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# The Biology of Mind: Neurobehavioral Foundations<sup>1</sup>

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## I. WHAT PSYCHOLOGY CAN BE ABOUT

I have been asked to describe here my systematic approach to problems in psychology. At first this seemed a reasonable request, easily met since my experimental work has progressed in a fairly systematic fashion and my lectures to students of psychology seemed not unduly chaotic. A closer review provoked by an undergraduate student who asked simply "what is

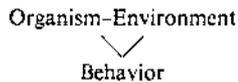
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the basic tenet of your system?," made me realize that my approach was not nearly as rigidly forethought as I had suspected. I had not coolly fashioned a model—on the contrary, much healthy intuition, guesswork, groping, and fortuitous circumstance went into what might be called "my position." In fact I was hard put to come up with any one central tenet. What I can present, however, is a central tenet at each level of inquiry to which my interests are directed. Let me begin at the top, the most general level of scientific inquiry, then illustrate the operations by which I implement this approach, and finally present some of my views of brain function and of the psychological process which have resulted from such implementation.

I believe that psychology must concern itself with the problems of mind, i.e., with the contents and processes which become subjectively experienced, verbally and instrumentally communicated, and validated through social consensus. In this I differ from most of my contemporaries who talk and write about psychology as the science of behavior, i.e., they are behaviorists. Behaviorism is usually concerned with some form of stimulus-response relationship, some sort of correlation between an input to the organism and the output generated by this organism consequent to that input. Sophisticated variants of behaviorism include such formulations as Estes (1959) who describes stimuli as mathematical sets of events whose partitions are the responses of the organism. Skinner (1968) recently has clarified his response-oriented views by stating that he is interested in detailing all the environmental events necessary to produce reinforcement, i.e., the increased probability that a particular response will recur. Note that behaviorists are primarily concerned with correlations among environmental events; both stimuli and *responses* are so conceived, e.g., in an operant situation responses are the marks on the cumulative record which, at the end of an experiment, can be taken home for study and analysis. One learns about the organism indirectly from these environmental relationships. The organism is mediator, a black box containing the intervening variables or constructs which give rise to the relationships.

My own approach from its inception has critically departed from behaviorism (Pribram, 1954a). I treat the organism directly as a class of independent variables to be manipulated, not as an intermediate between environmental happenings. Thus I publish the anatomical reconstructions of brain resections I have made as part of the method of my experiments and not, as is the vogue among behaviorists, as a part of the results section of the report. In such experiments behavior becomes the dependent variable which is used to tease out organism-environment (in my experiments brain-environment) relationships. Behavior is used much as is litmus paper by the biochemist studying the interactions of acids and bases.

The results of experiments made within this mold, i.e.,



can be applied in a variety of ways. They can be used to gain behaviorally relevant knowledge about organismic variables; in my experiments to enhance understanding of the frontal lobes of the brain or of its hippocampus. Or the results can be made party to a further analysis of the behaviorally relevant environmental variables and thus be used as would the behaviorist to study the problem of what constitutes reinforcement. But, in addition, the results can be applied to the formulation of the behaviorally relevant organism-environment *relationship*, and I feel free to apply mental language to describe the terms of such a relationship. In this way an objective, operationally sound study of mind becomes possible.

## II. MINDING BRAIN FACTS

I do not for a moment underestimate the power which behaviorism has brought to the study of psychological processes. I have exercised seriously in behavioristic research—for a decade I managed a neurobehavioral laboratory devoted to the analysis of brain function by the use of operant conditioning techniques. During this period Lashley was wont to note that every good psychologist must go through a behavioristic phase—Lashley's own experience in behaviorism came when he worked intensively with Watson. But, in time, his data and his interests forced Lashley to abandon behaviorism and he once assured me affectionately that I too would some day "grow up." Lashley (1952) never was able to formulate how psychology was to accomplish this growing up but he did indicate, as he so often did, the directions that must be taken. His most explicit statement on this subject was destined to be his last:

Today I shall discuss a subject which, like our Oedipus complexes, has probably troubled many of us but has been suppressed, especially in scientific meetings. Suppression, is a sign of conflict, and I hope that I may be able to contribute something to lessen the tension. I refer to the problem of how the brain knows that it knows; what characteristics of neural activity constitute mind. The pioneers in neurology were not troubled by this problem. They accepted the metaphysics of their day, which regarded the brain only as the agent of mind. Mind was for them a little man, seated in the head, who did all the thinking and willed all the actions for the brain. The brain was really only an impediment to him since, by the doctrine of survival, he could get along even better without it. (No one seems to have noted that this concept involved an infinite regression, like puppy dogs and little fleas.) When Fritsch and Hitzig reported the excitability of the cortex, they readily interpreted the excitable areas as "the place of entry of single psychic functions into

material." And students of cerebral localization, even when they did not subscribe to mind-brain interaction, were content to ascribe mental functions to specific areas, without inquiring how the areas carry out the functions. Even today this mixing of the mental and physical retards analysis of the actual functions of specialized parts of the nervous system.

In 1881 Bubnoff and Heidenhain wrote, "It seems to us absolutely necessary that investigations of the physiology of the brain be kept as distinct as possible from the accompanying psychological processes." Pavlov, who studied for a time with Heidenhain and was much influenced by him, developed this attitude still further in his attempt to construct a complete account of behavior in terms of conditioned reflexes without reference to mental phenomena. Bechterev, who anticipated Pavlov in the formulation of behavior in terms of associative reflexes, accepted a psychophysical parallelism. Pavlov was less explicit but came to regard mental phenomena as of no concern to the physiologist; a fit subject only for psychologists and philosophers, whom he held in slight esteem. The behaviorist school in America has carried this conception to its logical conclusion, not only denying that mental phenomena are relevant in the study of behavior, but asserting that they do not provide a basis for any scientific study whatever. Their position, however, still leaves them with the problem of how man ever developed the delusion that he is conscious.

Students of neurology might well be content to leave the problem in such hands, although it is certain that no solution will come from those sources. Mind, for psychologists since Watson, has become a naughty word. Metaphysicians and theologians have spent so many years weaving fairy tales about it that they have come to believe one another's phantasies.

There are indications, however, of an increasing interest in the problem of mind among neurologists. I would not intrude the question here, in what I am sure will otherwise be a serious scientific discussion, save that, within the past few years, three leaders in neurology, specialists in different fields, have asserted that mind cannot be explained by the activities of the brain and have sought to reseal the little man on his throne in the pineal gland.

Sherrington, after demonstrating that mind is not a special form of energy, wrote:

The sun's energy is part of the closed energy cycle. What leverage can it have on the mind? Yet through my retina and brain, it seems able to act on my mind. The theoretically impossible happens. In mine, I assert that it does act on my mind. Conversely my thinking "self" thinks that it can bend my arm. Physics tells me that my arm cannot be bent without disturbing the sun. My mind then does not bend my arm. Or, the theoretically impossible happens. Let me prefer to think that the theoretically impossible does happen.

Eccles accepts Sherrington's conclusion that the mind is not a form of energy. Then evolves an elaborate theory as to how non-energy mind can act on matter, appealing to telepathy as supporting evidence. He accepts Eddington's misrepresentation of Heisenberg's principle of uncertainty and makes elaborate calculations to show that a minute "influence," within the limits which Eddington sets to the uncertainty principle can act upon a synaptic junction and modify behavior. As Heisenberg himself has told me, the principle of uncertainty is entirely irrelevant to the question of causal determination. It is a principle of unobservability, and as a basis for doctrines of will it is in a class with the belief that the invisible face of the moon is made of green cheese. Also, I still consider the gambling house odds more reliable than Rhine's statistics.

Waishe bases his argument for reviving the soul chiefly upon the assertion that man is more wonderful and more dignified than the carwig. I cannot quote the carwig but can quote Archy, the cockroach, in reply (Don Marquis). "A man thinks he amounts to a great deal but to a mosquito he is only something good to eat."

I am not ready to accept these doctrines of scientific despair and Christian hope. They

are based upon a thorough misconception of the facts of consciousness. They fail to analyze the problem and show no conception of what phenomena are to be explained, or cannot be explained, by the action of the brain. The problem requires an entirely different approach; a thorough analysis of the phenomena of consciousness, oriented with reference to the phenomena of neural activity. Only when such an analysis has been made, will it be possible to test the correlation of mental states and processes with the brain's activity. I am confident that when the questions which are now held to be unanswerable are properly formulated, they will turn out to be capable of translation into physiological terms and will fall within the competence of present methods of physiological research.

As Lashley notes, the class of organismic independent variables which has most to do with organizing the psychological process is brain. Thus, my career has been devoted to the study of brain mechanisms. But I believe Lashley leaves unsaid a most important aspect of the problem—that brain research without the proper environmental-behavioral analysis will lead nowhere. There are those, mostly physiologists, who feel strongly that we will come to an understanding of mind when we have researched the physiology of the brain completely. My view is that a considerable number of brain facts may be irrelevant to an understanding of mind and that an understanding of brain alone will not give rise to an understanding of mind. A good analogy to use here is provided by present-day computers. Complete knowledge of their hardware, their machinery, does not completely specify their power. Only when software, the multitude of ways the machinery can be programmed to behave, is taken into account can we know what these devices are capable of.

At the most general level, therefore, my tenet is that we gain access to mind through the use of both brain and environmental-behavioral analyses. Since the latter include observations of verbal as well as of instrumental behavior, the question is raised as to the relation between consciousness and other psychological states. What of those determinants of behavior which do not have easy access to awareness? There is, of course, the likelihood that awareness accompanies only some and not all states of the neural apparatus. Kamiya and his students (1968) have shown that by using ordinary operant techniques awareness of certain neural states can be learned. Whether all central states are subject to such training remains to be seen. But there is another set of neural happenings which are not readily accessible to awareness. These are the neural processes which compose the *structure* of mind. Again an example: The grammatical rules by which we speak and write are not readily accessible to awareness, but linguistic analysis has shown, not only the possibility of coming to grips with the structure of language but the importance of doing so. The study of mind does not exclude unconscious determinants.

Today the surge both of cognitive and of physiological psychology has made some aspects of my position reasonably respectable with each group

although neither has as yet accepted, to any considerable extent, the researches performed and conclusions reached by the other. But the situation has not always been even this favorable.

As a practicing neurosurgeon I attended my first American Psychological Association convention twenty years ago. The occasion was the presentation of a paper prepared jointly with Blum and Semmes, both graduate students in psychology at the time. The experiment to be reported was an important one. It related to the then-popular therapeutic procedure of frontal lobotomy (or leukotomy). This surgical operation, though producing marked changes in the personality of patients, was said to leave intellect intact. Hebb had just published his well-received study which showed no detectable deterioration in IQ, etc., after bilateral frontal lobe resection in man (Hebb, 1945; Hebb & Penfield, 1940).

Working with Lashley, we were interested in preparing animal "models" of clinical neurological conditions so that we could examine the brain-behavior relationship experimentally. Monkeys had brains ideally suited for the purpose. However, removals of monkey frontal lobes produced a characteristic problem-solving difficulty: tests of short-term memory such as delayed response and alternation had been shown by Jacobsen (1936), Nissen (Jacobsen & Nissen, 1937), Malmö (1942), Finan (1942), and a few others to be selectively impaired by the frontal lesions—in monkey.

Thus a major discrepancy stood in the way of letting us make the rhesus monkey our experimental model. We turned to the chimpanzee for help. And indeed we found that the ape could perform the delay task but did so by giving himself external reminders such as tapping the side of the cage during the delay interval.

I found also that human lobotomy patients could perform the delay tasks—the delay could even stretch through the surgical procedure—provided they coded the task verbally. Many years later, I was to show that a major problem-solving deficit does, in fact, result from lobotomy when the verbal mode of solution is precluded (Poppen, Pribram & Robinson, 1965; Pribram, Ahumada, Hartog, & Roos, 1964).

But twenty years ago we came to the American Psychological Association eagerly to present our chimpanzee data. It was our good fortune to have Jacobsen in the chair for our session—so we introduced ourselves en masse—i.e., all three of us. Jacobsen was moderately interested in the results of our experiments but had to excuse himself because of a committee meeting. This posed the dilemma which is the point of this anecdote. What should we do—three authors of an important study on the frontal lobes of the brain, a vital subject both in the clinic and in the laboratory? Our Chairman had deserted us, and with his departure went our audience. *For no one else came to the brain function session and ours was the only paper scheduled.*

It was the heyday of behaviorism. The Division of Physiological and Comparative Psychology had been dissolved and absorbed into the Division of Experimental Psychology. The organism had become a black box. The search for the engram in the brain had shown that no mechanism existed whereby learning and remembering could possibly occur (Lashley, 1950).

Twenty years later—now—the situation has changed. There is an audience, estimated to be well above the 500 mark for each of the major papers sponsored by the division. The division has been reconstituted, and knowledge about brain function is accumulating rapidly. Every paper presented tells us of the functions of some nucleus in the brain, of some part of the cortex, of some autonomic neural mechanism, of this or that endocrine substance. Physiological psychologists have become, as they should, hard scientists. But they have taken this to mean that they should devote themselves exclusively to learning more and more about less and less. And so they have not changed the image of psychology. Members in good standing in this division herald, as did their forebears twenty years ago, their texts by the unfortunate title "Psychology, The Study of Behavior."

So again I must declare: psychology is and has been, except during the excesses of the behaviorist revolution, the study of mind. We who deal with brain should be the first to realize that the study of behavior and the study of brain complement each other: we do not just use behavior to determine brain function; nor do we work with brain just to find out how we behave. It is the psychological process, mind, we wish to understand. My proposal is that the time is ripe for an objective study of mind and that the study of brain function coupled with that of behavior must lead the way to this more mature psychology.

### III. THE EXPERIMENTAL ANALYSIS OF MIND

Presented in such global terms, this proposal sounds heretical and visionary. But in practical fact a growing number of researchers are already engaged in shaping psychology to this image. Let me illustrate: could the strict behaviorist legitimately study the process of attention? He could not, because he limited himself to studying observing responses, and attention is not observable in behavior. But we, the brain people, can objectively measure what is going on inside the organism. The pioneering work of Lindsley in this area of research is familiar (Haider, Spong, & Lindsley, 1964). Let me here provide an example from my own laboratory. We have been pursuing the functions of the inferior part of the temporal lobes of primates. This part of the brain is usually called an "association" area but it is better termed "intrinsic" since its functions are specifically visual and not in any

sense associative (Pribram, 1960a). Our problem has been that we could not trace any direct input from the visual system to this part of the brain. In fact, neither radical disconnections of the cortico-cortical (Chow, 1951; Pribram, Spinelli, & Reitz, 1969), nor complete destructions of the thalamocortical pathways (Chow, 1954) result in the visual deficit obtained when the inferior temporal gyrus is ablated. I therefore suggested that perhaps the influence of this part of the cortex works downstream, via a pathway leading from the brain and terminating in the visual system (Pribram, 1958) (see Fig. 1). Thus the functions in vision of the so-called association cortex would be ascribable to the control exerted over the visual mechanism.

A number of experimental results have supported this hypothesis.

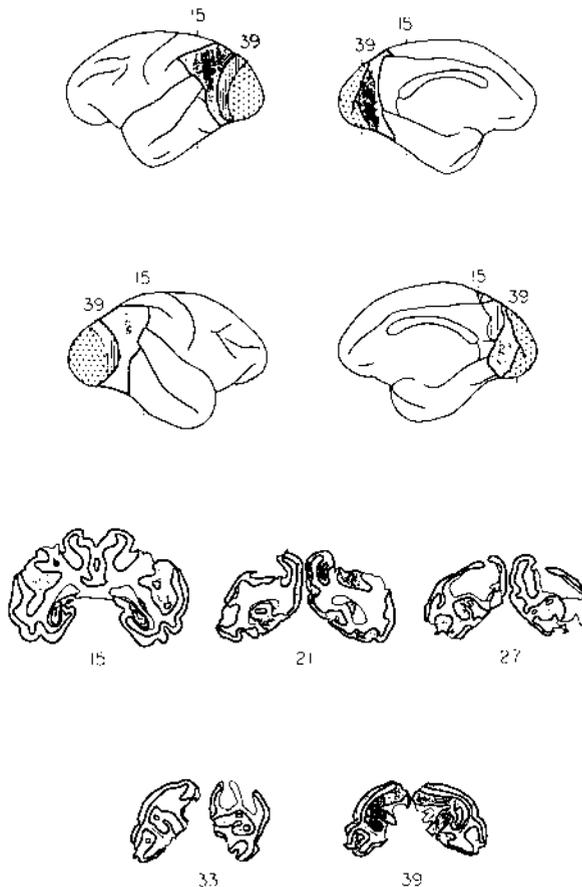


FIG. 1. Reconstruction of prestriate lesion in S 283.

Nauta and Whitlock anatomically demonstrated the efferent pathways from the inferior temporal gyrus (Nauta & Whitlock, 1954). Using electrophysiological techniques, we have confirmed these results and extended them to show that these effects of electrical stimulation of the cortex extend far into the periphery of the visual system. About 8–10% of the fibers in the optic nerve of cats have been shown by electrophysiological techniques to be efferent to the retina (Spinelli & Weingarten, 1966). Changes in the shape and extent of the visual receptive field of units in the optic nerve are produced when these efferent fibers are centrally activated (Spinelli & Pribram, 1966, 1967). (See Fig. 2.)

But what has all this to do with "attention?" One of the experiments we performed showed that we could change the "excitability" of the visual mechanism by electrical stimulation of the inferior temporal cortex (Spinelli & Pribram, 1966) (see Fig. 3). This result was especially welcome since it gave us a clue as to the neurological mechanisms by which the inferior temporal cortex exerts its control over vision. We thus wanted to explore further this mechanism. Successive groups of graduate and postdoctoral students came to the laboratory fired with enthusiasm by our finding. But alas, as so often occurs when a really new result is obtained, we could not replicate. At least not consistently enough to begin a study in depth. Yet the problem was sufficiently important to warrant persistence. We needed a more stable indicator of excitability in the visual system so we abandoned, for the moment, the paired flash paradigm. Bypassing the retina, stimulating electrically within the system itself, should provide the stability we needed. It might also bypass the lability necessary to obtain the effect we were interested in, but we had to take this chance. And indeed, at first, electrical stimulation of the inferior temporal cortex failed to have an effect.

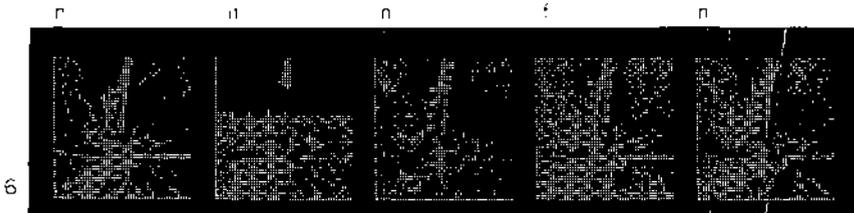


FIG. 2. Effects of stimulation of the posterior "association" cortex of a cat on a visual receptive field recorded from a neural unit in the optic tract. (These records are made by moving a spot with an X-Y plotter controlled by a small general purpose computer, PDP-8, which also records the number of impulses emitted by the unit at every location of the spot. The record shown is a section parallel to and 2 SD above the background firing level of the unit. Note the dramatic change in the configuration of the receptive field, especially after stimulation of the posterior "association" cortex, IT, inferotemporal.)

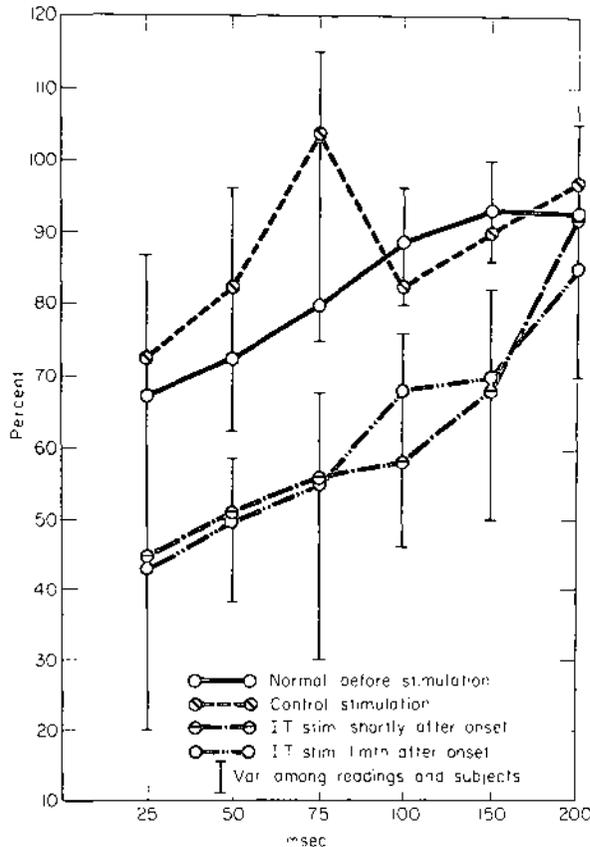


FIG. 3. A plot of the recovery functions obtained in five monkeys before and during chronic cortical stimulation.

Persistence and ingenuity were not to be permanently denied a harvest, however. Gerbrandt, a postdoctoral fellow, solved the dilemma with a simple observation. He showed that the amplitude of the responses evoked by electrical probe stimulations within the visual system was a function of the attentiveness of the monkey during the experiment. When the monkey was enclosed in a box, the response evoked was small. When the box was opened and the monkey was looking around, the response evoked was large. Further, inferior temporal cortex stimulation could make the small response obtained in the closed box into a large response, but had no influence on the large response. Finally, using the size of this probe-evoked response as a monitor, he could predict in the closed-box situation whether inferior temporal cortex stimulation would or would not affect the recovery function of the visual system (Gerbrandt, Spinelli, & Pribram, 1970). (See Fig. 4.)

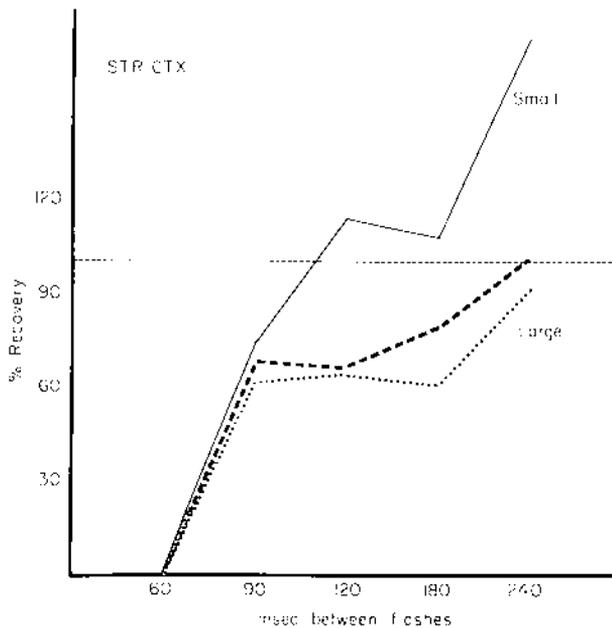


FIG. 4. The response evoked in the striate cortex to the second of a pair of flashes is compared with the response to the first flash. Solid line indicates the recovery function when the flash response pair is preceded by a probe response which is small. Dotted line indicates the recovery function when the flash pair is preceded by a probe response which is large. The dashed line indicates the paired-flash recovery function when no probe is given. A probe response is produced in the striate cortex by electrical stimulation of the lateral geniculate nucleus.

Thus whenever the monkey was attentive, the effects we had earlier obtained were not observed. When, however, the monkey became "bored," tended to nod into sleep, etc., the effect on the recovery function was clear-cut. In our initial experiments we had daily performed a long routine of procedures: paired flashes, paired clicks, click-flash and flash-click combinations, patterned flashes, etc., were presented in regular order, day in, day out, week in, week out. Not only the monkeys, but Spinelli and I, who were performing the experiments, became disenchanted with the routine. One of us was delegated to keep watch on the other two to see to it that sleep would not intervene. The monkey was watched through a peephole and when he nodded the enclosure was tapped gently. A small displacement of the stool of the nodding investigator accomplished the same end. In short, we got our results because the monkeys were not attentive. Subsequent teams testing monkeys only on the recovery cycle phenomenon, working with monkeys fresh to the situation and apparently interested in

the goings-on of the experiment, obtained different results. Only when, through repetition, the situation became boring to us and the monkey, did the recovery-cycle effects again emerge.

This is not all there is to the neural mechanism of attention. These experiments deal more with the vigilance aspect of the attentive process than with selective or focal attention. We are now engaged in exploring these dimensions. Mackworth has developed a superb instrument to study visual observing behavior (Mackworth, 1968). Using eyeball photography, it is possible to measure the sequence of visual fixations on a stimulus pattern. Monkeys with removal of the inferior temporal gyrus show disturbances of their fixation durations and sequences. But we are still in the process of analyzing results and so cannot report the full meaning of the observed disturbances.

I have presented in detail the explorations of attention for a purpose. It is my view that once there is sufficient specification of *the environmental, the organismic, and the behavioral* variables that go into the description of a mental term, the term loses its mystical aura, and stigma, and becomes scientifically respectable. Few of you raise an eyebrow when the term visual field or its derivative, visual receptive field, is used. The definition of visual field is "that part of the environment responded to by an organism using one eye without moving that eye." The receptive field of a neural unit is defined in a like fashion—in fact, we often talk colloquially about the field "seen" by the cell. These terms are acceptable because we know so much about the physical and the sensory neural events that make up vision. In short: the mental terms "vision," "to see," "to look," are objectively respectable.

The term "expectancy" is going through a similar scientific legitimization by physiologically oriented experimentalists. The work of Sokolov (1960) has shown that behavioral habituation was not a simple fatigue-phenomenon but the construction of, as he calls it, a neuronal model against which input must be matched. The work of John, Zubin, and Sutton (Sutton, Tueting, Zubin, & John, 1967) and that of Lindsley (Haider *et al.*, 1964) has shown that the components of responses evoked by visual stimuli vary with the expectations of organisms (including man). The work of Walter (1964) and of Lacey (Lacey, 1969; Lacey & Lacey, 1958) has shown that a wave of negative electrical potential sweeps the brain from front to back as organisms (including man) prepare to perform a task. This last observation is perhaps more relevant to the process of intention than to that of expectancy, but "intention" has as yet not been sufficiently investigated to attain objective respectability. Within a few years I am sure this will have been remedied and clear-cut neurological as well as behavioral distinctions will make it possible to talk objectively about both expectancy and intention.

"Voluntary" is another such term. Clinical neurologists have never

given up its use. But the defining operations, behavioral and neurological, have never been adequate to the subtleties demanded by the full meaning of the process. Now, however, beginnings are being made. MacKay (1966) and Mittlestaedt (1968) talk of feed-forward mechanisms and Teuber (1960) searches for corollary discharges to account for the differences between the results perceived when movement is passive and when it is voluntary.

Obviously we are at the beginning of an era during which a biologically based, objective study of mind will redress the extreme provincialism produced in experimental psychology by the behaviorist revolution.

#### IV. A DIFFERENCE THAT MAKES A DIFFERENCE

Most physiologically oriented psychologists and brain scientists are first and foremost experimentalists exploring the universe with the tools and techniques they have labored to forge. They are not all that seriously concerned whether someone wishes to call a particular performance a voluntary action or a piece of operant behavior—*unless it makes a difference to their explorations*.

I believe, seriously and strongly, that it *does* make a difference which language is used (and by this I don't mean just the words in that language) to describe one's interests and the results of pursuing those interests. Again let me turn to an example from research in my laboratory to illustrate how this difference comes about.

Some years ago we showed that the effects of temporal lobectomy on changes in temperament and personality resulted from the removal of the amygdala, one of the limbic system structures contained within the temporal lobe (Pribram & Bagshaw, 1953; Pribram, 1954). Further analysis showed that these limbic formations were involved in a variety of behaviors labeled as the four F's, an extension of Cannon's "fight and flight" label for sympathetic neural function (Pribram, 1960b). Our four F's included, in addition to Cannon's, feeding and sexual behavior. The close anatomical linkage between the limbic and hypothalamic structures made this result a reasonable one. The problem arose when I became dissatisfied with just a descriptive correlation between brain anatomy and behavior and tried to understand the mechanism of operation of this relationship.

Had I been satisfied to pursue behavior per se I should have next asked, as others have, whether different parts of the amygdala served feeding, fighting, fleeing, and sexual behavior. Just for the record, a negative answer appears to have been given when experiments (whether ablation or stimulation) have been addressed to this question. But what I wanted to know had to do with the psychological process, the commonality, that characterized

the four F's, so that a single lesion (even of a somewhat complex anatomical formation) could alter, at one stroke all of these diverse behaviors. The concept, "instinct" (Beach, 1955) though plausible, failed to satisfy for a number of reasons as did a variety of forms of the concept "drive," which would have been a natural because of the strong connections between amygdala and hypothalamic mechanisms. As will be noted later, such drive concepts have also failed to account for the effects of hypothalamic damage and stimulation. I therefore decided to take an opposite approach to the problem and ask whether behaviors which in no apparent way were innately based or drive controlled, would be affected by amygdalectomy.

The experiments performed therefore went far afield from the proverbial four F's. In collaboration with Schwartzbaum (Schwartzbaum & Pribram, 1960), with Bagshaw (Bagshaw & Pribram, 1965) and with Hearst (Hearst & Pribram, 1964a, b), transfer of training experiments were undertaken. In one procedure transposition behavior was studied, in the other the reaction to stimulus equivalences. Stimulus generalization was analyzed as a control measure. The tasks were chosen because they seemed to us reasonably remote from hypothalamic influence.

Amygdalectomy affected performance in both transposition experiments but not in those testing stimulus generalization. My conclusion was therefore that the amygdala, at least, influences processes other than those ordinarily ascribed to the hypothalamus.

A clue to what this process might be came from an observation made while testing the monkeys on the transposition task. The amygdalectomized subjects neither transposed nor did they choose the absolute cue. Instead they treated the test trials as a completely novel situation, performing initially at chance (Douglas, 1966; Schwartzbaum & Pribram, 1960).

Pursuing this observation, Bagshaw and her collaborators in my laboratory (Bagshaw & Benzie, 1968; Bagshaw & Coppock, 1968; Bagshaw, Kimble, & Pribram, 1965; Bagshaw & J. Pribram, 1968, Kimble, Bagshaw, & Pribram, 1965), showed that amygdalectomy did indeed alter monkeys' reactions to novelty. Behavioral (and some components of EEG) habituation to novelty were markedly prolonged. On the other hand, the viscerio-autonomic indicators (GSR, changes in heart and respiratory rates) of orienting to novelty were wiped out by the lesions (without impairing the response mechanisms *per se*). These results led me to suggest that orienting to novelty proceeds through two hypothetical stages. The first, characterized by behavioral orienting reactions, "samples," scans the novelty. The second, characterized by viscerio-autonomic reactions, leads to the "registration" of experiencing the novelty and so to its habituation (Pribram, 1969, a, b). Without such registration the temporal organization of behavior cannot occur, thus fighting, fleeing, feeding, and sexual behavior, as well as delayed

alternation behavior (Pribram, Lim, Poppen, & Bagshaw, 1966), etc., become impaired.

Thus a much greater span of data regarding amygdala function are encompassed. And this is not all. Reference to the psychological process of registration helped explain an, until then, inexplicable observation I made many years ago (Miller, Galanter, & Pribram, 1960, Ch. 14). A patient on whom a bilateral amygdectomy had been performed a year earlier had gained much weight. She seemed to present a golden opportunity to find out directly what she experienced to make her eat so much. Her answer was always that she experienced little—she did *not* feel, i.e., register that she was inordinately hungry. Such a lack of registration is a commonplace in clinical epileptic seizures originating from abnormalities around the amygdala, abnormalities which also produce the famous *deja vu* and *jamais vu* phenomena.

As noted earlier, the type of analysis of limbic system function of the amygdala which I have presented here does not stand alone. For hypothalamic function a similar route has been forced on physiologically oriented psychologists. The paradoxical finding of Miller, Bailey, and Stevenson (1950)—that rats with ventromedial lesions will eat more but work less for food—remained unexplainable in drive, i.e., behavioristic terms. The impasse remained until some sense was made of the data by Teitlebaum (1955) and by Grossman (1966) who invoked mental concepts such as “finickiness” and “affect.” Immediately new meaningful experiments and new analyses were generated.

Again I have dwelt on detail because the terms used by the experimentalists working in the field are mental ones. To give them objective as well as subjective substance takes a great deal of careful observation, experimentation, and scientific analysis, both at the environmental-behavioral and at the neurobehavioral level. Neither level by itself has the explanatory power nor serves as well the generation of meaningful experiments. Thus the challenge to psychologists today, it seems to me, stems from the very fact that the study of brain added to the study of behavior appears to make psychology whole again.

The fascinating problems which behaviorism had to exclude not only *can*, but *must*, be tackled when neurobehavioral techniques are brought into play.

## V. OF INTERDISCIPLINARY ENDEAVORS

Laboratory experiences such as these have led me to try to formulate the process by which my brand of scientific inquiry proceeds. This is difficult to do thoroughly without altering the process and thus chancing damage to a

successful operation. Some formulation has been possible, however. First, my experiments are conducted as interdisciplinary efforts. Any single discipline centers on a technique such as the use of microelectrodes, of operant behavior, of mathematics, or of paper chromatography. As long as only one technique is used, the results can be codified only in descriptive terms. When several techniques are brought to bear on a problem, the resultant interdisciplinary data allow the structure of the problem to be explored more fully: the processes and mechanisms involved can be approached. Further, the data obtained in *intradisciplinary* programs tend to generate technique-oriented variations on those data. What was found on the mouse is looked for in the rat; the description of the visual receptive field of a unit in the optic nerve leads to an experiment detailing the visual receptive field at the lateral geniculate level of the visual system. By contrast, *interdisciplinary* programs tend to be problem oriented: the process of pattern perception is studied by simulation on computers, by the use of microelectrode recordings in cats, in monkeys and in man, by a variety of behavioral techniques and by a combination of any or all of these if possible. Since the problems are never completely solved, new technology is continuously recruited and older methods abandoned. Thus interdisciplinary programs are never seen by outsiders as very original: the problems were posed centuries ago and have been reframed in ever more precise form since; the techniques are usually borrowed from technologies which, as a whole, are far more advanced than those aspects adapted to the program. What intrigues those of us inside the interdisciplinary endeavor is the enduring nature of the issues, the fact that they are fundamental and that any progress in sharpening our views of them will be equally enduring and fundamental.

Second, and related, my brand of scientific inquiry makes abundant use of analogy and metaphor. I am not afraid to view the brain as a computer nor the cortex as a hologram. The attributes of the mechanical artifacts are more accessible to manipulation than is the biological organism and so we can sharpen issues more quickly and then test them more precisely in the biological world. The biochemist performs many such *in vitro* (in glass) experiments before relating them to what is going on *in vivo*. Note however, that I said view the brain as a computer, I did not say the brain is a computer. I do not for a moment think the brain works just as does a present-day computer. But some parts do, and some aspects of brain functioning and computer functioning are remarkably similar. It is our job, and fun to boot, to find out just where the similarities and differences lie.

Finally, my brand of scientific inquiry is systematic. It aims at comprehending large issues not trivial ones; at the interrelations among many sets of variables, not just a few. I want a view of the brain and of the psychological process, not just a theory of the functions of the reticular formation or

of fixed-interval behavior. This may seem grandiose but my experience is quite the contrary. As Ashby notes (1960) the job of any experimenter is to produce results; as he calls it, to obtain straight-line behavior. In multi-variate systems such as all brain and behavior experiments are, straight-line behavior can be managed in a variety of ways. The experimenter may be fooled into thinking that the way he has done it is relevant to the biological universe. He therefore is tempted to generalize his limited but highly predictable result into a more meaningful context. The behavior of pigeons in an operant situation too readily becomes a theory for understanding language. The reactions of rats to 24-hour food deprivation and to shock too easily become gradients of approach and avoidance explaining conflicts experienced by man. When one takes the larger view from the beginning, the irrelevancies spewed as data by our technology can relatively quickly come to be seen as such.

A penalty entailed by approaching one's subject matter from a wider scope is that focus on any particular is attained more slowly. This is, of course, a virtue as well as a penalty. But even the whole takes shape reluctantly and keeps changing contours. Contrary to the image many of my colleagues currently hold of me, only recently, and after much laboratory experimentation and laborious analysis of the results obtained, have I been able to formulate in words some reasonably satisfactory statements of my own views.

## VI. THE LANGUAGE OF THE BRAIN

Many of these statements are contained in a forthcoming book entitled *The Language of the Brain* (Pribram, in press). In this book, the brain is considered to be a device which codes the information provided to it, and then recodes and recodes and recodes it. In short, the brain is conceived as a producer of languages.

The coding operations take place at various levels. At the most basic, a solution must be sought for the fact that the brain must both process and store information. This double task is accomplished not so much by virtue of neurons, the anatomical units of the nervous system which relay signals from one location to another, but by a microstructure, a set of patterns, organized of the events which occur at the junctions among neurons. These junctional microstructures (constituted of patterns of postsynaptic electrical potentials) (Stevens, 1966) are in the first instance more or less temporary but steady neuroelectric *states* with which inputs from sensory receptors must interact. Second, they provide the matrix in which more permanent chemical and histological residues of experience, memory traces, can develop.

These basic operations are combined into logic elements which form the

building blocks of the nervous system. Interactions among junctional patterns of activity compose the logic. These interactions are of two sorts, excitatory and inhibitory. It is the inhibitory interactions which give form to the logic.

Inhibitory interactions can be classified according to the functions they perform. One class of inhibition enhances contrast among the elements making up the pattern. Contrast is enhanced because each neuron inhibits its neighbors, thus emphasizing the neuroelectric differences between the fields of excited and nonexcited neighboring neurons. As detailed earlier in this paper, this contrast-enhancing mechanism is assumed the basis for reactions to novelty (orienting responses) and therefore to what is to constitute information for further processing.

The other class of inhibition acts on the excited neuron itself, damping its activity over time. This self-inhibition takes place because of the negative feedbacks present everywhere in the central nervous system. The ubiquity of such negative feedback has made it necessary to view the organization of reflexes not as stimulus-response chains or reflex arcs, but as servomechanisms, thermostat-like devices, controlling behavior via changes in biases, the tuning of receptors to accept or reject as information the signals introducing perturbation—a process somewhat like adjusting the wheel of a thermostat to indicate the temperature to which one wants to set the heating system. Inhibition among neighboring neurons which enhances contrast is critically involved in the setting of the servo; the mechanism of self-inhibition, of feedback, is basic to its stability of operation. The biasable servoprocess, or as it is often called, the homeostat, is thus conceived as the basic logic element of the nervous system.

Logic elements must in turn be combined if they are to be effectively functioning structures. In the brain, one way in which logic elements appear to be composed is into "screens" which process signals in stations all along the sensory and motor systems. As already noted, the neuroelectric events (the configurations of postsynaptic potentials) which are generated at neural junctions, enhanced and given stability by neural inhibitory interactions, form a screen in which a microstructure of interfering wavefronts develops. The effects of these neuroelectric events can be stored as changes in conformation of macromolecules such as proteins along synaptic and dendritic networks, and thus influence subsequent synaptic and postsynaptic activity. By way of analogy with the powerful use of interference effects in the field of optical information processing, the microstructures derived from interference effects are called neural holograms (Pribram, 1966). Just as in the optical hologram, the assumption is made that when appropriately activated by either a reference mechanism or by an input similar to that which originally composed it, perceived "images" are produced from neural holograms.

Images are of different kinds. Perceptual images are "images-of-events." The images involved in feelings are of a somewhat different nature. The distinction between perceptions and feelings, between our knowledge of the world-out-there and the world-within, is based both on differences in the organization of the neural mechanisms controlling the receptors and on what it is they receive. The images involved in feelings turn out to be "monitor-images," monitoring hungers and thirsts, the variety of moods, as well as the motivational and emotional feelings of interest, tranquility, or upset.

The core parts of the brain stem, including the hypothalamic regions and reticular formations contain the receptors which monitor the chemicals which compose the stimuli for the world-within. The limbic formations of the forebrain are, as detailed earlier, the neural systems critical to the continuing, i.e., temporal organization of the monitoring process. Though this neuro-behavioral relationship is reasonably well established, we remain almost totally ignorant of the details of the mechanism by which monitor-images might be constructed.

A third type of image is involved when the organism acts. One of the major puzzles in the study of behavior has been that though behavior is effected by the use of moving muscles, a great variety of patterns of movement may in actuality be used to accomplish any particular behavioral result. How can consistent action develop in the face of the organism's variability? How can a nest be built of sticks and stones, or shreds of leaves or newspapers, with beak or claw or both? How can the selfsame manuscript be produced in handwriting, type, or as a tape recording? A step toward an answer is given when it is recognized that the brain may generate what I have called "images-of-achievement." The fact is that most movement is controlled not by any direct excitation or inhibition of muscles but by a change in the bias or tuning of the *receptors* attached to muscles (the muscle spindles, etc.) which inform the central nervous system of the forces acting on the muscle. Thus, even the generation of behavior, the control of movement, is effected by the tuning of muscle receptors, and thus turns out to be primarily the problem of managing receptors and only secondarily that of controlling effectors (Held, 1968).

In fact, microelectrode recordings from the motor cortex indicate that the critical stimulus dimensions of which images-of-achievement become constructed are the forces which impinge on the receptors of the muscles carrying out the task (Evarts, 1967). The consistency of actions is due therefore to consistencies in the field of environmental forces necessary to accomplish the act. These consistencies are processed to make up the image-of-achievement.

From what has been already stated it is clear that the screens from which images are constructed must be continually modified by experience. How this

occurs constitutes the set of problems usually handled under the rubric "reinforcement." Reinforcement is conceived as a mechanism by which the consequences of behavior interact with the organism's already developed competences (the logic elements composing screens) to produce new ones. Perception, feeling, and action all undergo such modifications by experience. The neural mechanisms involved follow a course similar to that described in embryogenesis by the process of induction, and the possibility is posed that reinforcers induce neural changes (by way of RNA) which lead to growth of connections in the brain much as growth of a structure (e.g., an optic cup) is induced (e.g., by RNA) in the appropriate site of an embryo (Pribram, 1966).

The process of reinforcement entails the interactions between consequences and competences within an image-mode; interactions between image-modes also occur. The primary interimage interactions are between images-of-achievement and the other two types. Thus, images-of-achievement interact with images-of-events in the construction of "signs" and with monitor-images in the construction of "symbols." Signs are produced by making discriminative choices, actions on events to classify them. Signs are attributive. Symbols are generated when acts are monitored and thus made relevant to the world of feelings. Symbols are arbitrarily assigned on the basis of usefulness. Much of my own brain-behavior research with monkeys has been occupied with specification of the neural processes involved in sign and symbolic behavior. The so-called association cortex of the brain is especially involved in accomplishing this mix among images. However, as we saw earlier in this chapter, the mix is not performed within this cortex per se but by controlling and tuning the electrical events occurring in the more primary sensory and motor systems.

At the highest level of neurobehavioral organization are thought and talk. Here the neurological evidence is harder to come by since it must come almost exclusively from clinical studies of man. These, however, can be evaluated as part of the systematic approach developed here and some realistic interpretations can be made. Thought is viewed as the processing of symbols into signs; the logical operations of choice and classification are brought to bear on symbolic content. Talk is the obverse; language makes arbitrary symbolic use of signs and thus provides flexibility to the linguistic process. Thought and talk build on one another, each new level constructed of the complexity of the language achieved by the previous level.

The major requirement which must be met to bring these operations to fruition is reversibility in coding, i.e., the transformations involved in processing one code into another must allow the reverse transformation to occur without undue distortion. This can be done by keeping track of the transformations employed, as is done in the cultural use of language or more easily

in the central nervous system by using a transformation which when used again will generate a reasonable replica of the original. The type of transformations which involve the holographic process of image construction have this replicating characteristic. This is thus another powerful attribute of the holographic hypothesis of brain function.

## VII. THE EMERGENCE OF A BIOLOGICAL RATIONALISM

These then are the powers of the brain. Being the recoding device that it is, generating languages to describe the signals it begets and receives, man's brain solves the philosophical bind of describing itself. This is accomplished by applying over and over again in rapid juxtaposition that old and ubiquitous biological trick of replicating its essential structure in different form. Biology is thus the key to the brain's power and as such it is the key to understanding the psychological process.

I have already shown that a biologically based objective study of mind can cope with such concepts as attention, relevance, expectancy, intention, volition, finickiness, and affect. The challenge continues. We must take the step which goes beyond even that radical empiricism which provides much insight into the nature of experience. In the mind-brain-behavior area of investigation, empiricism, applied to its pragmatic purpose with the usual scientific controls, generates a new dimension in the understanding of psychological processes—a new biological rationalism. This, as I see it, is the departure which the work and workers interested in the biology of mind can bring to psychology. The message before us is clear, its voice compelling: are we willing to think as psychologists and not just as physiologists and behaviorists and will we speak out unashamedly so as to make the intellectual community listen?

For more is at stake than the direction taken by scientific psychology. The mind-brain-behavior relationship is, as it has always been, an important focus of how man views himself. An outmoded neurology based on a horizontal analysis of brain function once proclaimed that primitive spinal and brain stem functions come in evolutionary history progressively under the control of higher, i.e., upstairs, cortical mechanisms. Though in part correct, more recent vertical analysis of the nervous system such as those detailed earlier in this chapter and also in my article in Koch's, *The Study of a Science* (Pribram, 1962), have shown that each neural structure has its primitive components overlaid by new accretions which may alter its system properties considerably. The old and inadequate data led to a popularly held view of man, initiated by psychoanalysis and today promulgated by such lay "spokesmen for science" as Ardrey (1966) and Koestler (1967): the view that man

behaves as beast because at base he is beast. Cortical control expressed as language and culture is, according to this view, a veneer which is either thin and easily and dangerously cracked, or else so thick that it constrains the inner man, shackling self-expression and communication with his fellow men.

By contrast, today's neurological data find no unaltered primitives, no beast within the shell. Phylogenetic comparisons show neurological systems enriched and altered, not superceded. It is a fallacy, for instance to think of dogs as macrosmatic and man as microsomatic. True, the dog's olfactory brain is sizable and he can track odors we barely discern. But man's olfactory brain is made larger yet by developments not present in subhuman mammals and by virtue of these developments man can appreciate the culinary artistry of a Tour D'Argent which is well beyond the ken of any dog.

Aggression is a topic currently so important to us that it must be seriously looked at in this light. Much is being said these days of the territorial imperative and much of what is said rings true. However, what is fiendish about man is not that he shares with other mammals the fact of territorial needs, i.e., the need to be himself, to isolate a part of his universe from the unbounded complexities that assail him. What makes man fiendish is his rational capacity to formulate and codify his territorial claims conceptually and so to proclaim them religiously right. It is the rational in man—the new neurology, not the primitive—which gives rise to his problems. Man's wars are not bestial. As has been pointed out repeatedly, *intraspecies* annihilation is a rarity in nature. *Man's wars are rational* and any hope we may have of staying war is through understanding man's rationality and not his bestiality. At present we tend to equate the rational with equable reasonableness. Biological studies of mind show it to be otherwise. The neural mechanisms serving motivation and emotion are those, which when they become more differentiated, become the substrate of rational action (Pribram, 1967; Pribram, 1970; Pribram & Melges, 1969). When action is blocked, rationality becomes fierce with emotion and in defense of its motives. The rational becomes dysrational in its fierceness, especially when it becomes institutionalized and its various aspects can be distributed among a number of individuals. Only by recognizing this fierce dysrationality for what it can do to us will we be able to come to grips with it, and therein lies our hope.

And so I have come full circle. A behaviorist looking at aggression sees agonistic behavior and studies its presumed environmental antecedents and consequences. The physiologist looks for a neurochemical substrate he hopes he can correlate with anger. The biologically oriented student of mind also does these things but he is clearly aware that agonistic behavior is merely one expression of anger and that there may be a variety of nonangry *reasons* for a display of agonistic behavior. The total mental process (anger and/or reason) to be studied must be approached through an analysis of brain

function, of observations of relevant environmental (in this case cultural) determinants and of behavior.

So far, what research undertaken in this spirit has taught me is that the brain is the unique instrument which through the coding and recoding of information produces languages and through languages the culture by which we live. Thus it is man's linguistic rationality that provides both the culprit and the hope, both rigid reason for enduring and flexible reasonableness for creating.

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