is secondary to excitation (and probably produced intracortically). For complete report, see Sutter (1975).

These characteristics describe this cell's receptive field when ordinary techniques (such as presenting a line in a specific orientation) are used to demonstrate the cell's feature sensitivity. But the cell, by itself, cannot distinguish a line from the visual white noise presented to it on the oscilloscope. Gibson is correct that only the system's network properties can make this distinction. But how can Gibson and his colleagues, *ex cathedra*, be so certain that the relevant organization ("information") is "out there" when his (and their) own brain cells can't distinguish a line from visual noise? For unless it has a nonphysiological basis, it is the connections of these very brain cells that must be involved in constructing whatever "information" is being processed, and the information might just as well "reside" in these brain connectivities as in the environment. My own view, repeatedly documented with neurophysiological data (as noted earlier), is that the organization in the environment must match some "representational" organization in the organism. So let us return to the central issue of the conference and review some evidence once again in the context of Gibson's approach.

Consider Gibson's statement about qualities being perceived directly, but note that he adds the phrase "in relation to the needs of the observer." The term *needs* can refer to two types of states: (1) those "physiological" needs that we ordinarily conceive as being determined by physicochemical stimuli impinging on the homeostatic mechanisms of the brain stem; and (2) those that are more "psychological" in nature, such as the "need" to investigate due to uncertainty (used here in its strict definition) generated by information (strict definition) in the input. The homeostatic mechanisms are representational in that the quantities they control are being sensed by the controlling system. Note that sensing is here equated with a form of representation (more about the theoretical validity of this later). Here, the evidence is concrete: The quantities to be sensed are in many instances literally absorbed by the sensing mechanism, and the absorbed portions are acted upon by subordinate regulating mechanisms to determine the set points around which the homeostats then operate (for review, see, e.g., Pribram, 1977b). Thus these are true representations in a most precise meaning of the term.

The more "psychological" needs, such as curiosity, also involve representational mechanisms. The term *need* is invoked when an organism varies its responses in the absence of observable covariation in the environment: A varying internal organization is inferred to account for varying behavior. Investigative behavior shows such variations and thus the conception of an information-hungry organism, an *informavore* whose appetitive fervor can be sated only to arise once more (Pribram, 1960). Information in its strict definition implies a set of alternatives about which the information is informative. When this set of alternatives describes the channel competency (or its inverse, the equivocation inherent in the channel) of the organism—its ability to process information—it constitutes its uncertainty. Information describing the set of alternatives (whether Shannonian [Shannon & Weaver, 1949] or Gibsonian) must address this uncertainty, as even

Gibson admits in his statement that information "is perceived *in relation* to the needs of the observer." The set that describes the uncertainty of the organism must, of course, constitute some representation. Perception, whether within a special sensory mode or beyond it, cannot take place without that specified *relation* between organism and environment. Consider an analogy: Humans do not perceive most of porpoise communication because we have no mechanism attuned to the information carried at 20 KHz and above. Thus no uncertainties were set, nor did we become curious, nor did investigatory behavior begin until John Lilly played a recording of porpoise communication at half speed, making it audible to us. Only then did the wealth of information (strict definition) that is now perceived in their communication make us uncertain as to the meaning of the information (Gibson's sense).

Further, the set of alternatives that describes the uncertainty of the organism is often referred to as "attention," and whether focused or skilled, Gibson admits to the existence of attention. My laboratory has devoted considerable attention to attention, demonstrating three major neural control systems that operate on the "focusing" of attention (Pribram & McGuinness, 1975) and showing by making electrical brain recordings that the association cortex is definitively involved in organizing attentional skills (Nuwer & Pribram, 1979; Pribram, Day, & Johnston, 1976; Rothblat & Pribram, 1972). Figure 17.2 diagrams the control systems, and Fig. 17.3 gives an example of the brain electrical activity involved in attentional skill. How could attention be controlled or attentional skill become developed if the attentional mechanisms were not apprised of what (information) was to be attended to? How could attention operate without some representational brain process upon which to operate?

Gibson creates a conundrum with his use of phrases such as "resonating, optimizing, symmetricalizing" with respect to perceiving, and "orienting, exploring, investigating or adjusting" with respect to acting. It appears that the brain that Gibson envisions can do none of these, yet current neurophysiology is concerned almost exclusively with just such mechanisms. Figure 17.4 illustrates the tuning curves of cells in the visual cortex to spatial frequencies. The cells are the same cells that respond selectively to lines presented in specific orientations. The sensitivities appear to be influenced as well by the number of lines and their relative widths and spacings. Thus the cells can be shown to "resonate" to one or another octave of the spatial frequency spectrum—the spectrum of light and dark that patterns the visual world. Optical engineers and computer scientists describe the result of such resonances as "image" or optical information processing to distinguish it from the processing of alternatives as performed by digital computers. Terms such as "optimizing" and recourse to symmetry analyses are commonplace, and I have repeatedly drawn the parallel between holograms that encode spatial frequency and certain aspects of brain function in perception and memory (Pribram, 1966; 1971; Pribram, Nuwer, & Baron, 1974). Is the notion of a brain process so irrelevant that even when the processes are shown to do

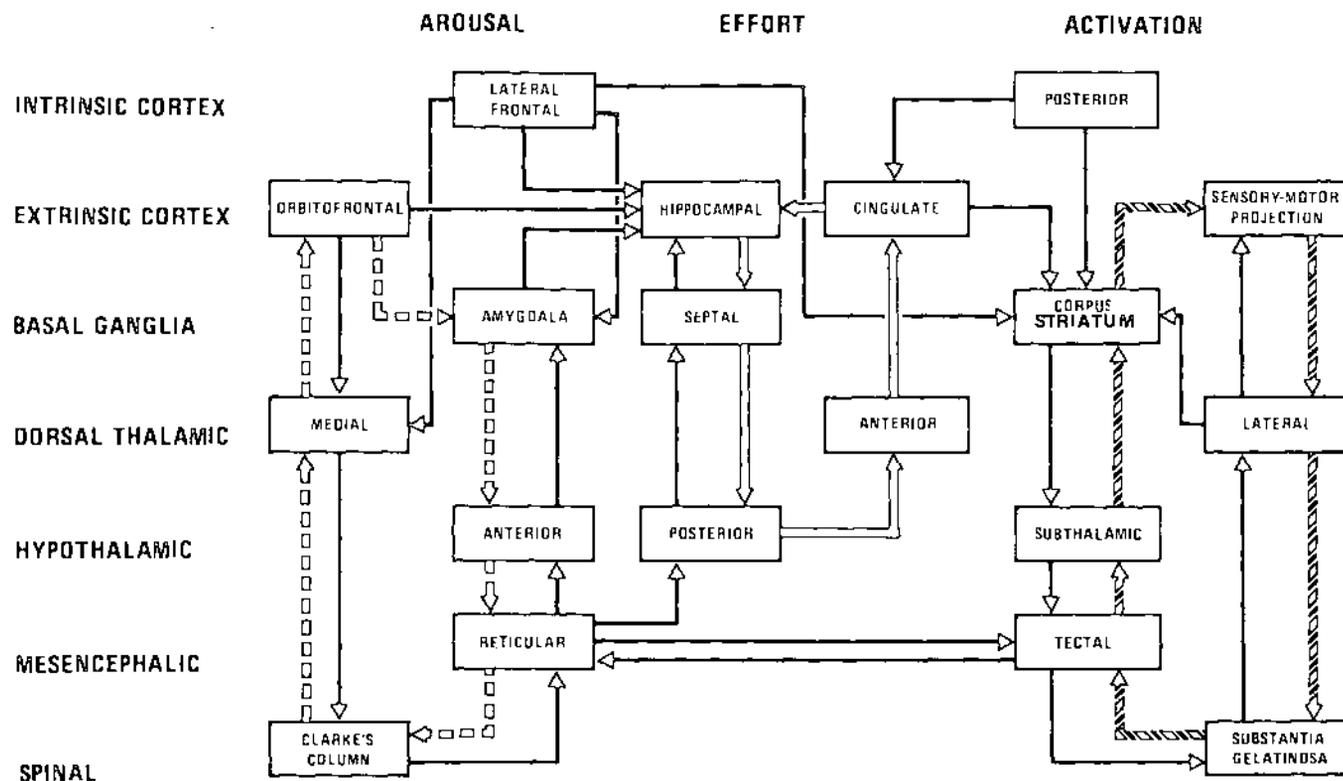


FIG. 17.2. Diagram of the three neural systems involved in the control of attention. These systems can be discerned neuroanatomically, neurochemically, and by the results of psychophysiological, neurophysiological, and neurobehavioral experiment. For complete review of the evidence, see Doherty and McGuinness (1975).

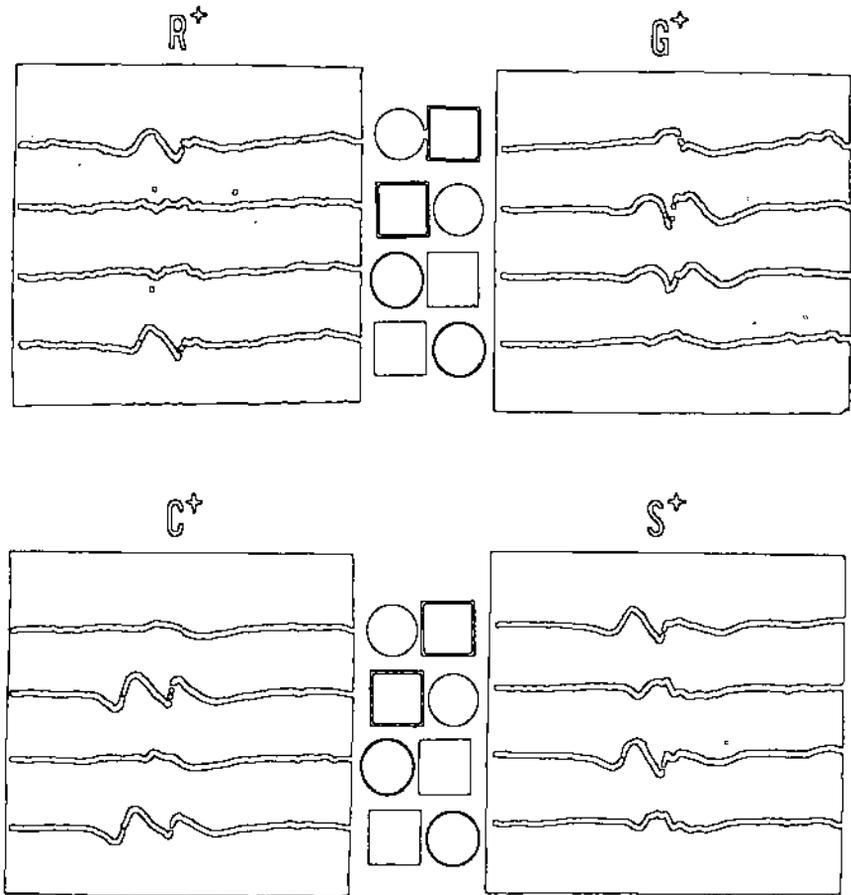
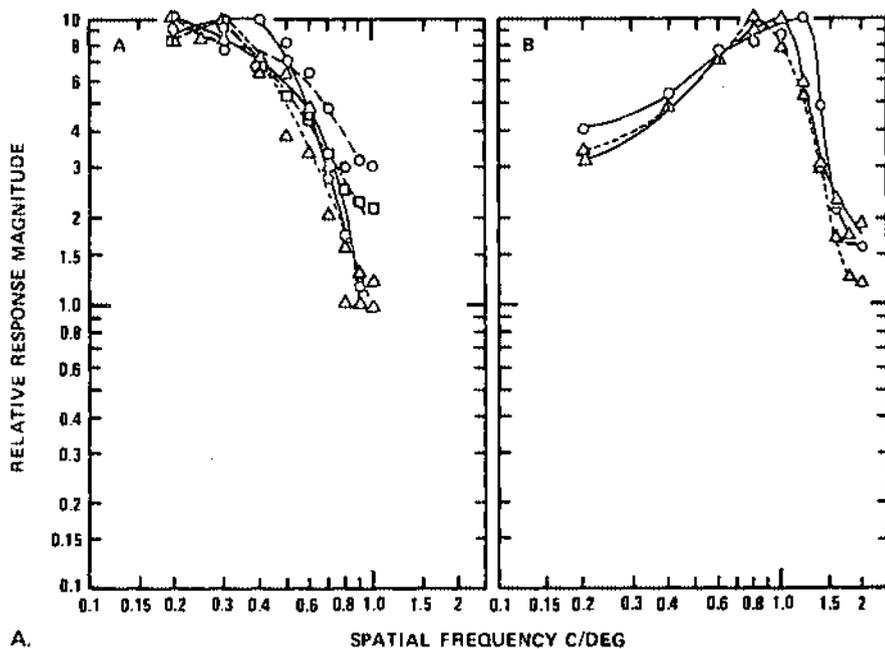


FIG. 17.3. Records of averaged electrical responses obtained from the inferotemporal cortex in the fully awake monkey. The averages are run both backward and forward from the midpoint of the record. The geometric figures represent slides that were briefly back-projected in random sequence, one at a time onto a vertically split panel. The monkey responded (i.e., paid attention) on the basis of the reinforcing contingencies that rewarded either one of the colors (red or green—represented here by black and white). Note that when, as in the upper panels, colors are being reinforced, the brain waves look alike when the colors appear on the same side irrespective of shape. When shape is being reinforced (lower panels), the brain waves look alike when the same shape appears in the same place irrespective of which color the shapes are.

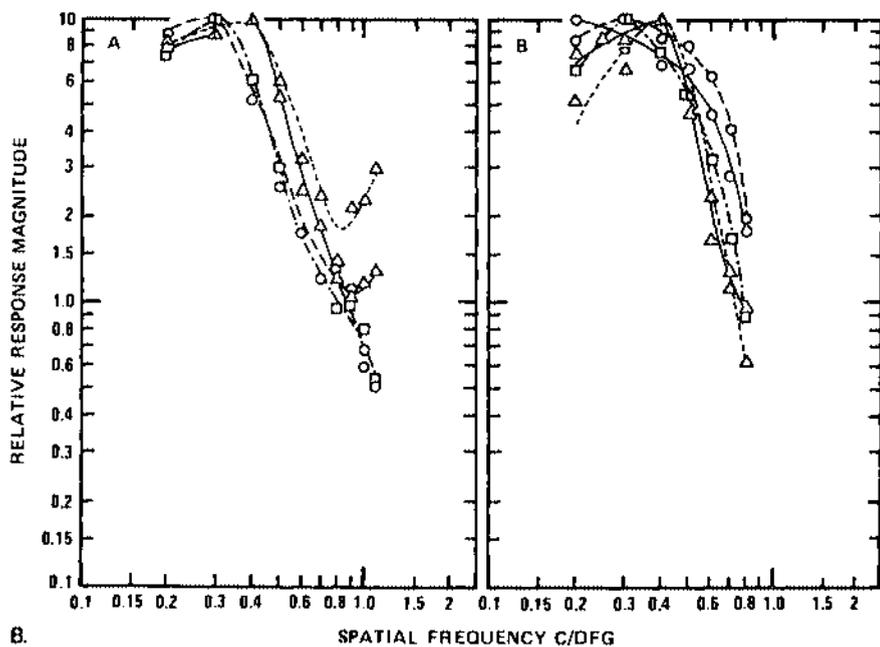
exactly what is needed, they must be denied lest they do violence to the presumed virginal purity of psychological inquiry? Or does Gibson avoid discussion of neural processes because he has some objection to available theory and data? If so, why does he not at least mention it to argue against it?

COMPLEX-SUSTAINED CELLS



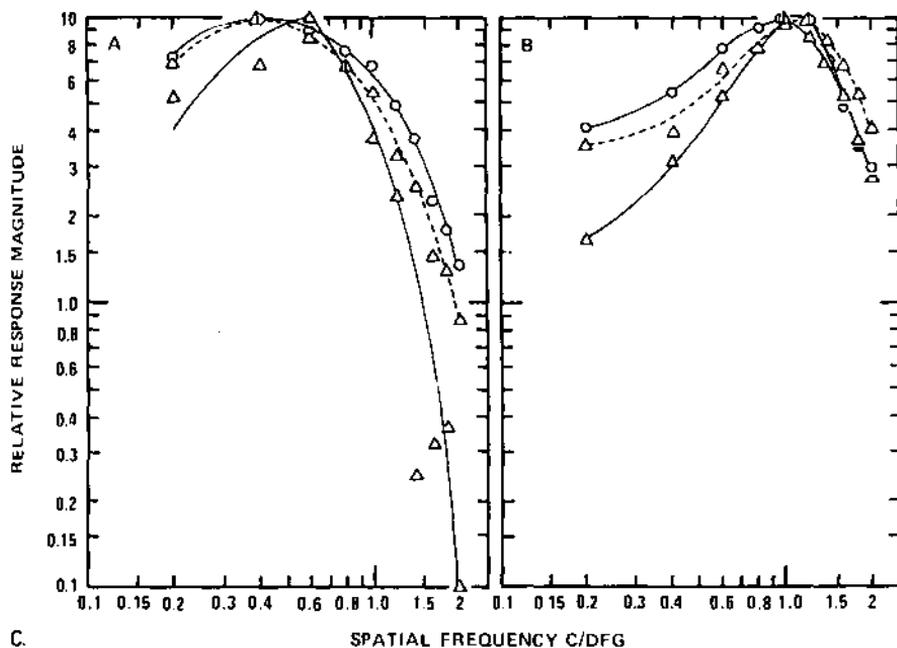
A.

COMPLEX-TRANSIENT CELLS



B.

SIMPLE-SUSTAINED CELLS



SIMPLE-TRANSIENT CELLS

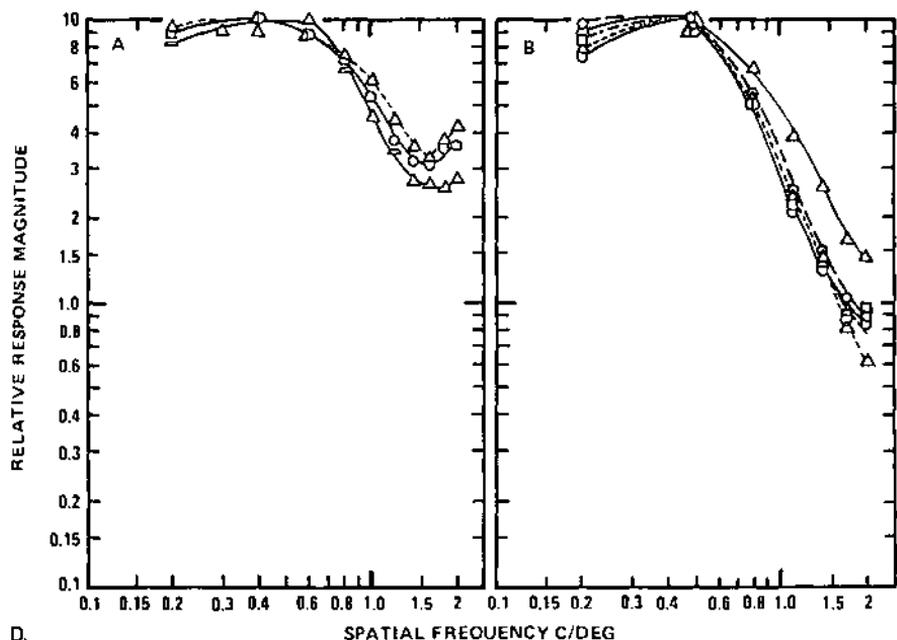


FIG. 17.4. Tuning curves of cells in the visual cortex of cats responding to suprathreshold drifting gratings of a variety of spatial frequencies presented in random sequence. Note the stability of the curves over time and a variety of conditions (represented by the different curves plotted in each graph).

I have already discussed "exploring" and "investigating," two of the action terms Gibson invokes. Ten years of research on the brain mechanisms involved in the orienting reaction (Bagshaw & Benzies, 1968; Bagshaw, Kimble, & Pribram, 1965; Kimble, Bagshaw, & Pribram, 1965; Pribram & McGuinness, 1975; Pribram, Reitz, McNeil, & Spevack, 1979) should be relevant to anyone interested in orienting, as should the earlier work on the neural processes allowing orienting to occur (see, e.g., the reviews by Horn & Hinde, 1970; and by Groves & Thompson, 1970). And certainly the exquisite work on the neural feedback and feedforward mechanisms operating to adjust to changes of load in both sensory and motor systems is relevant (for a review, see Pribram, 1971, Chaps. 12, 13).

One objection that the Gibsonian might raise to the relevance of such data must be dismissed in advance. Neurological data might be dismissed because they are at too fine grained a level of analysis to be relevant to some levels of analysis but this does not hold for an ecological approach. Further, Gibson's emphasis upon perception as the pickup of (ecologically relevant) information *over time* cannot be used to dismiss neurological data because such data are assumed to refer only to momentary, "frozen image," or static analyses, (as e.g. proposed by "snippeting" theorists such as Richard Gregory, 1966). Current neurophysiology deals with processes such as "orienting, exploring, investigating or adjusting" over time exactly as Gibson's account requires. Sooner or later his account must either incorporate such data or state specifically how it is incompatible with his ecological analysis.

COMPLEMENTATION

The subject of action brings us to the excellent contributions of Turvey and his colleagues. These investigators show less reluctance to mention the nervous system. For example, "spinal interneuronal pools" are invoked to handle Gelfand, Gurfinkel, Tsetlin, and Shik's (1971) observations. Statements such as "This approach yields *some* of the organizing problem to the environment" indicates that other aspects are left to the organism to handle. Thus Turvey's approach is more compatible with mine in that his ecology includes *specifications* of organism (especially neural) variables and constraints, whereas Gibson tends to leave the organism, if not empty, apparently stuffed with foam rubber. Moreover, Turvey's discussions in terms of free variables (information in the strict sense), constraints (redundancy and control; see, e.g., Pribram & Gill, 1976, Chap. 1), and coordinate structures are completely compatible with my own approach. To view muscles (especially the vocal muscles) as vibratory systems, to take one specific example, as did Carol Fowler in her presentation, made me nostalgic (Pribram, 1971):

"I love you." It was spring in Paris, and the words held the delightful flavor of a Scandinavian accent. The occasion was a UNESCO meeting on the problems of research on Brain and Human Behavior. The fateful words were not spoken by a curvaceous blonde beauty, however, but generated by a small shiny metal device in the hands of a famous psycholinguist.

The device impressed all of us with the simplicity of its design. The loudspeaker was controlled by only two knobs. One altered the state of an electronic circuit that represented the tension of the vocal cords; the other regulated the pulses generated by a circuit that simulated the plosions of air puffs striking the cords.

Could this simple device be relevant to man's study of himself? Might not all behavior be generated and controlled by a neural mechanism equally simple? Is the nervous system a "two knob" dual process mechanism in which one process is expressed in terms of neuroelectric states, the other in terms of distinct pulsatile operators on those states? That the nervous system does, in fact, operate by impulses has been well documented. The existence of neuroelectric states in the brain has also been established, but this evidence and its significance to the study of psychology has been slow to gain acceptance even in neurophysiology. This first chapter therefore examines the evidence which makes a two-process model of brain function plausible [pp. 3-5].

This compatibility of viewpoint and approach also provides a rich field for an appraisal of differences. For the main, these differences are in the detail with which each conceptualization is thus far developed. As an example of an important detail which has moved my thinking forward, take the following: An *overall* structure of the mechanism of control is already presented in *Plans and the Structure of Behavior* (Miller et al., 1960), *Languages of the Brain* (Pribram, 1971, Chap. 5), and *Freud's "Project" Re-assessed* (Pribram & Gill, 1976). Thus the organization of behavior, its serial ordering, is due not to the chaining of movements but to the differentiation, the decoding, of an already formed *spatial* configuration. The neural specification of such spatial configuration (see Fig. 17.5) is described more fully in Chapter 12 of *Languages of the Brain* (Pribram, 1971). The conception of hierarchical relationship among coordinate structures (although they are called "predictive representations") is detailed in Chapter 16 and is summarized:

Given (1) that the neural mechanism "because of its selective control over its own modification, allows a change in representation to occur over successions of trials," and (2) that whenever "complete match between representation and input is not achieved the representation is modified to include this information and trials continue . . . until corrective change of the representation no longer occurs," then any succession of *predictive* representations in essence constitutes a program or Plan producing an intent.

In review, achievements are organized performances and steps toward an achievement theory of performance have been taken. These steps account for the

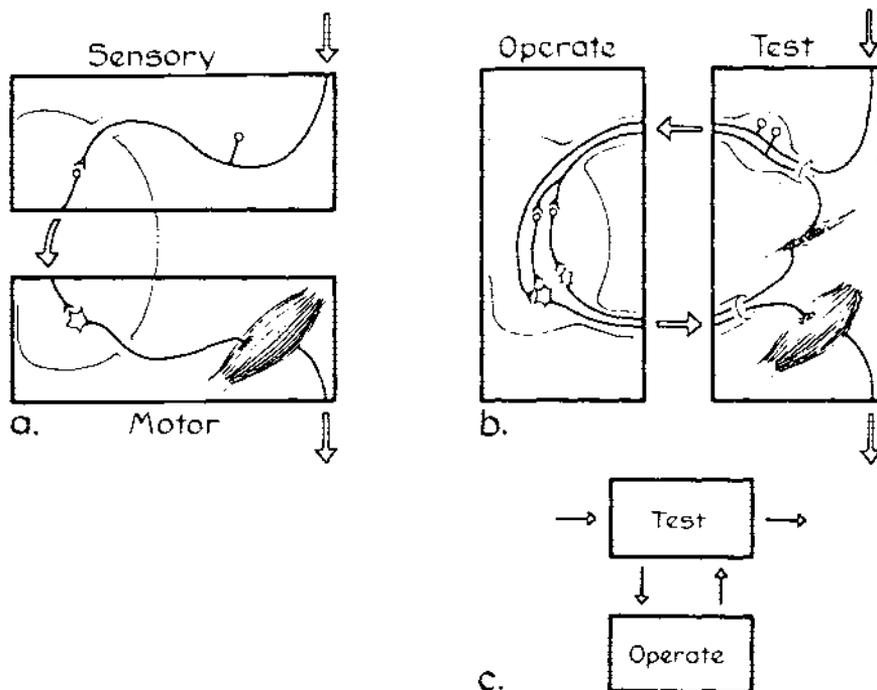


FIG. 17.5. Representation of the organization of control in a low-level (reflex) coordinate structure according to the Test-Operate-Test-Exit (TOTE) system. (a) The old reflex arc; (b) the TOTE feedback loop based on the γ efferents; (c) the TOTE representation. Note in b how the two types of γ efferents interact to produce an adaptive response. For complete details, see Pribram (1971), Chap., 13).

differences in function of reinforcers during learning (when they provide information) and during performance (when they value, bias behavior). At least one class of variables, response rate, has been shown to play a major part in determining the contingencies under which an organism no longer learns yet continues to perform—the means-end reversal. Performances achieve because of the hierarchical nature of the reinforcing (in stimulus language, the discriminative) process: Meanings are derived when information is hierarchically processed in sensory systems, and Plans, intentions, are constructed by hierarchical processing in the predictive motor mechanism [pp. 299-300].

Missing is a detailed statement as to the nature of the predictive representation. Carol Fowler's view of muscles as vibratory systems makes explicit the nature of the state variables which are noted in the foregoing quotation. By treating the muscular system as basically vibratory, Bekesy's classical experiments on the auditory and somatosensory mechanism become relevant to motor function and the conception of a holographically (i.e., n -dimensional frequency

coded) constituted Image of Achievement (Pribram, 1971, Chap. 13) more plausible. Neurophysiological evidence for vibratory activity or control of the entire motor system (other than in vocalization, eye movement, scratch reflex, and locomotion where it is obvious) now needs to be sought experimentally.

Despite this compatibility of approaches a fundamental difference between Turvey's views and mine was voiced at the meetings. Turvey suggested that the relationship between the neural mechanism and the environment that is involved in guiding motor behavior should be regarded as a complementation rather than a representation. My reply was that complementation is a form of representation. This point is both difficult and important. Complementation suggests something akin to a *mirror* image, whereas representation suggests an image that is more of a duplicate of what is imaged. The essential point is that in complementation, both organism and environment mutually imply each other. Thus the acquisition of a motor skill (or, for Gibson, a perceptual skill) is an attunement, in which the organism becomes tuned to the environment. With all this, I agree totally. But I would add that a skill results just as often from the attunement of the environment to the organism: the development of skis, skates, skateboards, bicycles, tennis racquets, balls, bats, violins, cellos, trumpets, saxophones, drums, pianos, harps, automobiles, and steam shovels (to name only a few common artifacts that are involved in skilled performances). Thus complementation is not limited to the form proposed by Turvey. Further, the figure-ground relationship was used as a model of complementation, and we know from studies using reversible figures that many figure-ground reversals can take place. In accord with Shaw's suggestion (see later), these circumstances are most likely described by symmetry relationships.

The question may therefore be posed: Does complementation entail representation? In *Languages of the Brain* (1971, Chap. 13), evidence is presented in support of the hypothesis that acts, not movements, are *represented* in the cerebral motor cortex. Acts are defined as the environmental consequences of movements—specifically, the forces engendered by and acting upon the muscular system. Turvey and his group define the coordinations involved in coordinate structures as follows: "It is neither muscles nor joints that are coordinated in the performance of skills, but forces, those supplied by the actor and those supplied by the environment." Experiments on the functions of the motor cortex (Malis, Pribram, & Kruger, 1953; Pribram, Kruger, Robinson, & Berman, 1955) demonstrated that "neither muscle contractions nor movements (defined as sequences of muscle contractions coordinated around joints) were impaired" by extensive resections. However, *specific skills were impaired*, thus the inference was made that this cortex was involved in actions, defined as the environmental consequences of the movement. The cerebral motor cortex is therefore a critical party to the formation and maintenance of coordinate structures as defined both by Gelfand, Gurfinkle, Tsetlin, and Shik (1971) and by Turvey (this vol.). Could not one conclude, on the basis of this evidence, that a *representation of coordi-*

nate structures is formed by virtue of the functions of the motor cortex? A representation encodes the invariances, the constraints, the parameters that describe the function. Such a representation makes possible environment-organism complementation instantiated in the skill. Note that I did not suggest that the representation of the coordinate structure was to be totally localized "in" the cerebral motor cortex—although this is not yet ruled out by the evidence. It is a more likely option that a number of other brain structures (such as the basal ganglia and cerebellum) are involved, and of course, they, in turn, control more peripheral motor organizations.

An important clarification needs to be made here. Whenever we talk of the localization of a function, there are two sides to the coin—and the two sides differ. The lungs are clearly involved in respiration. When, however, we begin to investigate respiratory functions, oxygen, carbon dioxide, lungs, red blood cells, membrane properties, hemoglobin, the brain stem respiratory control mechanism, and even temperature and gastric acidity (among other factors) become involved. What is localized in the lungs is the exchange of oxygen and carbon dioxide, and this property is critical to respiration. In biology we do not throw up our hands in despair and abandon the search for localized mechanisms because respiration is complex and involves the function of many structures. We do not in biological ecology eschew a search for the mechanisms by which organisms become attuned to their environments. Psychological ecology—the ecology of mind—need be no different. Precise mechanisms are localized in the cerebral motor cortex. They are critical to coordinate structures and may in fact be the embodiment of the most superordinate of these in that they encode the complex of relationships among the forces that describe an act.

How can the consequences of movements—the forces engendered in an existing field of forces—be encoded? Consider once again that the motor system functions as a vibratory system. The forces describing the action would fluctuate, and the fluctuations would occur with specifiable frequencies. This is essentially what Bernstein (1967) found in his elegant experiments. Bernstein was able to make predictions of the course of actions by performing Fourier analyses on the records he obtained. I suggested (Pribram, 1971) that what Bernstein could do, perhaps his *brain* might do; and if his, then ours as well. We have recently completed experiments that provide evidence in support of the hypothesis that the motor system behaves as a frequency analyzer of the forces involved in actions (Ahmad Sharafat 1981.) Thus the motor system can be seen to function much as the auditory system, the somatosensory system (Bekesy, 1967), and the visual system (Pribram, 1971, Chap. 8).

There remains the question of whether this complementation effected by the functions of the cerebral motor cortex provides evidence for a representation. In my contribution (Pribram, 1977c) to the previous conference and volume of this series, I detail evidence for isomorphism between environment and cortex in perceptual systems. Surprisingly, this evidence points to an order in the physical

world that theoretical physicist David Bohm (1973) calls the implicate order and that is not the perceived world of appearance. The question is therefore raised as to whether complementation exists between the organism and an implicate order in the environment or between the organism and the explicate order of appearance. Or should the question be rephrased to ask whether complementation describes the relationship between implicate and explicate orders, whether in the environment or in the organism? I opt for this latter view because both Bohm and I were led to prehending the implicate order through the hologram. Holograms encode the frequency domain, yet because of symmetry relations (complementation) they can be readily decoded. Such decoding into the perceived world of images and objects can be effected by the operation of the feedbacks and feed-forwards that constitute coordinate structures. But I would add that what is complemented between organism and environment might be the frequency domain in one and the image-object domain in the other.

My conclusion, therefore, is that representations do exist in the brain; that they are instrumental in complementation, and that some representations encode, in the frequency domain, coordinate structures and thus complement the image-object domain. Complementation therefore describes the symmetry relationships (reversibility of transformations) between these domains and perhaps between one domain in the organism and the other in the environment. There may, of course, be other domains over which such relationships hold, but at present these have not been studied.

DUALITY OF IMAGE AND PLAN

Shaw's penetrating analysis of complementation suggests that dualities of this sort are the rule rather than the exception. I have been intrigued for years with the commonalities of description that make up the body of general systems theory, but have deplored the overly simplistic assumption that when two processes can be described by the same equation, they can be equated structurally. Something very important is missed when the interfaces between "levels" are ignored. So often the transformations from one level to another seemed to result in "mirror images" rather than similarities. The transformation from the image/object domain to the frequency (holographic) domain and back again by way of the inverse transform is a case in point. Even the term *mirror image* does not really convey the near identity yet vast difference between the two domains. I have used the term "stereo-isomer" (Pribram, 1965) because the functional properties of each isomer are often so different, but a more general conceptual language is needed. In his discussion of the variety of "dualities," Shaw has provided such a language.

One duality that is not recognized as such is the duality between what Patten (1971) has called the dynamical and the linguistic (or structural) modes of or-

ganization, which in *Plans and the Structure of Behavior* (Miller et al 1960) were called Image and Plan. We pointed out then that Image and Plan mutually imply each other. But it is one thing to provide an intuition that a duality is involved and another to show how the duality might come about. David Bohm suggested once that perhaps two holographic (dynamical, Image) organizations orthogonal to each other might, at their intersection, produce the nonlinearities that give rise to structure (linguistic, Plan). Rene Thom (1975) has developed the theme that continuous topological representations can, under certain conditions (constraints), give rise to "catastrophes"—the discontinuities that form nonlinear structures. Anderson (e.g., Anderson, Silverstein, Ritz, & Jones, 1977) has made the most precise suggestion as to how such a process might work. His model of the dynamical Image domain is based on Walsh transforms, which are finite, discrete, two-valued analogues of Fourier transforms. His model therefore also results in a holographic, dynamically distributed organization. Since its variables are discrete, however, he can represent this organization by a lattice. He defines "features," the elements of structure, as the eigenvectors of this lattice. A similar approach might be taken by using Lie groups (see, e.g., Hoffman, 1947) in which continuous variables can be represented. Whether these continuous variables are to be thought of as Fourier components of wave forms remains to be shown empirically in each specific situation.

SENTIENCE, SAPIENCE, AND SELFHOOD

The foregoing considerations were derived for most of us from experimental results that we could not explain in any more ordinary fashion. But of course, the novelty of this general approach has both philosophical roots and implications. Weimer has admirably drawn out both. He addresses the ultimate duality (the mind-brain problems) under the rubrics sentience, sapience, and selfhood. Once again I "resonate" to the views expressed since they "complement" my own. In "*Proposal for a Structural Pragmatism*" (1965), I suggested that a systems approach (but not general systems theory) to the mind-brain duality leads to a view in which mind and brain may be conceived as optical isomers of one another—brain being the product of a descriptive reductionist, whereas mind is the result of a relativistic conventional approach. Brain talk results from looking downward in a hierarchy of conceptualizations; mind talk, from looking upward.

The empirical problems of the organization of sentience yield definite answers once the dynamical holographic model is taken seriously. In a profession of reviews of pertinent neurophysiological data, I first proposed holography as a metaphor in "*Some Dimensions of Remembering*" (1966) and developed it into an analogy in *Languages of the Brain* (1971) and finally into a full-fledged model in the *Holographic Hypothesis of Brain Function in Perception and Memory* (Pribram, Nuwer & Baron 1974). The place of the model in sentience is

detailed in "Problems Concerning the Structure of Consciousness" (1976b) and in "Some Comments on the Nature of the Perceived Universe" (1977c). Weimer amplifies considerably the nonneurological aspects of the theory presented in these reviews. Of special interest to me was the reference to Leibniz' *Monadology* (Lettvin et al. 1959, 1961, 1968). My attention to monads was first drawn by an article entitled "A Code in the Nose" by Lettvin, Gesteland, Pitts, and Chung (1968). Having pioneered the feature detector theory in "What the Frog's Eye Tells the Brain" (1959), Lettvin proceeded to use the same microelectrode techniques to explore the olfactory bulb. Contrary to what he had found in the frog's tectum, the olfactory system did not seem to detect any specific features. Rather, sets of neurons appeared to respond to a large range of olfactants. Lettvin used these data to argue that his earlier feature detection views had limited applications in explaining perception, an argument that went largely unheeded by the neuroscience community. He was puzzled by his results, which he said appeared to suggest a code similar in organization to the monads in the *Monadology*. The work of Rall (e.g., 1970) and of Gordon Shepherd, reviewed in his *Synaptic Organization of the Brain* (1974), has detailed the similarities in the organization of the olfactory bulb with those of the retina and brain structures such as the thalamus and cortex. The studies of Freeman (1975), of Békésy (1967), of Hartline and Ratliff (see, e.g., Ratliff, *Mach Bands*, 1965), and of Campbell and Robson (1968) have provided the quantitative data and mathematical descriptions that support Lettvin's intuition. It is of course Leibniz' mathematics, his invention of the integral calculus, that led him to his views; and it is the same mathematics that describes the functions of the synaptic networks of receptors and brain—the very same mathematics that Gabor (1969) used to invent the hologram.

I discuss the neuropsychology of sapience in "Neurological Notes on Knowing" (1972), "The Comparative Psychology of Communication: The Issue of Grammar and Meaning" (1973), *Language in a Sociobiological Frame* (1976a), and "In Search of the Elusive Semiotic" (1977a). It is interesting that with respect to knowing, Gibson's position is very similar to the one I derived from neurobehavioral experiments. Gibson speaks of mediated "secondhand information" acting back on the mediate "firsthand pickup"; my experiments show that the intrinsic (association) cortex functions by way of an output that preprocesses the information flow in the primary sensory projection systems before that flow reaches the cortex. My conclusion, because of the preprocessing nature of this interaction between mediated "secondhand" and mediate "firsthand" information, is more Kantian than Gibson's. I believe the evidence points to a much tighter interweaving of noumena and sensation within perception than Gibson allows, although one might have thought that the thrust of *The Senses Considered as Perceptual Systems* (Gibson, 1966) would have taken Gibson in this direction. When Gibson says that knowledge does not come from anywhere within, but from looking, listening, feeling, smelling, and tasting, he assumes

that these activities by the organism are innately unstructured and only become structured as a result of experience. I have already presented here some of the evidence that makes me suspect that Gibson's assumption is wrong, although I would not go as far as Lashley, who once told me that he believed there was no organization ("information," Gibson's definition) at all in the environment and that it is the organism's brain that organizes the buzzing, blooming confusion of the universe. I think the evidence that is subsumed under the theory of evolution strongly suggests that the extreme views of Gibson and Lashley are wrong and that both organism and environment contribute to that organization we perceive as information and image/object.

I think we can, as Weimer details so clearly, go considerably further. His systems approach to duality and symmetry draws out the distinction between causes and reasons. He suggests that within a system, *causes* operate; between systems, *reasons* (which he defines as invariance relationships, harmonies) operate. Though close, I don't believe this is exactly correct (e.g., though we might claim that atomic number makes the periodic table of elements reasonable, we also ascribe the cause of the periodic table to atomic number). Rather, I would propose that in a hierarchy of systems, when the scientific analysis procedure is downward (thus reductive and descriptive), causal mechanisms are sought. On the other hand, whenever our investigations are directed upward (synthesis) in the hierarchy (as they are in thermodynamics, relativity theory, and some aspects of nuclear and quantum theory), then—as in the ecological approach to perception—it is reasons and invariances, not causes, that are the appropriate relationship terms.

It is this looking upward that also marks the presence of selfhood. In "Self-Consciousness and Intentionality" (1976c), I present neuropsychological evidence that bears on the problems posed by Brentano (1960) and William James (1950). Brentano defined "intentional inexistence" as the criterion for selfhood. Whenever we can separate our intentions from accomplished acts and our intentions and images from objects, we become self-conscious beings. Parenthetically, Brentano stated that explorations of consciousness were the province of psychology that would not yield to physiological analysis while nonconscious processes were physiologically determined. In a prescient footnote, however, he added the caveat that even conscious processes would yield to physiology if Leibniz' Monadology provided to be correct!

The model I have derived for self-consciousness is based on the feedforward, open-loop helical mechanism. Feedforwards are created whenever the tests of TOTE units become connected in parallel (Pribram, 1971) so that each test biases the others in the network. Parallel information and image processing results in willed, voluntary, intentional behavior. Biofeedback is an example where, by providing an appropriate external monitor (the biofeedback), the internal autonomic, unconscious feedback loop is brought under bias control (i.e., voluntary control, much as a thermostat's set point can be controlled by changing its

bias, the little wheel on top). Whenever attention can be deployed in such open loops, intentionality and thus selfhood result. Evidence is presented (in Pribram, 1971) that demonstrates some of the neural mechanisms involved (cerebellar systems, hippocampal system). Whether the model is adequate to the profound difficulties that selfhood poses with respect to the uniqueness of humanity remains to be seen.

CONCLUSION

It is also this looking upward that unites us all in the ecological approach to mental function. Whether it is Dirac discussing matter swimming in a sea of densely packed antimatter (monads?) or Gibson, Pattee, Shaw, Weimer, Pribram, and Hayek in this conference, we all share this way of investigating our subject matter. When listening to Hayek, for instance, I was struck with the importance he placed on the distribution of information in the marketplace that allows each individual to act with respect to the whole. Are we economic monads perhaps? Is the supply of money the hologram of the marketplace, and is the neural hologram the marketplace of the brain? Some quantitative studies using Gabor's mathematics (Fourier transforms and the like) might uncover some of the reasons why *Small Is Beautiful* (Schumacher, 1973).

I have said enough to indicate the reach of this new view of scientific endeavor. I have stopped short of theology, but Weimer is bolder and quotes Weyl with respect to the deification of symmetry. But certainly within the more ordinary compass of scientific endeavor, a paradigm shift in Kuhn's (1962) sense is heralded by what we have all been inspired to accomplish by James Gibson's provocative approach to understanding phenomenal reality. The new frontiers created by looking upward are not all in outer space. At every level of inquiry, exciting vistas, heretofore ignored and invisible, seem to stretch before us, tantalizingly awaiting further exploration.

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