

A PROGRESS REPORT ON THE SCIENTIFIC UNDERSTANDING OF PARANORMAL PHENOMENA

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Introduction

In 1968, I attended a conference on methodology in psi research organized by Roberto Cavanna. The participants contributing to the neurophysiological parts of the program included Monte Buchsbaum, Joe Kamiya, Don Lindsley, Alan Rechtschaffen, Julie Silverman and Grey Walter. We were seriously offering our services to the others in the gathering—the believers in paranormal phenomena. In our discussions among ourselves, we reassured each other of our scientific skepticism and integrity. Still, we had to face the fact that we had accepted the invitation to participate and that we were presenting our own work in good faith. At the time, I summed up our position—in an attempt to resolve the tension between skepticism and faith—as a scientist's willingness to suspend disbelief until the facts are in. My own current position remains essentially unchanged since the 1968 meetings. At the same time, however, I am much more comfortable with the putative phenomena concerning which my disbelief needs to be suspended. This increased comfort does not arise so much from any accretion in facts as it does from important new insights which have enhanced understanding. These insights, I believe, augur a paradigm shift in all of science—to use Kuhn's classic phrase—a shift which may make it feasible for scientific understanding to encompass paranormal experiences.

Note that I am addressing the problem of a scientific base for understanding. Such a base would allow the phenomena under consideration to be approached from a variety of directions with a variety of methods. Only in this way can experiences be truly shared. In saying this I do not mean to disparage sharing by experiencing *per se*, nor the attempts at proving (i.e. testing) reliability. Nonetheless, we must admit, that even one experience by one person, if it is believed,

must be explained if scientific understanding is to be achieved. And belief will rest on prior understanding (shared values, etc.) and very little else.

The Hologram

In 1968, I ventured the suggestion that the holographic hypothesis of brain function held a hidden promise for understanding unmatched by other developments in the neurosciences. This suggestion was based on the fact that holography encodes waveforms and could therefore explain how organisms might tune-in on and "resonate-with" energy configurations other than those that give rise to our ordinary perceptions. The hidden promise is no longer hidden. Much has happened in support of the holographic hypothesis and a larger understanding of its import has been achieved.

Holograms were the mathematical invention of Dennis Gabor (1948) in the late 1940's. Gabor was attempting to improve the resolution of electron microscopy by recording, instead of images, the patterns of light diffracted by (filtered through or reflected from) the tissue to be examined. A decade or so later, Gabor's mathematics became realized in hardware when coherent light from lasers could be used to form optical holograms (Leith and Upatnicks, 1965). Also, the advent of inexpensive general purpose computers made it possible to simulate the holographic process in order to formulate a precise model of how it might be realized in neural tissue (Pribram, Nuwer and Baron, 1974). These hardware realizations made it obvious that object \rightarrow wave storage \rightarrow image construction is a linear process and that, according to Gabor's equations, the identical mathematical transfer function transformed object into wave storage and wave storage into image. The storage of wave patterns (the hologram) is thus reciprocally related to the imaging of objects. The wave functions are transforms of objects and their images.

Gabor named the wave pattern store a *hologram* because one of its most interesting characteristics is that information from the object becomes distributed over the surface of the photographic film, thus allowing the object to be reconstructed from each part. Each point of light diffracted from the object becomes blurred, spread over the entire surface of the film, as is each neighboring point of light. The equations that describe this blurring are called spread functions. The spread is not haphazard, however, as the blur would lead one to believe. Rather, ripples of waves move out from the point of light, much as ripples of waves are formed when a pebble strikes the smooth surface of a pond of water. Throw into the pond a handful of pebbles

or sand and the ripples produced by each pebble or grain will crisscross with those produced by the other pebbles or grains setting up patterns of interfering wave fronts. The smooth mirror-like surface has become blurred but the blur has hidden within it an unsuspectedly orderly pattern. If the pond could suddenly be frozen at this moment, its surface would be a hologram. The photographic hologram is such a frozen record of interference patterns.

It seemed immediately plausible that the distributed memory store of the brain might resemble this holographic record. I developed a precisely formulated theory based on known neuroanatomy and known neurophysiology that could account for the brain's distributed memory store in holographic terms. In the dozen or so years since, many laboratories, including my own, have provided evidence in support of parts of this theory. Other data have sharpened the theory and made it even more precise and fitting to the known facts.

Essentially, the theory reads that the brain at one stage of processing performs its analyses in the frequency domain. This is accomplished at the junctions *between* neurons, not within neurons. Thus graded local waxings and wanings of neural potentials (waves) rather than nerve impulses are responsible. Nerve impulses are generated within neurons and are used to propagate the signals that constitute information over long distances via long nerve fibers. Graded local potential changes, waves, are constituted at the ends of these nerve fibers where they adjoin shorter branches that form a feltwork of interconnections among neurons. Some neurons, now called local circuit neurons, have no long fibers and display no nerve impulses. They function in the graded wave mode primarily and are especially responsible for horizontal connectivities in sheets of neural tissue, connectivities in which holographic-like interference patterns can become constructed.

Aside from these anatomical and physiological specifications, a solid body of evidence has accumulated that the auditory, somatosensory, motor and visual systems of the brain do in fact process, at one or several stages, input from the senses in the frequency domain. This distributed input must then in some form, perhaps as changes in the conformation of proteins at membrane surfaces, become encoded into distributed memory traces. The protein molecules would serve the neural hologram in the same way as oxidized silver grains in the photographic hologram.

The explanation of the fact that specific memory traces are resistant to brain damage (remembering demands only that a small part of the distributed store remain intact, in the same way that images can be

reconstructed from small parts of a photographic hologram) has been only one of the contributions of holographic theory. Characteristics of the experience of imaging have been explained in an equally powerful manner. The projection of images away from their sources of origin has been demonstrated to result from processing phase relations (just as in the stereophonic audio systems). Simulations of image processing by computer have found no technique other than the holographic to provide the rich texture of scenes such as those that compose our experiences. And the complicated computations that go into three dimensional x-ray imaging by computerized tomography have relied heavily on the fact that such computations (mostly correlations) are performed readily in the frequency (holographic) domain.

The Evidence

The evidence regarding a brain holographic process developed from studies on the brain mechanisms involved in memory and perception. Only a little over 25 years ago Lashley uttered his famous remark that, on the basis of his lifetime of research on brain function, it was clear that "learning just could not take place." Lashley's despair was produced by his repeated findings of equivalence of function of parts of brain systems. Not only was he unable to excise any specific memory, but was unable to account for the facts of sensory and motor equivalence: "These three lines of evidence indicate that certain coordinated activities, known to be dependent upon definite cortical areas, can be carried out by any part (within undefined limits) of the whole area. Such a condition might arise from the presence of many duplicate reflex pathways through the areas and such an explanation will perhaps account for all of the reported cases of survival of functions after partial destruction of their special areas, but it is inadequate for the facts of sensory and motor equivalence. These facts establish the principle that once an associated reaction has been established (e.g., a positive reaction to a visual pattern), the same reaction will be elicited by the excitation of sensory cells which were never stimulated in that way during the course of training. Similarly, motor acts (e.g., opening a latch box), once acquired, may be executed immediately with motor organs which were not associated with the act during training." (Lashley, 1960, p. 240)

What sort of brain mechanism could be imagined which would account for the principle that "once an associated reaction has been established, the same reaction will be elicited by the excitation of sensory cells which were never stimulated in that way during the course of training"? And what mechanism could be devised to deal with the

fact that "motor acts, once acquired, may be executed immediately with motor organs which were not associated with the act during training"? What sort of mechanism of association could be taking place during learning so that its residual would, as it were, act at a distance?

The difficulties of conceptualization may be summarized as follows: during acquisition associative processes must be operative. However, these associative processes must result in a distributed store. On the basis of Lashley's analysis, input must become dismembered before it becomes re-membered. Association and distribution are in some fundamental fashion inexorably linked.

During the mid nineteen-sixties it became apparent that image processing through holography could provide the model for a mechanism with such "distribution by association" properties. As in the case of every novel approach, there were, of course, earlier formulations, including those of Lashley, that attempted to explain these aspects of brain function in terms that today we would call holographic.

Historically, the ideas can be traced to problems posed during neurogenesis when the activity of relatively remote circuits of the developing nervous system must become integrated to account for such simple behaviors as swimming. Among others, the principle of chemical "resonances" that "tune" these circuits has had a long and influential life (see, e.g. Loeb, 1907; Weiss, 1939). More specifically, however, Goldscheider (1906) and Horton (1925) proposed that the establishment of tuned resonances in the form of interference patterns in the adult brain could account for a variety of perceptual phenomena. More recently, Lashley (1942) spelled out a mechanism of neural interference patterns to explain stimulus equivalence and Beurle (1956) developed a mathematically rigorous formulation of the origin of such patterns of plane wave interferences in neural tissue. But it was not until the advent of holography, with its powerful damage-resistant image storage and reconstructive capabilities, that the promise of an interference pattern mechanism of brain function became fully appreciated. As the properties of physical holograms became known (see Stroke, 1966; Goodman, 1968; Collier, Burckhardt and Lin, 1971), a number of scientists saw the relevance of holography to the problems of brain function, memory, and perception (e.g., van Heerden, 1963; Julesz and Pennington, 1965; Kabrisky, 1966; Pribram, 1966; Westlake, 1968; Baron, 1970; Cavanagh, 1972).

The advent of these explanations came with the development of physical holography (e.g. Stroke, 1966) from the mathematical principles enunciated by Gabor (1948). Equally important, however,

was the failure of computer science to simulate perception and learning in any adequate fashion. The problem lies in the fact that computer based "perceptions" (e.g. Rosenblatt, 1962) were constructed on the basis of an assumed random connectivity in neural networks, when the actual anatomical situation is essentially otherwise. In the visual system, for instance, the retina and cortex are connected by a system of fibers that run to a great extent in parallel. Only two modifications of this parallelity occur: 1) The optic tracts and radiations that carry signals between the retina and cortex constitute a sheaf within which the retinal events converge to some extent onto the lateral geniculate nucleus of the thalamus from where they diverge to the cortex. The final effect of this parallel network is that each fiber in the system connects ten retinal outputs to about 5,000 cortical receiving cells. 2) In the process of termination of the fibers at various locations in the pathway, an effective overlap develops (to about 5° of visual angle) between neighboring branches of the conducting fibers.

Equally striking, and perhaps more important than these exceptions, however, is the interpolation at every cell station of a sheet of horizontally connected neurons in a plane perpendicular to the parallel fiber system. These horizontal cells are characterized by short or absent axons, but spreading dendrites. It has been shown in the retina (Werblin and Dowling, 1969) and to some extent also in the cortex (Creutzfeldt, 1961), that such spreading dendritic networks may not generate nerve impulses; in fact, they usually may not even polarize. Their activity is characterized by hyperpolarization that tends to organize the functions of the system by inhibitory rather than excitatory processes. In the retina, for instance, no nerve impulses are generated prior to the amacrine and the ganglion cells from which the optic nerve fibers originate. Thus, practically all of the complexity manifest in the optic nerve is a reflection of the organizing properties of depolarizing and hyperpolarizing events, not of interactions among nerve impulses.

Two mechanisms are, therefore, available to account for the distribution of signals within the neural system. One relies on the convergence and divergence of nerve impulses onto and from a neuronal pool. The other relies on the presence of lateral (mostly inhibitory) interactions taking place in sheets of horizontal dendritic networks situated at every cell station perpendicular to the essentially parallel system of input fibers. Let us explore the possible role of both of these mechanisms in explaining the results of the lesion studies.

Evidence is supplied by experiments in which conditions of anesthesia are used that suppress the functions of small nerve fibers,

thus leaving intact and clearly discernible the connectivity by way of major nerve impulse pathways. These experiments have shown that localized retinal stimulation evokes a receptive field at the cortex over an area no greater than a few degrees in diameter (e.g. Talbot and Marshall, 1941). Yet, the data that must be explained indicate that some 80 percent or more of the visual cortex, including the foveal region, can be extirpated without marked impairment of the recognition of a previously learned visual pattern. Thus, whatever the mechanisms, distribution of input cannot be due to the major pathways, but must involve the fine fibered connectivity in the visual system, either via the divergence of nerve impulses and/or via the interactions taking place in the horizontal cell dendritic networks.

Both are probably to some extent responsible. It must be remembered that nerve impulses occurring in the fine fibers tend to decrement in amplitude and speed of conduction, thus becoming slow graded potentials. Further, these graded slow potentials or minispikes usually occur in the same anatomical location as the horizontal dendritic inhibitory hyperpolarizations and thus interact with them. In fact, the resulting micro-organization of junctional neural activity (synaptic and ephaptic) could be regarded as a simple summation of graded excitatory (depolarizing) and inhibitory (hyperpolarizing) slow potential processes.

These structural arrangements of slow potentials are especially evident in sheets of neural tissue such as in the retina and cortex. The cerebral cortex, for instance, may be thought of as consisting of columnar units that can be considered more or less independent basic computational elements, each of which is capable of performing a similar computation (Mountcastle, 1957; Hubel and Wiesel, 1968). Inputs to the basic computational elements are processed in a direction essentially perpendicular to the sheet of the cortex and, therefore, cortical processing occurs in stages, each stage transforming the activation pattern of the cells in one of the cortical layers to the cells of another cortical layer. Analyses by Kabrisky (1966) and by Werner (1970) show that processing by one basic computational element remains essentially within that element and, therefore, the cortex can be considered to consist of a large number of essentially similar parallel processing elements. Furthermore, the processing done by any one of the basic computational elements is itself a parallel process (see, for example, Spinelli, 1970a), each layer transforming the pattern of activity that arrived from the previous layer by the process of temporal and spatial summation, the summation of slow hyper and depolarizations in the dendritic microstructure of the cortex. Analyses by Ratliff

(1965) and Rodieck (1965) have shown that processing (at least at the sensory level) that occurs through successive stages in such a layered neural network can be described by linear equations. Each computational element is thus capable of transforming its inputs through a succession of stages, and each stage produces a linear transformation of the pattern of activity at the previous stage.

Let us trace in detail the evidence regarding these stages in the visual system. Quantitative descriptions of the interactions that occur in the retina are inferred from the output of ganglion cells, from which receptive field configurations are recorded by making extracellular microelectrode recordings from the optic nerve. The retinal interactions *per se* take place initially by virtue of local graded slow wave potentials—hyperpolarizations and depolarizations that linearly sum within the networks of receptors, bipolar and horizontal cells, from which nerve impulses are never recorded. The receptive fields generated by these graded potential changes display a more or less circular center surrounded by a ring of activity of sign opposite that of the center. This configuration has been interpreted to mean that the activity of a receptive neuron generates inhibition in neighboring neurons through lateral connectivities (e.g. Hartline, Wagner and Ratliff, 1956; Békésy, 1967; Kuffler, 1953) perpendicular to the input channels. In view of the fact that no nerve impulses can be recorded from the cells (e.g. horizontal) that mediate the lateral inhibition, the inference can be made that the interactions among graded potentials, wave forms, are responsible (Pribram, 1971; Pribram, Nuwer and Baron, 1974). Such wave forms need not be thought of as existing in an unstructured homogeneous medium. The dendritic arborizations in which the graded potential changes occur can act as structural wave guides. However, as Beurle (1956) has shown, such a structural medium can still give rise to a geometry of plane waves provided the structure is reasonably symmetrical. The mathematical descriptions of receptive field configurations bear out Beurle's model. Such descriptions have been given by Ratliff (1965) and Rodieck (1965). Mathematically, they involve a convolution of luminance change of the retinal input with the inferred inhibitory characteristics of the network to compose the observed ganglion cell receptive field properties.

The gist of these experimental analyses is that the retinal mosaic becomes decomposed into an opponent process by depolarizing and hyperpolarizing slow potentials and transforms into more or less concentric receptive fields in which center and surround are of opposite sign. Sets of convolutional integrals fully describe this transformation.

The next cell station in the visual pathway is the lateral geniculate nucleus of the thalamus. The receptive field characteristics of the output from neurons of this nucleus are in some respects similar to the more or less concentric organization obtained at the ganglion cell level. Now, however, the concentric organization is more symmetrical, the surround usually has more clear-cut boundaries and is somewhat more extensive (e.g., Spinelli and Pribram, 1967). Furthermore, a second penumbra of the same sign as the center can be shown to be present, though its intensity (number of nerve impulses generated) is not nearly so great as that of the center. Occasionally, a third penumbra, again of opposite sign, can be made out beyond the second (Hammond, 1972).

Again, a transformation has occurred between the output of the retina and the output of the lateral geniculate nucleus. This transformation apparatus appears to act as a rectification process. Each geniculate cell thus acts as a peephole "viewing" a part of the retinal image mosaic. This is due to the fact that each geniculate cell has converging upon it some 10,000 ganglion cell fibers. This receptive field peephole of each geniculate cell is made of concentric rings of opposing sign, whose amplitudes fall off sharply with distance from the center of the field. In these ways the transformation accomplished is like very near-field optics that describes a Fresnel Hologram.

Pollen, Lee and Taylor (1971), though supportive of the suggestion that the visual mechanism as a whole may function in a holographic-like manner, emphasize that the geniculate output is essentially topographic and punctate, is not frequency specific and does not show translational invariance—i.e. every illuminated point within the receptive field does not produce the same effect. Further, the opponent properties noted at the retinal level of organization are maintained and enhanced at the cost of overall translational invariance. Yet a step toward a discrete transform domain has been taken since the output of an individual element of the retinal mosaic—a rod or cone receptor—is the origin of the signal transformed at the lateral geniculate level.

When the output of lateral geniculate cells reaches the cerebral cortex, further transformations take place. One set of cortical cells, christened "simple" by their discoverers (Hubel and Wiesel, 1968), has been suggested to be characterized by a receptive field organization composed by a literally linelike arrangement of the outputs of lateral geniculate cells. This proposal is supported by the fact that the simple-cell receptive field is accompanied by side bands of opposite sign and occasionally by a second side band of the same sign as the central field. Hubel and Wiesel proposed that these simple cells thus

serve as line detectors in the first stage of a hierarchical arrangement of pattern detectors. Pollen et al. (1971) have countered this proposal on the basis that the output from simple cells varies with contrast luminance as well as orientation and that the receptive field is too narrow to show translational invariance. They argue, therefore, that an ensemble of simple cells would be needed to detect orientation. They suggest that such an ensemble would act much as the strip integrator used by astronomers (Bracewell, 1965) to cull data from a wide area with instruments of limited topographic capacity (as is found to be the case in lateral geniculate cells).

Another class of cortical cells has generated great interest. These cells were christened "complex" by their discoverers, Hubel and Wiesel, and thought by them (as well as by Pollen) to be the next step in the images processing hierarchy. Some doubt has been raised (Hoffman and Stone, 1971), because of their relatively short latency of response, as to whether all complex cells receive their input from simple cells. Whether their input comes directly from the geniculate or by way of simple cell processing, however, the output from complex cells of the visual cortex displays transformations of the retinal input, characteristics of the holographic domain.

A series of elegant experiments by Fergus Campbell and his group (1974) have suggested that these complex cortical cells are spatial-frequency sensitive elements. Initially, Campbell showed that the response of the potential evoked in man and cat by repeated flashed exposure to a variety of gratings of certain spacing (spatial frequency), adapted not only to that fundamental frequency, but also to any component harmonics present. He concluded, therefore, that the visual system must be encoding spatial frequency (perhaps in Fourier terms) rather than the intensity values of the grating. He further showed that when a square wave grating was used, adaptation was limited to the fundamental and its third harmonic as would be predicted by Fourier theory. Finally, he found neural units in the cat's cortex that behaved as did the gross potential recordings.

Pollen (1973) has evidence that suggests that these spatial-frequency sensitive units are Hubel and Wiesel's complex cells, although both his work and that of Maffei and Fiorentini (1973) have found that simple cells also have the properties of spatial frequency fibers, in that they are sensitive to a selective band of spatial frequencies. In addition, the latter investigators have found that the simple cells can transmit contrast and spatial phase information in terms of two different parameters of their response: contrast is coded in terms of impulses per second and spatial phase in terms of firing pattern.

The receptive field of complex cells is characterized by the broad extent (when compared with simple cells) over which a line of relatively indeterminate length, but a certain orientation, will elicit a response. Pollen demonstrated that the output of complex cells was not invariant to orientation alone—number of lines and their spacing appeared also to influence response. He concluded, therefore, as had Fergus Campbell, that these cells were spatial frequency sensitive and that the spatial frequency domain was fully achieved at this level of visual processing. Additional corroborating evidence has recently been presented from the Pavlov Institute of Physiology in Leningrad by Glezer, Ivanoff and Tscherbach (1973), who relate their findings on complex receptive fields as Fourier analyzers to the dendritic microstructure of the visual cortex much as we have done here.

Even more recently, a series of studies from the Cambridge laboratories, from MIT, Berkeley and our own at Stanford University, have substantiated the earlier reports. Pribram, Lassonde and Ptilo (submitted to *J. Neurophysiology*) have confirmed that both simple and complex cells are selective to restricted band widths of spatial frequencies, but that simple cells encode spatial phase while complex cells do not. Thus, simple cells may be involved in the perception of spatial location while complex cells are more truly “holographic” in that they are responsible for translational invariance. Schiller, Finlay and Volman (1976a, b, c, d) have performed a comprehensive coverage of receptive field properties, including spatial frequency selectivity. Movshen, Thompson and Tolhurst (1978a, b, c) in another set of experiments showed that receptive fields could be thought of as spatial filters (much as van Heerden, 1963, originally proposed) whose Fourier transform precisely mapped the cell’s response characteristics. De Valois, Albrecht and Thorell (1978) have taken this work even a step further by showing that, whereas these cells are tuned to from $\frac{1}{2}$ to $1\frac{1}{2}$ octaves of band width of the spatial frequency spectrum, they are not tuned at all to changes in bar width. Finally, De Valois has tested whether the cells are selective of edges making up patterns or their Fourier transforms. The main components of the transforms of checkerboards and plaids lie at different orientations from those of the edges making up the patterns. In every case the orientation selectivity of the cells was shifted when gratings were changed to checkerboards or plaids and the shift was to the exact amount in degrees and minutes of arc predicted by the Fourier transform.

The results of these experiments go a long way toward validating the holographic hypothesis of brain function. However, as I have noted previously (Pribram, Nuwer and Baron, 1974), a major problem

remains even after these data are incorporated in the construction of a precise model. Each receptive field, even though it encodes in the frequency domain, does so over a relatively restricted portion of the total visual field. Robson (1975) has thus suggested that only a "patch" of the field becomes represented. However, this major problem has now been resolved and the solution has brought unexpected dividends. Ross (see review by Leith, 1976) has constructed holograms on the principles proposed by Bracewell (1965) and espoused by Pollen (see Pollen and Taylor, 1974). Such multiplex or strip integral holograms are now commercially available (Multiplex Co., San Francisco, California). Not only do they display all the properties of ordinary holograms, but can be used to encode movement as well. Thus, by combining frequency encoding with a spatial "patch" or "slit" representation, a lifelike three-dimensional moving image can be constructed.

Although detailed specification has been given for the visual system only, the foregoing analysis is in large part also relevant to the auditory system, the tactile system and the motor system (see Pribram, 1971, for review). The recently accumulated facts concerning the visual system are the most striking because it was not suspected that spatial pattern perception would be found to be based on a stage that involves frequency analysis. The finding of the ubiquity of frequency analysis by brain tissue has made accessible explanations of hitherto inexplicable observations, such as the distributed nature of the memory trace and the projection of images away from the surface in which their representation has become encoded. The model has had considerable explanatory power.

The Nature of Reality

I want now to address some consequences to psychology (and perhaps to philosophy) of the holographic process of brain function. The theory, as we have seen, 1) stems from the metaphors of machine and optical information processing systems; 2) has developed by analogy to those systems, spelling out some similarities and some differences; until 3) a testable holonomic model of brain function could be proposed. One way of understanding the model better is to compare it to another and to observe its relative explanatory power.

An apparent alternative to the holographic process is presented by James Gibson's comprehensive "ecological" model of perception (1966). Gibson's model proposes that the "information" perceived is inherent in the physical universe and that the perceiver is sensitive to

whatever information remains invariant across transformations produced by changes in the environment, by organism-environment displacements and by the organism's processing apparatus. The key concept in the ecological theory is "direct perception"—the environment is directly apprehended by the perceiver.

By contrast, the holographic process is constructional. Images are constructed when input from the inferior temporal cortex (or its analogue in other perceptual systems—see Pribram, 1974a) activates and organizes the distributed holographic store. Images are produced and are therefore as much a *product of* the "information residing in" the organism, as they are of "information" contained in the environment. Philosophically speaking, the holonomic model is Kantian and Piagetian, the ecological model partakes of a naive realism.

Clinical neurological experience wholly supports the holographic view. Patients are seen who complain of macropsia and micropsia and other bizarre distortions of visual space. For instance, I once had a patient who, after a blow on the head, experienced episodes of vertigo during which the visual world went spinning. His major complaint was that every so often when his perceptions again stabilized, they left him with the world upside down until the next vertigo which might right things once again. He had developed a sense of humor about these experiences, which were becoming less frequent and of shorter duration: his major annoyance he stated to be the fact that girls' skirts stayed up despite the upside-down position!

Further "clinical" evidence in support of the holographic process comes from the experimental laboratory. Resections of the primate inferior temporal cortex markedly impair size constancy—the transformations across various distances over which environmental information must remain invariant in order to be "directly" perceived as of the same size.

Yet Gibson (1966; 1968) and others who share his views (e.g., Johansson, 1973; and more recently Hebb, in press), make a good case that, in normal adult humans, perception is direct. A series of ingenious experiments has shown that by appropriate manipulations of "information," illusions indistinguishable from the "real" can be created on a screen. The demonstrations are convincing and make it implausible to maintain a solipsistic or purely idealistic position with respect to the physical universe—that nothing but a buzzing blooming confusion characterizes external reality. With respect to the experiments he has devised, Gibson is correct.

Furthermore, if perception is direct, a dilemma for the holographic process would be resolved. When an optical hologram produces an

image, a human observer is there to see it. When a neural hologram constructs an image, who is the observer? Where is the "little man" who views the "little man"? Direct perception needs no little men inside the head. Gibson, in fact, (1966) deplores the term image because it calls up the indirectness of the representational process. However, if what we "directly perceive" is a constructed *image* and not the true organization of the external world—and we mistake this perception as veridical—perception would be both direct and constructional.

The question to be answered therefore is by what mechanism can perception be both direct and constructional? A clue to the resolution of this dilemma comes from the Gibson (and Johansson) experiments themselves. Their displays produce the *illusion* of reality. When we know the entire experiment we can label the percept as an illusion, even though we directly experience it. In a similar fashion, the sound coming from the speakers of a stereophonic system is experienced directly. When we manipulate the dials of the system (changing the phase of the interacting, interfering sound waves) so that all of the sound comes from one of the speakers, we say the speaker is the source of the perception. When we manipulate the dials so that the sound emanates from somewhere (e.g. the fireplace) between the speakers, we say that an illusion has been produced—the sound has been projected to the space between the speakers. Perception continues to be direct, but considerable computation is involved in determining the conditions over which the "information" contained in the sound remains invariant. We do not naively assume that the fireplace generates the sound. Despite the directness of the perception, it can be superficially misleading as to the actual characteristics of the physical universe.

The issues appear to be these. Gibson abhors the concept "image." As already noted, he emphasizes the "information" which the environment "affords" the organism. As an ecological theorist, however, Gibson recognizes the importance of the organism in determining what is afforded. He details especially the role of movement and the temporal organization of the organism-environment relationship that results. Still, that organization does *not* consist of the construction of percepts from their elements; rather, the process is one of responding to the invariances in that relationship. Thus, perceptual learning involves progressive differentiation of such invariances, not the association of sensory elements.

The problem for me has been that I agree with all of the positive contributions to conceptualization which Gibson has made, yet find myself in disagreement with his negative views (such as on "images")

and his ultimate philosophical position. If, indeed, the organism plays such a major role in the theory of ecological perception, does not this entail a constructional position? Gibson's answer is no, but perhaps this is due to the fact that he (in company with so many psychologists) is basically uninterested in what goes on inside the organism.

What, then, does go on in the perceptual systems that is relevant to this argument? I believe that to answer this question we need to analyze what is ordinarily meant by "image." Different disciplines have very different definitions of this term.

The situation is similar to that which obtained in neurology for almost a century with regard to the representation we call "motor." In that instance, the issue was stated in terms of whether the representation in the motor cortex was punctate or whether in fact movements were represented. A great number of experiments were done. Many of them, using anatomical and discrete electrical stimulation techniques, showed an exquisitely detailed anatomical mapping between cortical points and muscles and even parts of muscles (Chang, Ruch and Ward, 1947). The well known homunculus issued from such studies on man (Penfield and Boldrey, 1937).

But other, more physiologically oriented experiments, provided different results. In these, it was shown that the same electrical stimulation at the same cortical locus would produce *different* movements depending on such other factors as position of the limb, the density of stimulation and the state of the organism (e.g. his respiratory rate). For the most part, one could conceptualize the results as showing that the cortical representation consisted of movements centered on one or another joint (e.g. Phillips, 1965). The controversy was thus engaged — proponents of punctate muscle representation vis à vis the proponents of the representation of movement.

I decided to repeat some of the classical experiments in order to see for myself which view to espouse (reviewed in Pribram, 1971, Chapters 12 and 13). Among the experiments performed was one in which the motor cortex was removed (unilaterally and bilaterally) in monkeys who had been trained to open a rather complex latch box to obtain a peanut reward (Pribram, Kruger, Robinson and Berman, 1955-56). My results in this experiment were, as in all others, the replication of the findings of my predecessors. The latch box was opened, but with considerable clumsiness, thus prolonging the time taken some two- to three-fold.

But the interesting part of the study consisted in taking cinematographic pictures of the monkeys' hands while performing the latch-box task and in their daily movements about the cage. Showing these films

in slow motion we were able to establish to our satisfaction that no movement or even sequence of movements was specifically impaired by the motor cortex resections! The deficit appeared to be *task* specific, not muscle or movement specific.

My conclusion was, therefore, that depending on the *level of analysis*, one could speak of the motor representation in the cortex in three ways. Anatomically, the representation was punctate and of *muscles*. Physiologically, the representation consisted of mapping the muscle representation into *movements*, most likely around joints as anchor points. But behavioral analysis showed that these views of the representation were incomplete. No muscles were paralyzed, no movements precluded by total resection of the representation. *Action*, defined as the environmental consequence of movements, was what suffered when motor cortex was removed.

The realization that acts, not just movements of muscles, were represented in the motor systems of the brain accounted for the persistent puzzle of motor equivalences. We all know that we can, though perhaps clumsily, write with our left hands, our teeth or, if necessary, our toes. These muscle systems may never have been exercised to perform such tasks, yet, immediately and without practice, can accomplish at least the rudiment required. In a similar fashion, birds will build nests from a variety of materials and the resulting structure is always a habitable facsimile of a nest.

The problem immediately arose, of course, as to the precise nature of a representation of an act. Obviously there is no "image" of an action to be found in the brain, if by "image" one means specific words or the recognizable configuration of nests. Yet some sort of representation appears to be engaged that allows the generation of words and nests—an image of what is to be achieved, as it were.

The precise composition of images-of-achievement remained a puzzle for many years. The resolution of the problem came from experiments by Bernstein (1967), who made cinematographic records of people hammering nails and performing similar more or less repetitive acts. The films were taken against black backgrounds with the subjects dressed in black leotards. Only joints were made visible by placing white dots over them. The resulting record was a continuous wave form. Bernstein performed a Fourier analysis on these wave forms and was invariably able to predict within a few centimeters the amplitude of the next in the series of movements.

The suggestion from Bernstein's analysis is that a Fourier analysis of the invariant components of motor patterns (and their change over time) is computable and that an image-of-achievement may consist of

such computation. Electrophysiological data from unit recordings obtained from the motor cortex have provided preliminary evidence that, in fact, such computations are performed (Evarts, 1967, 1968).

By "motor image," therefore, we mean a punctate muscle-brain connectivity that is mapped into movements over joints in order to process environmental invariants generated by or resulting from those movements. This three-level definition of the motor representation can be helpful in resolving the problems that have become associated with the term "image" in perceptual systems.

In vision, audition and somesthesia (and perhaps, to some extent, in the chemical senses as well) there is a punctate connectivity between receptor surface and cortical representation. This anatomical relationship serves as an *array* over which sensory signals are relayed. At a physiological level of analysis, however, a mapping of the punctate elements of the array into functions occurs. This is accomplished in part by convergences and divergences of pathways, but even more powerfully by networks of lateral interconnectivities, most of which operate by way of slow graded dendritic potentials rather than by nerve impulses propagated in long axons. Thus, in the retina, for instance, no nerve impulses can be recorded from receptors, bipolar or horizontal cells. It is only in the ganglion cell layer, the last stage of retinal processing, that nerve impulses are generated to be conducted in the optic nerve to the brain (reviewed in Pribram, 1971, Chapters 1, 6 and 8). These lateral networks of neurons operating by means of slow graded potentials thus map the punctate receptor-brain connectivities into functional *ambiences*.

By analogy to the motor system, this characterization of the perceptual process is incomplete. Behavioral analysis discerns perceptual constancies, just as this level had to account for motor equivalences. In short, *invariances* are processed over time and these invariances constitute the behaviorally derived aspects of the representation (see e.g. Pribram, 1974b). Ordinarily, an organism's representational processes are called *images* and there is no good reason not to use this term. But it must be clearly kept in mind that the perceptual image, just as the motor image, is more akin to a computation than to a photograph.

I have already presented the evidence that for the visual system at least, this computation (just as in the motor system) is most readily accomplished in the Fourier or some similar domain. The evidence that pattern perception depends on the processing of spatial frequencies has been reviewed. It is, after all, this evidence more than any other that has suggested the holonomic hypothesis of perception.

The perceptual image, so defined, is therefore a representation, a mechanism based on the precise anatomical punctate receptor-cortical connectivity that composes an *array*. This array is operated upon by lateral interconnections that provide the *ambiences* which process the *invariances* in the organism's input. The cortical representations of the percepts go, therefore, beyond the anatomical representations of the receptor surfaces, just as the cortical representation of actions goes beyond the mere anatomical representations of muscles.

It is, of course, a well known tenet of Gestalt psychology that the percept is not the equivalent of the retinal (or other receptor) image. This tenet is based on the facts of constancy (e.g. size) and the observations of illusions. Neurophysiologists, however, have only recently begun to seriously investigate this problem. Thus Horn (Horn, Stechler and Hill, 1972) showed that certain cells in the brainstem (superior colliculus) maintained their firing pattern to an environmental stimulus despite changes in body orientation; and in my laboratory Spinelli (1970b) and also Bridgeman (1972), using somewhat different techniques, demonstrated constancy in the firing pattern of cortical neurons over a range of body and environmental manipulations. Further, neurobehavioral studies have shown that size constancy is impaired when the perivisual and inferior temporal cortex is removed (Humphrey and Weiskrantz, 1969; Ungerleider, 1975).

The fact that the cortex becomes tuned to environmental invariances, rather than just to the retinal image, is borne out dramatically by a hitherto unexplained discrepancy in the results of two experiments. In both experiments, a successful attempt was made to modify the orientation selectivity of the cortical neurons of cats by raising them from birth in environments restricted to either horizontal or vertical stripes. In one experiment (Blakemore, 1974) the kittens were raised in a large cylinder appropriately striped. A collar prevented the animals from seeing parts of their bodies—so they were exposed to only the stripes. However, and this turns out to be critical, the kittens could observe the stripes from a variety of head and eye positions. By contrast, in the other experiment, which was performed in my laboratory (Hirsch and Spinelli, 1970), head and eye turning was prevented from influencing the experiment by tightly fitting goggles onto which the stripes were painted. In both experiments cortical neurons were found to be predominantly tuned to the horizontal or vertical depending on the kitten's environment, although the tuning in Blakemore's experiments appeared to be somewhat more effective. The discrepancy arose when behavioral testing was instituted. Blakemore's kittens were consistently and completely deficient in their

ability to follow a bar moving perpendicular to the orientation of the horizontally or vertically striped environment in which they had been raised. In our experiment, Hirsch, despite years of effort using a great number of quantitative tests, could never demonstrate *any* change in visual behavior! The tuning of the cortical cells to the environmental situation which remained invariant across transformations of head and eye turning was behaviorally effective; the tuning of cortical cells to consistent retinal stimulation had no behavioral consequences.

These results are consonant with others obtained in other sensory modes and also help to provide some understanding of how brain processing achieves our perception of an objective world, separated from the receptor surfaces which interface the organism with his environment.

Von Bekesy (1967) has performed a large series of experiments on both auditory and somatosensory perceptions to clarify the conditions that produce projection and other perceptual effects. For example, he has shown that a series of vibrators placed on the forearm will produce a point perception when the phases of the vibrations are appropriately adjusted. Once again, in our laboratory we found that the cortical response to the type of somatosensory stimulation used by Bekesy was consonant with the perception, not with the pattern of physical stimulation of the receptor surface (Dewson, 1964; Lynch, 1971). Further, Bekesy showed that when such vibrators are applied to both forearms, and the subject wears them for awhile, the point perception suddenly leaps into the space between the arms. Other evidence for projection comes from the clinic. An amputated leg can still be perceived as a phantom for years after it has been severed and pickled in a pathologist's jar. A more ordinary experience comes daily to artisans and surgeons who "feel" the environment at the ends of their tools and instruments.

These observations suggest that direct perception is a special case of a more universal experience. When what we perceive is validated through other senses or other knowledge (accumulated over time in a variety of ways, e.g. through linguistic communication—see Gregory, 1966), we claim that perception to be veridical. When validation is lacking or incomplete, we tend to call the perception an illusion and pursue a search for what physical events may be responsible for the illusion. Gibson and his followers are correct, perception is direct. They are wrong if and when they think that this means that a constructional brain process is ruled out or that the percept invariably and directly gives evidence of the physical organization that gives rise to perception.

As noted, there is altogether too much evidence in support of a brain

constructional theory of perception. The holonomic model, because of its inclusion of parallel processing and wave interference characteristics, readily handles the data of projection and illusion that make up the evidence for direct perception. The holonomic model also accounts for the "directness" of the perception: holographic images are not located at the holographic plane, but in front or beyond it, away from the construction apparatus and more into the apparently "real," consensually validatable external world.

In the concluding part of this paper, I want, therefore, to explore some questions as to the organization of this external "real" physical world. Unless we know something of consensually validatable "information" that remains invariant across transformations of the input to the brain—and, as we have seen, we cannot rely only on the directness of our perceptual experience for this knowledge—how can we think clearly about what is being perceived? Questions as to the nature of the physical universe lie in the domain of the theoretical physicist. Physics has enjoyed unprecedented successes not only in this century, but in the several preceding ones. Physics ought to know something, therefore, about the universe we perceive. And, of course, it does. However, as we shall shortly see, the structure-distribution problem is as pervasive here as it is in brain function.

The special theory of relativity made it clear that physical laws as conceived in classical mechanics hold only in certain circumscribed contexts. Perceptions of the Brownian "random" movements of small suspended particles, or of the paths of light coming from distances beyond the solar system, strained the classical conceptions to the point where additional concepts applying to a wider range of contexts had to be brought in. As in the case of direct perception, the laws of physics must take into account not only what is perceived but the more extended domain in which the perception occurs. The apparent flatness of the earth we now know as an illusion.

The limitations of classical physics were underscored by research into the microcosm of the atom. The very instruments of perception and even scientific observation itself became suspect as providing only limited, situation related information. Discrepancies appeared, such as an electron being in two places (orbits) at once or at best moving from one place to another faster than the speed of light—the agreed upon maximum velocity of any event. And within the nucleus of the atom matters are worse—a nuclear particle appears to arrive in one location before it has left another. Most of these discrepancies result from the assumption that these particles occupy only a point in space—thus when the equations that relate location to mass or velocity are solved,

they lead to infinities. Furthermore, in the atomic universe, happenings take place in jumps—they appear to be quantized, i.e., particulate. Yet, when a small particle such as an electron or a photon of light, passes through a grating and another particle passes through a neighboring grating, the two particles appear to interact as if they were waves, since interference patterns can be recorded on the far side of the gratings. It all depends on the situation in which measurements are made whether the “wavicle” shows its particle or its wave characteristics.

Several approaches to this dilemma of situational specificity have been forwarded. The most popular, known as the Copenhagen solution, suggests that the wave equations (e.g., those of Schroedinger, 1935 and deBroglie, 1964) describe the average probabilities of chance occurrences of particulate events. An earlier solution by Niels Bohr (the “father” of the Copenhagen group, 1966) suggested that particle and wave were irreconcilable complementary aspects of the whole. Heisenberg (1959) extended this suggestion by pointing out that the whole cannot in fact be known because our knowledge is always dependent on the experimental situation in which the observations are made. Von Neumann (1932) added that, given a positivistic operational framework, the whole reality becomes, therefore, not only unknown, but unknowable. Thus, the whole becomes indeterminable because we cannot in any specific situation be certain that what we are observing and measuring reflects “reality.” In this sense, as well as from the viewpoint of brain processes, we are always constructing physical reality. The arguments of the quantum physicist and those of the neurophysiologist and psychologist of perception are in this respect identical.

But several theoretical physicists are not satisfied with these solutions or lack of solutions. Feynman (1965), for instance, notes that though we have available most precise and quantitative mathematical descriptions in quantum mechanics, we lack good images of what is taking place. (His own famous diagrams show time flowing backwards in some segments!) DeBroglie, who first proposed wavelike characteristics for the electron, fails to find solace in a probabilistic explanation of the experimental results that led him to make the proposal (1964). And DeBroglie is joined by Schroedinger (1935), who formulated the wave equation in question and especially by Einstein, whose insights led him to remain unconvinced that an unknowable universe, macro- and micro-, was built on the principle of the roulette wheel or the throw of dice.

I share this discomfort with attributing too much to chance because of an experience of my own. In the Museum of Science and Industry in

Chicago, there is a display which demonstrates the composition of a Gaussian probability distribution. Large lead balls are let fall from a tube into an open maze made of a lattice of shelves. The written and auditory explanations of the display emphasize the indeterminate nature of the path of each of the falling balls and provide an excellent introduction to elementary statistics. However, nowhere is mention made of the symmetrical maze through which the balls must fall in order to achieve their probabilistic ending. Having just completed *Plans and the Structure of Behavior* (Miller, Galanter and Pribram, 1960), I was struck by the omission. In fact, students of biology routinely use statistics to discover the orderliness in the processes they are studying. For example, when a measurable entity shows a Gaussian distribution in a population, we immediately look for its heritability. Perhaps the gas laws from which statistics emerged have misled us. A Gaussian distribution reflects symmetrical *structure* and not just the random banging about of particles. Again, the physical reality behind the direct perception may contain surprises.

Moreover, when we obtain a probabilistic curve, we often refer to a distribution of events across a population of such events—e.g. a Gaussian distribution. Could it be that for the physical universe, just as in the case of brain function, structure and distribution mutually interact? After all, the brain is part of the physical universe. For brain function, we found structure to be in the form of program and distribution in the form of holograms. Is the rest of the physical universe built along these lines as well?

David Bohm (1957), initially working with Einstein, has, among others, made some substantial contributions to theoretical physics compatible with this line of reasoning. Bohm points out, as noted above, that the oddities of quantum mechanics derive almost exclusively from the assumption that the particles in question occupy only a point in space. He assumes instead that the "wavicle" occupies a finite space which is structured by subquantal forces akin to electromagnetic and gravitational interactions. These interacting forces display fluctuations—some are linear and account for the wave form characteristics of the space or field. Other interactions are nonlinear (similar to turbulence in fluid systems) and on occasion produce quantal events. In biology, Thom (1972) has developed a mathematics to deal with such occurrences in the morphogenetic field and this mathematics has been applied to perception by Bruter (1974). Thom calls the emergence of quasi-stable structures from turbulent processes "catastrophes." In physics, the quantal structures that result from such catastrophic processes may, therefore, be only partially

stable. Thus, they can disappear and reappear nearby in a seemingly random fashion; on the average, however, they would be subject to the more regular oscillations of the subquantal forces. In biology, observations pertaining to the entrainment of oscillatory processes by clocks or temporary dominant foci parallel these concepts. Bohm goes on to point out where in the subquantal domain these events will become manifest: the interactions of high frequency and high energy particles in nuclear reactions, in black bodies, etc. An article in *Scientific American* reviews the contemporary scene in these attempts at a Unified Field Theory in the subquantal domain (Weinberg, 1974).

Bohm (1971, 1973) has reviewed the conceptual development of physics from Aristotelian through Galilean and Newtonian times to modern developments in the quantum mechanics. He points out how much of our image of the physical universe results from the fact that, since Galileo, the opening of new worlds of inquiry in physics has depended on the use of lenses. Lenses have shaped our images and lenses objectify. Thus, we tend to assess external space in terms of objects, things and particulars.

Bohm goes on to suggest that image formation is only one result of optical information processing and proposes that we seriously consider the hologram as providing an additional model for viewing the organization of physical processes. He and his group are now engaged in detailed application of this basic insight to see whether, in fact, a holographic approach can be helpful in solving the problems of high energy nuclear physics. Initial developments have shown promise.

As noted above, the subquantal domain shows striking similarities to holographic organization. Just as in the case for brain processes presented here, Bohm's theoretical formulations retain classical and quantum processes as well as adding the holographic. The holographic state described by wave equations and the particle state described quantally, are part of a more encompassing whole. The parallel holds because the holographic models describe only the deeper levels of the theory which is thus holonomic, rather than holographic, as we found it to be for the special case of brain function (where the deeper level is constituted of pre- and post-synaptic and dendritic potentials and the quantal level, of the nerve impulses generated by these slow potentials).

Bohm relates structural and holographic processes by specifying the differences in their organization. He terms classical and particle organization *explicate* and holographic organization *implicate*. Elsewhere (Pribram, 1976), I have made a parallel distinction for perceptual processes: following Bertrand Russell (1959), I proposed that scientific analysis as we practice it today, begets knowledge of the

extrinsic properties (the rules, structures, etc.) of the physical world. My proposal departs from Russell, however, in suggesting that intrinsic properties (which he defines as the stoneness of stones, e.g.) are also knowable—that in fact they are the “ground” in which the extrinsic properties are embedded in order to become realized. Thus artists, artisans and engineers spend most of their time realizing the extrinsic programs, laws and rules of the arts and sciences by grounding them in an appropriate medium. For example, a Brahms symphony can be realized by an orchestra, on sheet music, on a long-playing record or on tape. Each of these realizations comes about after long hours of development of the medium in which the realization occurs. Russell was almost correct in his view that the intrinsic properties of the physical world are unknowable—they have apparently little to do with the more enduring extrinsic properties, show no resemblances amongst themselves and demand considerable know-how to replicate.

The sum of these ideas leads to the proposal that the intrinsic properties of the physical universe, their implicate organization, the field, ground or medium in which explicit organizations, extrinsic properties become realized, are multiform. In the extreme, the intrinsic properties, the implicate organization, are holographic. As extrinsic properties become realized, they make the implicate organization become more explicit.

The consequence for this view is a revaluation of what we mean by probabilistic. Until now, the image, the model of statistics, has been indeterminacy. If the above line of reasoning is correct, an alternate view would hold that a random distribution is based on holographic principles and is therefore determined. The uncertainty of occurrence of events is only superficial and is the result of holographic “blurring” which reflects underlying symmetries (much as does the Gaussian distribution in our earlier example) and not just haphazard occurrences. This relation between appearance and reality in the subquantal domain of nuclear physics and its dependence on underlying symmetries (spin) is detailed in the review article in *Scientific American* already referred to (Weinberg, 1974).

A preliminary answer to the question posed at the outset of this section—what is it that we perceive—is, therefore, that we perceive a physical universe not much different in basic organization from that of the brain. This is comforting since the brain is part of the physical universe as well as the organ of perception. It is also comforting to find that the theoretical physicist working from his end and with his tools and data has come to the identical problem (which is, in Gibson’s terms, the nature of the information which remains invariant across

situations) faced by the neurophysiologist and psychologist interested in perception (Bohm, 1965, Appendix). Though surprising, the fact that at least one renowned theoretical physicist has made a proposal that addresses this common problem in terms similar to those set forth on the basis of an analysis of brain function, is most encouraging. For science is of a piece, and full understanding cannot be restricted to the developments made possible by one discipline alone. This is especially true for perception—where perceiver meets the perceived and the perceived meets the perceiver.

Conclusion

But perhaps the most profound insight gained from holography is the reciprocal relationship between the frequency domain and the image/object domain. A fundamental question that is raised by this duality is whether mind should be conceived only as an emergent property resulting from the interaction of an organism with its environment, or whether mind truly reflects *the* basic organization of the universe (including the organism's brain). Images are mental constructions. They result from processes involving the brain (object), the senses (objects) in their interactions with the environment (considered objectively, i.e., as objects, particles such as photons, electrons, atoms, molecules and the objects of the reality of appearances). Images (one aspect of mind) are thus emergents in any objective, object-i-fying philosophical formulation. But the process of image construction involves a reciprocal stage, a transformation into the frequency (holographic) domain. This domain is characteristic not only of brain processing, as we have seen, but of physical reality as well. Bohm refers to it as the implicate order in which points become enfolded and distributed throughout the domain.

In the implicate, holographic domain the distinction between points becomes blurred, information becomes distributed as in the example of the surface of a pond. What is organism (with its component organs) is no longer sharply distinguished from what lies outside the boundaries of the skin. In the holographic domain each organism represents in some manner the universe and each portion of the universe represents in some manner the organisms within it. Earlier in this paper this was expressed in the statements that the perceptions of an organism could not be understood without an understanding of the nature of the physical universe and that the nature of the physical universe could not be understood without an understanding of the observing perceptual process.

It is, thus, the fact that the holographic domain is reciprocally related to the image/object domain that implies that mental operations (such as mathematics) reflect the basic order of the universe. Of special additional interest is one characteristic of the holographic order. This domain deals with the density of occurrences only; time and space are collapsed in the frequency domain. Therefore, the ordinary boundaries of space and time, locations in space and in time become suspended and must be "read out" when transformations into the object/image domain are effected. In the absence of space-time coordinates, the usual causality upon which most scientific explanation depends must also be suspended. Complementarities, synchronicities, symmetries and dualities must be called upon as explanatory principles.

Scientists are, as yet, only barely acquainted with the implicate holographic order. I believe, however, that it is this order which is being explored experientially by mystics, psychics and others delving in paranormal phenomena. Perhaps if the rules for "tuning-in" on the holographic implicate domain could be made more explicit we could attain that scientific understanding of putative paranormal phenomena that we aim for in conferences such as this. As set out in the introduction, true scientific sharing depends on this base of understanding, not just on proving the reliability of experimental realities. I believe that the paradigm shift in science, occasioned by the insights obtained in quantum physics and carried forward by the holographic model of brain function, will, in fact, provide us with that base of understanding which makes it clear that the world of appearances is but a reciprocal of another reality, a reality that may already have been explored experientially for untold millennia.

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DISCUSSION

BELOFF: I'm going to start with a very simple point, because I get lost in anything more than just the simple parts of the exposition. You began by talking about the holographic theory of memory. As a psychologist, one point that has always worried me about this theory since I first came across it is that it's always been assumed by psychologists, certainly since the work of Frederic Bartlett, that memories are never a literal reproduction of past experiences. Yet, when I think about your hologram analogy, I can see how it would work very nicely for reconstructing an image exactly as it was originally presented. But I can't quite see how you are going to account for all the kinds of distortions and errors that human memory reveals.

PRIBRAM: I puzzled about this a great deal, because I don't know that the facts are. We know what Sir Frederic Bartlett came up with, but then there are also people who come up with veridical memories—Mimi Strohmeyer, for instance, is one person who was mentioned yesterday who has eidetic imagery and she, for instance, can tell the difference between ordinary memory and eidetic memory. The holographic store when tapped directly produces eidetic imagery. When retrieval mechanisms intervene, the dismembered holographic store becomes re-membered and distortions can take place.

MCGUINNESS: Would you expand the philosophical point of view in connection with interference patterns and holograms? Are our brains

responsible for space and time? Would space and time exist without brains?

PRIBRAM: That's an important question. First of all, we know that events are stored in a spaceless and timeless fashion in our brain. I've given this talk in a variety of ways; sometimes it takes two hours. Today I did it in half an hour. It's all in my head packed together, and how it comes out is a matter of programming it out. That's why the computer analogy of retrieval is so helpful, because it tells us about how we can sequence our behavior. If things are packed this way in our heads, what produces time and space? It is not the right and left hemispheres, as was suggested yesterday. We cannot take these two things apart in that way. If, in fact, storage is organized as a hologram, we then retrieve time and space by moving with respect to our input. According to evolutionary theory, we have pretty well adapted to an environment which is constituted much as it appears to be: there are organisms that are moving with respect to each other, etc. However, there is no way of knowing whether *we* are constructing the images of objects and their movement, because our brain organization has evolved to match such an environment, or whether we are simply constructed in this fashion. For instance, in our laboratory we have shown that the cells in the brain react to visual white noise exactly as they do to a line. Thus, we don't know whether what we see is the result of built in brain machinery and that therefore the world of appearances is simply an artifact of the way we're built, or whether the way we're built is the result of the way the world of appearances has shaped us. The same problem faces physicists at this point. Wigner noted that physicists are now dealing with relations among observations, and no longer with relations among observables. So we cannot tell whether it's our instrumentation that's producing the results, or whether they are "really there."

HONORTON: I don't see how you have a scientific explanation here. I don't see how you're solving the mind/brain problem any more than Popper and Eccles are. It seems to me that you're calling the mystery by another name and providing an "explanation," at this stage at least, that to me is not obvious in terms of what its scientific implications are for further research. By analogy with this, is mind the background film on which all this is displayed, or what?

PRIBRAM: What I'm saying is that we must transcend dualism *and* understand it. There are two positions that Popper and Eccles have confounded. One states that the brain is a generator, or, using the words that we used yesterday, mind is an emergent property of

behaving organisms. The other is a pan-psyche notion of mind, an extended mind, if you will, which operates *on* the brain. Mystics see conscious mind as being everywhere. Similarly, in a holographic reality, there are no boundaries. I'm suggesting that, in fact, when people have mystical experiences they tune in on this particular aspect of the duality. Since we can easily go back and forth between these two realities mathematically, perhaps there are ways in which organisms also go back and forth between the two.

You want to know what kind of experiments are to be done. Obviously, there are experiments on paranormal phenomena. Other types of experiments could be done at the quantum level in physics to find out whether in fact quarks "exist." Are quarks nothing more than nodes in a holographic matrix? Physicists are aware of such a possibility—that's why they have colors and flavors for quarks to remind us of the subjective element in their definition.

RUDOLPH: I have a comment and a question. The comment concerns the work of Lawrence LeShan on the Clairvoyant Reality versus the Sensory Reality. I've considered the possibility that these realities might, in some sense, be Fourier transforms of one another and this point of view seems to work. Because of the relationship between holography and Fourier transforms, I think this may be related to your work. My question is this. If the left brain/right brain dichotomy doesn't correspond to time and space, and it seems reasonable to me that it doesn't, does it perhaps correspond to the two transform domains?

PRIBRAM: No. I think we're trying to make too much of the left and right brain dichotomy and simply impose it on every dichotomy that exists to see if it fits. If we must oversimplify let us say that the right brain essentially is a visual brain, it makes visual images. The left brain is an auditory one and make auditory images. Now, when the auditory system begins to pull in more sequential properties, the visual system pulls in more spatial relationships and so the two hemispheres get to have somewhat different properties. But one must have holographic memory on the left as well as on the right side. Else how could we remember what to say?

DIXON: Some people have espoused an RNA theory of memory, which is another "distributed" theory of memory. It seems to me, thinking about it now, that this is not inconsistent with your hologram. What do you feel about that?

PRIBRAM: It can't be RNA *per se*, because RNA doesn't last long

enough, but it certainly can be RNA producing some change in a protein molecule. If we're going to have anything permanent, we must change the conformation of a protein, but the techniques are not far enough advanced as yet to study such conformational changes in the brain.

DIXON: So that could be the physical basis of the hologram, possibly?

PRIBRAM: Right. If I'm right that memory is holographically encoded, we need to find out whether storage is in terms of the square of the intensity of the input rather than just the intensity itself. That would be all that would be necessary—that, and to maintain the phase of neighboring relationships. Perhaps this sort of experiment can be done on invertebrates, but most likely it needs to be explored in a brain where there is some sort of cortex. Such experiments can be outlined right now, as suggestions of what we might do once the techniques in chemistry become available.

OLMEDO: You seem to imply that your hologram fits moment to moment, although that may not be the fact in reference to the reality we perceive—how I perceive you now, how I perceive my neighbors. How is it that I can perhaps have an experience over which I have no control of how my brain would perceive you at that moment?

PRIBRAM: One of the ways I like to express this is that the *Veda*, which was written anywhere from ten to eight thousand years ago, has many of the illuminations that we're only learning about now in modern physics and modern biology. How did these ancients get hold of all this knowledge? Perhaps they just tuned in on all of our findings and plagiarized them. We do all the work in the here and now and they tune in ten thousand years ago on what we're going to find out next year. That's a facetious way of putting it, but it gets across the point.

EHRENWALD: I'm overawed by the vistas opened up by this discussion and by Pribram's holographic image of the world. It seems to be a new revolutionary paradigm. But let me remind you that paradigms have changed over the millenia and so have theories about brain/mind relationships. There was the Greek pneumatic notion of the soul. It was replaced, let's say, by the hydraulic paradigm of Freud's metapsychology.

PRIBRAM: Freud did not have any hydraulic imagery whatsoever. It was electric. I published a book a year ago on Freud's "Project." There is no hydraulic imagery in it. He tries to come up with an Ohm's law of the nervous system. It's all electric. The English translations are so poor

that they have made it sound as though Freud's model were a hydraulic, because the translators knew no neurology. We mustn't do Freud in. He was a great neurologist and knew that the nervous system did not work by way of water pumps.

EHRENWALD: Whether we call it hydraulic or electric, we have in any case a different paradigm proposed by psychoanalysts. It grew into Freud's metapsychology and it is a distinctly hydraulic metaphor. Freud's "Project" was later abandoned because it did not go far enough. It was replaced by the metaphor of the brain as a giant telephone switchboard and later transformed into the metaphor of the computerized brain. Today we have Dr. Pribram's holographic metaphor. It is certainly a revolutionary new paradigm, telling us that the individual—or the homunculus—who ultimately reads the computer printouts is no longer necessary, that he can be dismissed. Yet I submit that this is a kind of epistemological sleight of hand because the homunculus goes only into hiding if and when we go into nirvana, into an egoless mystical state of mind. But as soon as we return, it is he or I or the homunculus or the son of homunculus who sneaks in through the backdoor, as it were, and the individual, or Eccles' "self-conscious mind," is back and tries to be in control again. After dismissing Cartesian dualism we are back where we started. As a result of an existential shift, when we shift from the clairvoyant reality or the psi form of reality to the here and now reality, the ego is again back in command and this is how we are communicating with one another. I think that the holographic imagery is extremely helpful, but does not do away with the ultimate questions which you can formulate in different forms—religious, epistemological, or otherwise.

PRIBRAM: I would only point out that it is no longer a metaphor. I gave you a brief account for the evidence that the brain does, in fact, work like this. Freud's "Project" does contain passages which clearly suggest that the cortex is a frequency analyzer; he calls it an analyzer of periodicities. The model has not changed from 1895 till now. I think the insights given by the data that we have now must be appreciated as being really revolutionary, but not in any sense that these insights have not been had before. Leibnitz, the inventor of the mathematics that Gabor used to invent the hologram for which he received the Nobel Prize—Leibnitz had this insight several centuries earlier. But we've attained more and more precision in these insights, and what we used to talk about as mystical, paranormal, etc., now falls within the purview of science. We now have available a science to deal with these phenomena.