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Toward a holonomic theory of perception*

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With 3 figures

Introduction

The face of psychology has undergone a series of changes during a century of growth as a science. Initial concerns with sensory processes, (as, for instance, in the hands of Helmholtz and Mach) and thought (as studies by *Külpe*, *Brentano* and *James*) gave way to investigations of feelings (e.g., *Wundt*) and motivations (e.g., *Freud*). The introspectionism of Titchener was succeeded by the factors of *Spearman*, *Thurstone*, and *Cattell* and by the behaviorism of *Watson*; the Gestalts of *Koffka*, *Köhler*, *Wertheimer* and *Metzger* were pitted against the learning theories of *Pavlov*, *Hilgard*, *Hull*, *Spence*, *Tolman* and *Skinner*. Each of these faces has left a legacy which can be traced through its descendants and the variety of their modifications, techniques and formal statements of what constitutes psychology, and attests to the vigor of this young science.

During the past quarter century, the ferment has continued. The major influences now are seen to be existential encounter on the one hand and structural analysis based on computers and mathematics on the other. Superficially, it appears as if the earlier apposition of Gestalt to learning theory had gone to extremes: wholism transcendent vs mechanism transistorized. But this would be superficial reading. A number of transcendentalists are beginning to be seriously concerned with physiological and social mechanisms as explanations of the philosophical teachings of Zen, Tantra and other eastern experiential systems, while, the mechanists have gone cognitive, allowing considerable fluidity and introspective latitude to the models they construct with their computers and mathematics.

The question I want to address, therefore, is whether the time is perhaps ripe for a more comprehensive view of psychological processes — a view that would encompass not only the variety that is psychology, but play a serious role in the scientific *Zeitgeist* as a whole. Meanwhile, because each current endeavor in psychology, as part of science, is deeply rooted in its technology, the confusion between disciplines continues to be aggravated. Loyalty is often to the discipline or subdiscipline, not to the content of psychology. Thus several groups, though pursuing the same problems, fail to communicate because of the technical jargon developed in each group, often even to the use of identical words to convey different referents.

My concern with the problem of disparate theoretical and technical descriptions is a very practical one. I have spent this quarter century performing experiments that purport to relate brain function and behavior to mental processes as these are expressed by verbal (and nonverbal) reports of my fellow

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humans (often in a clinical situation). In my attempts to communicate the specific fruits of the research results, I have related the function of the frontal cortex of primates to conditional operants; to decisional processes in ROC space; to attention as measured by eye movements, GSR, heart rate changes and reaction time in the presence of distractors; to motivation in relation to food deprivation and pharmacological manipulations; to learning as a functional change in performance; to the structure of memory using computer simulation; and to other brain processes by neuroanatomical and electrophysiological investigations. Intuitively, I feel that what I have found out about frontal lobe function (and the limbic system function, and temporal lobe function, etc.) is important not only to brain physiology, but to psychology — and this intuition is shared by most psychologists. Yet in trying to understand and communicate what I have discovered, I come up against a myriad of systems and beliefs: operant conditioners, decision theorists, attention theorists, motivation theorists, learning theorists, memory theorists and neuroscientists of various disciplinary persuasions (e.g., microelectrode artisans, evoked potential analysts, the CNV specialists or EEG computationists, let alone the neurochemists and neuropharmacologists) rarely relate their findings to one another. What is the connection between learning and memory, between attention and decision, between motivation and the various electrical manifestations of brain function? There is no universally agreed answer. It is as if in the physical sciences we did not know the relationship between the moons and their planets, between the solar system and galaxies, between atomic and molecular structure, between mechanical, gravitational and electromagnetic forces.

In short, if I am to make sense of my data, I must come to grips with the multiple framework within which these data have been gathered — the framework we call scientific psychology. This is the task I want to address. Only an outline, a proposal can be entertained in this paper. The detailed fitting of data, working the outline into a coherent body of scientific knowledge will require a more comprehensive effort over the next decades.

The proposal is contained in the holonomic theory. As the name suggests, the theory is holistic. It therefore addresses the interests of Gestalt, of existential concerns, of social encounter and transcendence. However, it is rooted in the disciplines of information, computer and systems analysis and thus aims toward expression of facts in precise mathematical form. The theory, because of its comprehensiveness, has philosophical implications (see e.g., *Pribram*, 1965, 1971a, 1971b, in press) but its corpus concerns the relationship of neural, behavioral and experiential levels of inquiry. At this stage, the theory must of necessity be primarily inductive, relying on a systematization of available data and drawing upon metaphor and analogy from more advanced knowledge concerning other physical, biological and social organizations for initial model construction.

In this paper I want, in the tradition of empiricism, to discuss the holonomic theory as it concerns problems of consciousness, perception, imagining and attention, because, as will be shown in the last section of this paper, in a very real sense this area of problems is central to a scientific understanding of anything at all and especially of psychology. My point of departure is brain organization and function as it relates to observations of the behavior (including verbal reports of experience) of the organism in which the brain is functioning.

The departure proceeds from a conflict of views which opposes holistic to analytic processes. The following account hopes to show that such opposition is unwarranted, that in fact both types of process occur in the brain and that their interaction is coordinate with perception.

The Brain and the Computer

One of the most challenging discoveries about brain organization concerns the precise connection between parts of the brain and between these parts and the topography of bodily surfaces. Localization of connections predicts a localization of function. Grossly, this prediction is often confirmed: for example, eyes and ears and nose project by way of nerve tracts to separate parts of the brain and when these parts are damaged, stimulated or electrically analyzed, a correspondence is obtained between anatomical projection and sensory function. The challenge is posed by the precision of the connections. Assignment of a precise function to a particular anatomical arrangement does not come easily. One investigator, Karl Lashley, has even despaired of ever making such assignment and suggested that the anatomy may represent a vestigial residue of some phylogenetically earlier functional organization, much as our veriform appendix represents an earlier functional digestive organ (*Lashley, 1960*).

The problem arises from the fact that large holes can be made in the anatomical organization of the brain without severely disturbing some functions that would be expected to depend on this precise organization. This does not mean that holes in the brain have no effect: when made in the sensory projection areas, for instance, such holes produce scotomata in the appropriate sensory receptive field. However, very little disturbance of sensory, perceptual, attentional, memory or other psychological process can be ascertained when tests are made within the remaining intact field. The remaining brain-behavior field, the remaining neural organization appears capable of taking over, functioning in lieu of the whole — the system shows equipotentiality as *Lashley* put it (*Lashley, 1960*). Currently, we would say that the sensory input becomes distributed over the reach of the projection system. The question arises, therefore, how.

An alternative to *Lashley's* phylogenetic argument is to look at current data processing systems for an appropriate analogy. General purpose computers are wired with very specific connections. Yet, one day, in the early period of computer technology, I experienced the following incident: The then current Stanford machine had been sold to a nearby commercial bank to make way for a new installation. Unfortunately, I had collected a batch of irreplaceable data on patients who had received frontal lobotomies some ten years earlier (*Poppen, Pribram and Robinson, 1965*), in a tape format tailored to the existing computer. Learning of the replacement only at the last moment, we rushed to the computer center to process our tapes. Much was completed in the next two days and nights, but a small amount of work still needed to be done when, on the third day, dismantling for shipment was begun. We discussed our problem with the person in charge, hoping to delay things by the crucial three or four hours we needed to finish our task. Much to our surprise he said, "go ahead and keep processing your tapes, we'll begin the dismantling in such a way as not to disturb you." We were grateful and expected peripherals and cabinets

to be tackled first, only to witness the removal of assemblies of switches and tubes from the innards of the machine. Our data processing meanwhile proceeded merrily without any interruption of the cadences to which we had become accustomed. Though we expected the whole affair to come prematurely to a grinding halt at any moment, this did not happen and we gratefully acknowledged the seeming equipotentiality of the man-made brain that had given us such excellent service.

Could it be, that our biological brains, though "wired" as precisely as any computer, are organized in a similar way — i.e., to be a generalpurpose instrument that, when properly interfaced and given proper bootstrap programs to get the "machine" going, can then handle more complex higher order programs with seeming equipotentiality? Why not? The underlying principles of the operation of biological and hardware brains may be sufficiently similar to warrant such an explanation. An early book with George Miller and Eugene Galanter explored this possibility (*Miller, Galanter and Pribram, 1960*) and more recently I presented the neurophysiological and neurobehavioral evidence in support of this approach, pointing out as well, however, the divergences and differences between biological brains and computers (*Pribram, 1971a*).

One difference involves the very problem of specificity of connections which initiated the present discussion. Computers currently are primarily serial and therefore analytic processors — one event leads to another. Brains, to a much larger extent, are parallel and therefore holistic processors — many related events occur simultaneously.

In an attempt to simulate biological brains on the computer, scientists have constructed programs utilizing highly interconnected hardware which are called random-net configurations. Though these do approximate an aspect of human perception, the constructive aspect (*Neisser, 1967*), they nevertheless fail when tested against the general characteristics of the human perceptual system (*Minsky and Papert, 1969*), and fail equally to correspond to the anatomical specificity of the human system in which sensory projections are topologically discrete.

These limitations of hardware simulations have been discouraging to those who felt that current computers were, at least in principle, models of biological brains, and have provided fuel for those who would like to reject the use of mechanistic analogies to the nervous system.

Another interpretation is possible, however. Perhaps we have gained only a partial insight into brain function by stressing essential similarities to the organization of computers. Perhaps what is needed, in principle, is a look at another type of organization conducive to parallel processing, working in conjunction with that represented by present-day computers.

The Brain and the Hologram

There is a set of physical systems that meets these requirements — i.e., they display the essentials of parallel processing. These are optical (lens, prism, diffractions, etc.) systems — often called optical information processing systems to distinguish them from the systems of digital switches comprising the computer mechanisms through which programmable information processing is conducted. In optical systems "connections" are formed by the paths which light traverses and

light bears little physical resemblance to the electrochemical energy that is the currency of both brain and computer. Thus the analogy must at once be seen as more restricted. What is to be taken seriously is the analogy between the *paths* taken by the energy, the interactions among these paths and the resulting organizations of "information" that are produced. Elsewhere, I have, with Nuwer and Baron, discussed possible (and even on the basis of current evidence some probable) physical correspondences between optical and brain systems with respect to these information processing capabilities (*Pribram, Nuwer and Baron, 1974*).

The essence of optical information processing systems is their image construction potential. This capacity is to be compared and contrasted with the programming potential of the computer. Neither programs nor images reside as such in the information processing system — they are configurations made possible by the constrictions of the system. Both images and programs can be captured and stored as such outside their processing systems. When this is done, there appears to be no superficial resemblance between the image or program and the system in which processing takes place, nor even with any readily recordable event structure that occurs during processing. This is because the topography of images and the statements of programs are re-presentations of the process and as such are subject to transformation. The job of the scientist is to specify the transformations that occur between image and optical information processing system and between program and computer. The power of these analogies to brain function comes when the mathematical description of these transformations can be shown by experiment to be identical for information processing by the brain as for processing by optical and computer systems. When in addition, the physical components responsible for the transformations are identified, a model of brain function can be constructed and tested deductively by subsequent experiment.

Images and programs are patently different constructions and a good deal of evidence is accumulating to show that in man the right hemisphere of the brain works predominantly in an image mode while the left hemisphere function is more compatible with program processing (see reviews by *Sperry, 1974; Milner, 1974; Gassaniga, 1970*). There is also a considerable body of evidence that this hemisphere specialization is derived from an earlier mammalian pattern of image construction by the posterior-lateral portions of the brain based on somatotopic and visual input, contrasted with a more sequential organization of the fronto-medial (limbic) systems by olfactory and auditory input (see *Pribram, 1960 and 1969* for review). These dichotomies are not exclusive and hold only for overall functions — there are many sequential processes involved in image construction (as for instance scanning by the eye of a pictorial array) and there are parallel processes involved in programming (for example, the conducting of a symphony or even the appreciation of auditory harmonics). Yet the fact that neurobehavioral data readily distinguish image and program processing suggests that both must be taken into account in any comprehensive understanding of psychological function.

By contrast to programs, images can be comprehended in their totality even after brief exposures to the energy configurations they represent. They tend to be wholistic rather than analytic, e.g., they tend to completion in the absence of parts of the input ordinarily responsible for them. Also, they tend to be

“good” or “bad” on the basis of the structure of the redundancy of their components (*Garner, 1962*). (Programs, on the other hand, have no such internal criteria for goodness. A program is good if it works — i.e., is compatible with the computer — and is better if it works faster. When, as in a musical composition, esthetic criteria can be applied, they pertain to the image-producing properties of programs, their compatibility rather than their internal structure.) In short, imaging obeys Gestalt principles (which were first enunciated in the visual arts) as would be expected, while programming takes its kinship from linguistics. Both have gained precision and a new level of understanding by recourse to information measurement and processing concepts.

Over the past fifteen years investigating the details of brain function and of psychological processes, in terms of information processing of the programming type, has become reasonably well accepted. Understanding brain function in terms of information processing as in optical systems, leading to image formation, is a more recent endeavor. Yet a sizable body of evidence has accrued to show how parts of the brain are in fact organized so as to construct images.

The Evidence

Much of the recent evidence concerning image formation in the visual system has been provided by *Fergus Campbell* and his associates. They have established that the visual system is sensitive to the spatial frequencies in pattern of light, much as the auditory system is sensitive to the temporal frequencies in pattern of sound. This sensitivity has been shown both at the cellular level in animals (*Enroth-Cugall and Robson, 1966; Campbell, Cooper and Enroth-Cugall, 1969; Campbell, Cooper, Robson and Sachs, 1969*) and in experiments on human psychophysics (*Campbell and Kulikowski, 1966; Campbell and Robson, 1968*). One of the most important findings from these studies illustrated that the visual system exhibits a systematic tendency to respond to the harmonics of a square-wave grating. This was demonstrated at threshold (*Campbell and Robson, 1968*) where contrast sensitivity for a square-wave grating was significantly affected by the contrast threshold of its third harmonic, and similarly *Blakemore and Campbell (1969)* found that adaptation to a fundamental frequency increased threshold for the third harmonic of that frequency. *Campbell* reasoned, therefore, that the visual mechanism must, much as does the auditory system, decompose any complex wave form into its components, as is done in a procedure developed by *Fourier* to specify the characteristics of wave forms. Whether in fact the visual mechanism serves as a *Fourier analyzer* is being tested in several laboratories at the moment by psychophysical experiments (e.g., *Stromeyer*, in press a; in press b; submittal). What is necessary is to determine the bandwidth of various channels sensitive to one or another spatial frequency. *Campbell's* analysis suggested that bandwidths of approximately an octave were involved — a finding consonant with the suggested *Fourier* mechanism (*Blakemore and Campbell, 1969*).

These findings have been confirmed and extended in several laboratories. *Maffei and Fiorentini (1973)* reported that visual cells functioned in fact as *Fourier analyzers*. *Pollen (1971; 1974)* determined that the medium band width spatial frequency sensitive cells were the “complex cells” of visual cortex that

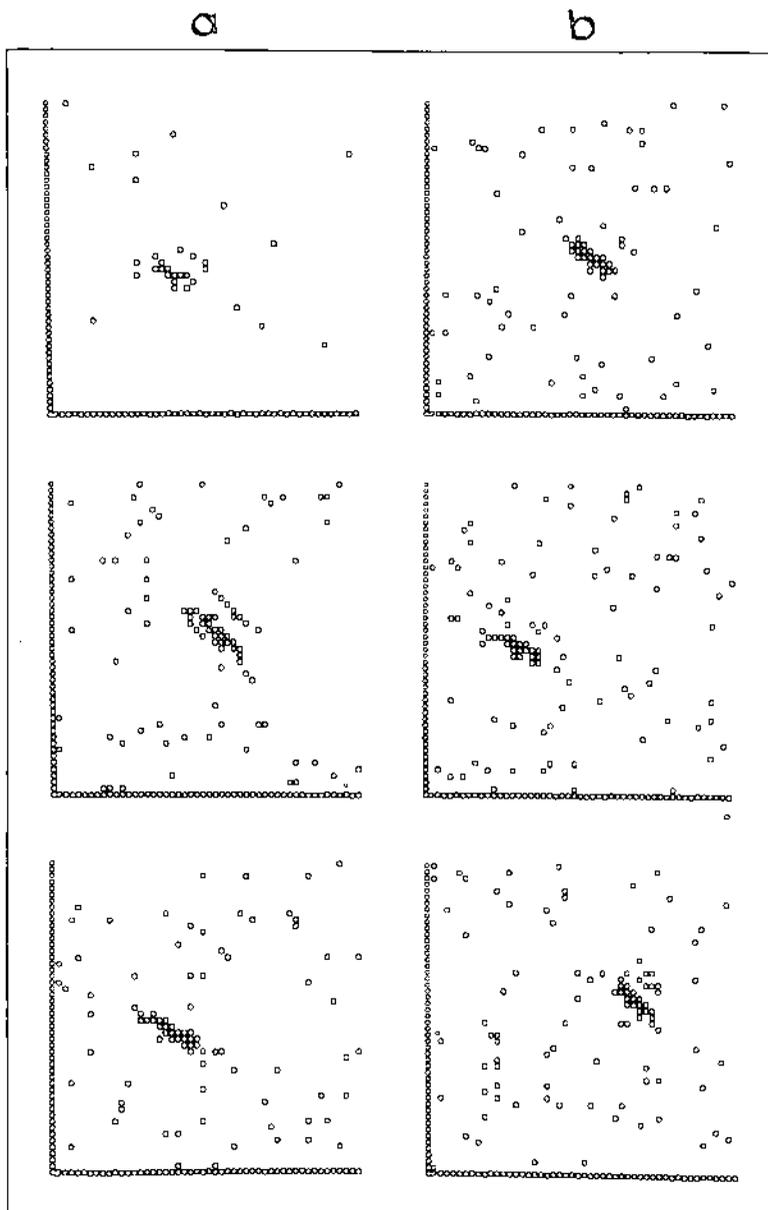


Figure 1: Visual receptive fields plotted with a moving dot stimulus. a) right eye; b) left eye. 1, 2 and 3 are different units. Note the inhibitory flanks next to the main elongated field and that in several of the fields there is a secondary excitatory region.

had hitherto been thought sensitive to single bars presented at a certain orientation ("simple cells by contrast are sensitive to edges, which can be interpreted as a sensitivity to the very high end of the spatial frequency domain). This result was independently obtained by a group of Soviet investigators in Leningrad (*Glezer, Ivanoff and Tscherbach, 1973*).

There can thus be little doubt that spatial frequency analysis is one function of the visual mechanism. What has this to do with image construction? As already noted, perceptions have more or less unsuccessfully attempted to make images by the additive sequential and hierarchical process of putting together a figure from the dominant features that compose it. Thus the outlines of a house can be constructed from lines and corners. What is lacking in such a construction is the rich detail, the resolution and fine grain that characterizes our subjective experience of images. This lack is overcome when image construction is based on a spatial frequency mechanism.

Computer simulation highlights the resolving power of the spatial frequency process. Such simulation is performed by composing a figure from square surfaces of different shades of grey (different luminances). It is possible then to manipulate spatial frequencies of different band widths and different dominant frequencies. For example, a crude construction of a face becomes readily recognizable when the high frequencies that determine the edges of the squares are removed, thus softening the transitions between the contrasting grey areas. *Campbell* had such a computer analysis and construction performed on a photograph in order to compare the results to those obtained when only lines or only lines and corners were used to make the reconstruction. The results demonstrate conclusively the advantage of the spatial frequency mechanism in providing detail to the image.

How does the brain manage a spatial frequency analysis? Or, for that matter, a temporal frequency analysis? What is the brain process that can perform the transformations necessary to such an analysis whether it be in the Fourier or some similar domain? Neurophysiology has until recently been concerned for the most part with the transmission of signals from one part of the nervous system to another. This transmission is effected by nerve impulses travelling along axons. Transmission is interrupted at axon endings where junctions, synapses, with other neurons occur. Transmission across such junctions is facilitated by the secretion of chemicals at axon endings - neurotransmitters that are stored in vesicles at the presynaptic site.

What has been ignored until lately is the fact that interactions of serious magnitude are occurring among junctional events. Both pre- and post-synaptically such interactions block or facilitate conduction of the electrical signal at any particular locus. The interactions can occur because axons branch at their termination and become fibers of small diameter. Post-synaptically, the dendrites leading to the nerve cell body are also fine fibers. When electrical records are made extracellularly from such fine fiber networks of interlacing branches of axons and dendrites, it is found that nerve impulses have decremented into small amplitude slow waves which propagate only short distances, if at all. Because of their low amplitude and sluggishness, slow waves are sensitive to local electrochemical fields whether these be generated by neurotransmitters, by the metabolic activities of glia, the nutrient supporting cells of the brain, or each other. In short, the pre and postsynaptic slow potentials can be viewed as con-

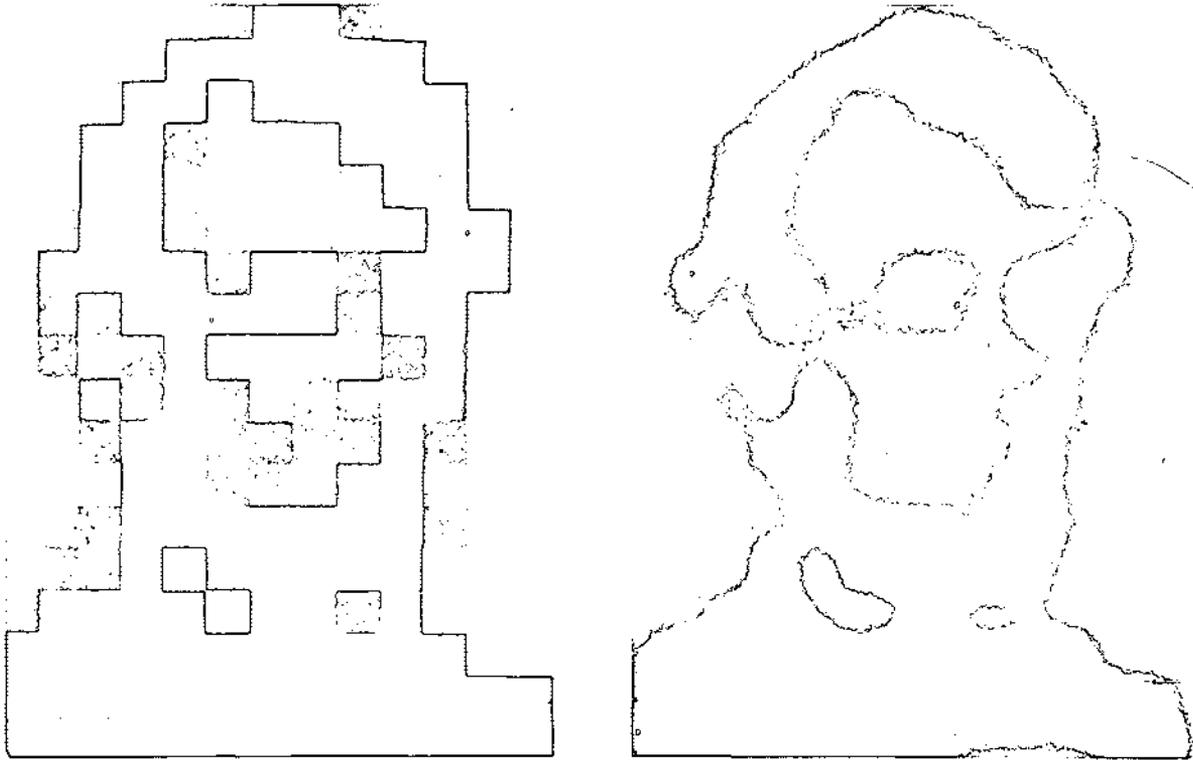


Figure 2: A computer plot using squares representing different luminances. When high frequency components (edges) are removed, the figure becomes a recognizable portrait.



Figure 3: A reconstruction of a picture by computer using lines only (right upper), lines and corner (right lower), and various bands of frequency (the four figures on the left). Note the marked improvement in resolution and detail when reconstruction is by the spatial frequency method.

stituting an interactive microstructure that has the potential for carrying out the computational work of the brain.

There is agreement among neurophysiologists that a large amount of this computational work occurs at the junctions between neurons — at synapses. By analogy with digital computers and because nerve impulses are discrete events, the workings of the brain have been conceived in digital terms. But by virtue of the interactive nature of the slow potential microstructure, the digital view may be misleading. A view more in keeping with the actual situation would take into account the slow wave nature of the microstructure by the hypothesis that the arrivals of nerve impulses creates a slow wave design — a wave front — which becomes transduced by virtue of the action of neurotransmitters into a departure pattern of interacting dendritic slow potentials (and minispikes) — also, therefore, a wave front. This hypothesis would allow the application of wave mechanical mathematics such as *Fourier* analysis and related techniques (e.g., convolutional integrals, *Fresnel* and *Bessel* transforms, etc.) to the study of brain function. The domain of optical information processing would be brought to bear as an important adjunct to the brain's digital programming functions assumed on the basis of integration of information into axonal nerve impulses.

What is the evidence, that in fact, computations by way of a slow potential microstructure do take place? Neuroscientists have come to believe that the most compelling evidence arises from the recent discovery that the computational work of the retina prior to the ganglion cell level is performed exclusively by interactions among slow potentials. No nerve impulses can be recorded from rods or cones, from bipolar or horizontal cells, and only rarely from amacrine cells (*Werblin and Dowling, 1969*). Retinal processes depend on computations performed by a slow potential microstructure. Everything we experience visually is computed by this slow potential microstructure.

The structure of the retina has often been thought to represent a mini-model of the sheetlike portions of the brain such as the cerebral cortex. Microelectrode analysis has supported the view that, at least with regard to the horizontal networks of dendrites (i.e., basal dendrites of the cortex) slow potentials (inhibitory and excitatory postsynaptic potentials) are responsible for the computations reflected in changes of the configurations of receptive fields at progressively more central levels of the visual system (*Benevento, in press*).

An important consequence of these results of investigation on neural organizations in the visual system is a possible explanation of the mechanism by which input becomes distributed in an essentially parallel processing system. That in fact, such distribution occurs has been shown directly — not only indirectly by sparing of functions after brain resection. Electrophysiological recordings have shown that patterns of electrical potentials evoked by visual stimuli, by responses and their consequences (reinforcements) become separately encoded in a more or less random distribution over the extent of the primate visual cortex (*Pribram, Spinelli and Kamback, 1967*) This distribution apparently depends on repetition: when nonsense syllables are presented to one retinal locus only once, they are unrecognized when presented at another retinal locus. When, however, such syllables are presented to the same locus several times, they are readily recognized when presented elsewhere (*Moyer, 1970*).

The data reviewed above, taken together with the demonstrations that the

visual system is sensitive to spatial frequencies, make it plausible to forward the hypothesis that the interactions among slow potentials especially in horizontally arranged dendritic networks, are responsible for the distribution of information within the visual system. In optical information processing systems there are loci, planes where the interactions among wave fronts of various spatial frequencies produce interference patterns and a resultant diffusion of information, i.e., information becomes distributed. When these distributed parts of the system (the interference patterns) are captured in a permanent record (as for instance on a photographic film), they are called holograms. By analogy, therefore, the distributed state of information shown to be characteristic in the brain may be called holographic.

The Holonomic Theory

Holograms provide a powerful mechanism for storing the image construction properties of optical information processing systems. As already noted, what called attention to the distributed information state is that it makes the brain highly resistant to damage. In addition, the holographic state allows a fantastic memory storage capacity: some hundred million bits of retrievable information have been stored in a cubic centimeter of holographic memory. This is accomplished by separately storing modulations of one or another spatial or temporal frequency. It is somewhat as if there were myriads of FM (frequency modulation) radios compressed into a tiny space. The short wave length of light (as compared to sound) makes such capabilities possible. In the brain, the short wave lengths characterizing the slow potential microstructure can be assumed to serve in a similar fashion.

There are other properties (e.g., associative recall, translational, i.e., positional, and size invariance) of holograms that make the analogy with brain function in perception and memory attractive. These have been presented in another paper (Pribram, Nuwer and Baron, 1974). Here I want to emphasize that testable hypotheses can be formulated and models of actual brain function can be proposed within the domain of what can loosely be called the holographic properties of optical information processing systems. We have reviewed the evidence for image construction by the brain. What assemblies of neurons (and their processes), if any, function as true Fourier holograms? Which brain structures function more like *Fresnel* holograms? Which mimic a *Fourier* process by convolving, integrating neighboring neural events and those at successive stages? These questions are being asked and experiments are being performed to provide answers.

As might be expected, such experiments have already encountered one serious obstacle in drawing too close a parallel between optical information processes and image construction by the brain. This obstacle concerns the size of the receptive fields recorded for cells in the primary visual projection systems. For example, the projection from the macular portion of the retina, the foveal receptive fields, is extremely small — some 3–5° of visual angle as a maximum. A hologram of this size will hardly account for the fact that information becomes distributed across the entire visual system as indicated by the evidence from resections and from electrophysiological recordings.

A search has therefore been made for larger receptive fields that integrate the input from the smaller fields of the primary projection cortex. Such larger fields have been found in the cortex that surrounds the primary projection areas. It would be simple if one could assume that there, rather than in the primary projection cortex, the true holographic process takes place.

But this simple assumption runs contrary to other evidence. First, it would not account, by itself, for the distribution of information within the projection cortex. Second, complete resection of this *peri* projection cortex (where the larger receptive fields are found) produces no permanent damage to image construction as far as one can tell from animal experiments (*Pribram, Spinelli and Reitz, 1969*).

Beyond these visual areas of the brain cortex, however, there is another, lying on the inferior surface of the temporal lobe which, when it is resected, leaves monkeys markedly and permanently impaired in their ability to make visual discriminations (*Pribram, 1954, 1960, 1969*). This impairment is limited to the visual mode (*H. Pribram and Barry, 1956; M. Wilson, 1957*). Only visual performances demanding a choice are impaired; other visual functions such as tracking a signal remain intact (*Pribram, Chapter 17, 1971a*). The difficulty involves the ability to selectively attend to visual input (*Gerbrandt et al., 1970; Rothblat and Pribram, 1972; Gross, 1972*).

Much to everyone's surprise, this visual "association" area (as the area with comparable function is known in man (*Milner, 1958*)) appears to function remarkably well when all known visual input to it is destroyed. As already noted, removal of the perivisual cortex has little permanent effect; destruction of the thalamic input (from the pulvinar) to the inferior temporal cortex has no effect whatsoever (*Mishkin, 1972; Ungerleider, personal communication*). Even combined lesions of perivisual and thalamic inputs do not permanently disrupt visual discriminations.

These data make plausible the hypothesis that the inferior temporal cortex exerts its effect on vision via an output to the primary visual projection system (*Pribram, 1958*). Evidence in support of this hypothesis has accrued over the past fifteen years: the configuration and size of visual receptive fields can be altered by electrical stimulation of the inferior temporal cortex (*Spinelli and Pribram, 1967*); recovery cycles in the visual projection system are shortened by such stimulation (*Spinelli and Pribram, 1966*); the pathways from the inferior temporal cortex have been traced (*Whitlock and Nauta, 1956; Reitz and Pribram, 1969*).

Thus, another, more specific hypothesis can be entertained — viz., the suggestion that the inferior temporal cortex helps to program the functions of the primary visual projection systems. Specifically, such programming, as well as programming by input from sensory receptors, could "get together" the distributed store of information from the various loci of restricted receptive field size. If the relevant loci were addressed in unison they would, in fact, function like a hologram.

The difference, therefore, between brain function and the function of optical information processing systems is the one set out at the beginning of this paper. Brain is *both* an image construction and a programming device. Optical systems construct only images.

The thesis presented here, therefore, suggests that the holographic-like store

of distributed information in the primary visual projection system is akin to the distributed memory bank of a computer. The computer's memory is organized more or less randomly; the brain's memory has been stored along holographic principles. Both must be addressed by programs which access the appropriate "bits" of information. The computer does this serially; the brain, to a large extent, simultaneously, by pathways that allow signals to be transmitted in parallel. Such simultaneity in function produces momentary brain states that are akin to the holographic patterns that can be stored on film. Because of these differences between brain and optical systems, it may be better to talk about brain function as holonomic rather than just holographic or hologrammic. The term holonomic is used in engineering whenever the systems, in an interactive set of such systems, are reasonably linear in their function. Linearity allows the computation of the functions of each system and therefore an estimate of the amount of their interaction — the "degrees of freedom" that characterize the interactive set. The interactions are known as the holonomic constraints on the system. In the context of the model of brain function in vision suggested here, the neural systems that determine any momentary visual state would have to be shown to be linear; then the amount of interaction among the systems in producing the holographic visual state would appear as the degrees of freedom characterizing that state.

Evidence is available to show that the visual system, despite local nonlinearities, acts linearly overall above threshold (e.g., *Ratliff*, 1965). This is the case in other neural systems, notably the motor system (*Granit*, 1970). It is thus reasonable to propose that the holonomic model applies to brain functions other than visual. Support for such a proposal comes from work on the auditory (*von Bekesy*, 1960), somatosensory (*von Bekesy*, 1959) and even gustatory (*von Bekesy*, 1967; *Pffaffman*, 1960) and olfactory systems (*Gesteland*, et al, 1968).

Briefly summarizing, the holonomic model of brain function proposes that the brain partakes of both computer and optical information processes. The brain is like a computer in that information is processed in steps by an organized and organizing set of rules. It differs from current computers in that each step is more extended in space — brain has considerably more parallel processing capability than today's computers.

This parallel processing aspect of brain function leads to another difference. The rules of parallel processing are more akin to those that apply to optical information processes than they are to those used in current serial computers. Thus the momentary states set up by the programming activity are considerably like those of image constructing devices, i.e., holographic. Thus memory storage is also holographic rather than random as in today's computers. This does not deny, however, that storage of rules also takes place — as it does in machine peripherals (e.g., DEK tapes for minicomputers). What the model requires is that the "deep structure" of the memory store is holographic.

Since the holographic state is composed by programs and since the distributed store must be got together by the actions of and interactions among programs, the holographic brain state can be analyzed according to the systems that produces it. Thus the holonomic constraints or degrees of freedom that characterize the holographic state can be determined. The holonomic model of brain function is therefore mathematically precise, and its assumptions (such as overall linearity of component programming systems) and consequences (the

distributed nature of the deep structure of the memory store) are, at least in principle, testable.

Is Perception Direct or Constructional?

I want now to address some consequences to psychology (and perhaps to philosophy) of the holonomic theory 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 "holonomic" model 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 holonomic theory is constructional. Images are constructed when input from inferior temporal cortex (or its analogue in other perceptual systems — see *Pribram, 1974a*) activates, organizes the distributed holographic store. Images are produced and are therefore as much as *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 holonomic view. Patients are seen who complain of macropsia 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 girl's skirts stayed up despite the upside-down position!

Further "clinical" evidence in support of the holonomic model 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* in press; 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 holonomic theory 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) deplors 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 which 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 that 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 other 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 ordi-

narily 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, the state of the organism (e.g. his respiratory rate, etc.). 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 or 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 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 prac-

tice 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 by *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*.

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 representation of the percepts go therefore beyond the anatomo-

mical 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 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 (1970)* 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 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 vertical 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 kittens' 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 Bekey (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 the 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 constructional apparatus and more into the apparently "real", consensually validatable external world.

Structure and Probability

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 success 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 wi-

der 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 complimentary 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 the 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 a 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?

The Structural and Holonomic Aspects of Organization

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 assumed 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-quantal 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, which, on the average, however, are subject to the more regular oscillations of the 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 a recent issue of *Scientific American* reviews the contemporary scene in these attempts at a Unified Field Theory in the sub-quantal domain (Weinberg, 1974).

More recently, *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 postsynaptic 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*, in press), I have made a parallel distinction for perceptual processes: following *Bertrand Russel* (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 come 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 among 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,

is holographic. As extrinsic properties become realized, they make the implicate organization become more explicit.

The consequence for this view is a reevaluation 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 renown 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.