T-20

Reprinted from SCIENTIFIC PSYCHOLOGY
Edited by Benjamin B. Wolman

23 KARL H. PRIBRAM

PROPOSAL FOR A STRUCTURAL PRAGMATISM: SOME NEUROPSYCHOLOGICAL CONSIDERATIONS OF PROBLEMS IN PHILOSOPHY *

I HAD COMPLETED this chapter only to be plagued by a vague dissatisfaction. I had in hand four loosely connected sections, each in its own right the subject of an essay. Why this odd juxtaposition of memory, induction, mind-brain, and ethics—and why in this order? It finally occurred to me that my dissatisfaction stemmed not so much from the disconnectedness of the various sections, but from a feeling that a connection was there but that it remained unexpressed. Once this became evident, it was but a short distance to the identification of the connection.

I had been trying through these paragraphs and pages to discern my own posture toward certain problems in scientific philosophy. The consistencies and contrasts in this posture quickly crystallized—I was clearly a structural pragmatist.

How had this come about? To a physiologist, the choice is open as to whether he will gain understanding of the functions of the organ he is studying by (1) pursuing the reductive course to learn more and more about ever smaller units and processes, or (2) directing his efforts in a more holistic setting, to the examination of the relations between organ and organism. In choosing the holistic path, as a neurologist I was faced with

* If we are to take seriously the results of the Würzburg experiments, a *ms* such as this is as much due to those who pose the problems as to the author. I hope therefore that my uncle, Karl Pribram (*Conflicting Patterns of Thought*, Public Affairs Press 1949), Mrs. Elisabeth Wadleigh (with her perceptive editor's eye), George Miller (whose sharp distinction between normative and descriptive science "made" section III for me), Merton Gill (whose patient and continuing tutelage can be seen in section IV), and David Hamburg (who contributed in so many ways) will consent to share credit and responsibility for whatever has been accomplished.

the need for some sophistication in the experimental analysis of systematic variations made on behavior which it is the supposed office of the nervous system to control. As a neuro-behaviorist, I found much of use in the positivist, operationist approach. Pseudoproblems were uncovered (Pribram, 1954, 1958; Malis, Pribram, & Kruger, 1953; Pribram, Kruger, Robinson, & Berman, 1955-1956). Distinction between the levels of discourse that refer to the neural and the behavioral systems sharpened the way in which questions could be put in the laboratory and how the results of experiments could be communicated. I probably should have remained happy with some sort of neuro-behaviorism, were it not for a particular experience.

A patient had been given a bilateral removal of the anteromedial tip of the temporal lobe. In monkeys (and other mammals) such a surgical procedure is followed by a syndrome which includes excessive oral investigation of edible and nonedible objects and a marked increase in food intake (Pribram & Bagshaw, 1953). We had puzzled considerably and carried out many experiments to try to assess the factors responsible for the appearance of this disturbance. Had the operation impaired the sense of smell? The sense of taste? Had an increase in metabolism been effected? Or was it a decrease in the sensitivity of the satiety mechanism?

Here was an opportunity to go to the heart of the matter with a simple procedure. Ask the patient. She had been observed to put nonedible objects in her mouth and to eat excessively; she had gained over 100 pounds in weight. So, we inquired of her: are you excessively (or moderately) hungry (most of the time; right now; when shown food—candy, meat, etc.)? Always, the same answer: "Not especially; no more so than before surgery; not so I noticed it, Doctor."

Quite accidentally, an inquiry of this sort took place just before lunch one day. The door of the examining room opened onto the ward dining hall where other patients were already seated around a large table. Our woman took in this scene with a glance and made a beeline for the food-laden table, pushed others out of the way, and began to stuff herself, using both hands.

Immediately recalled to the examining room, her answers to our questions were as before. Further, when shown a piece of chocolate (which she was not allowed to grab), she gave no reaction such as "I'd love to have that," "I'm so hungry," or "That looks good." Rather, the piece was closely and intently scrutinized, described in detail and not mentioned again once put out of sight.

This patient was mentally ill, though communicative and cooperative. One would in any case be cautious in drawing conclusions from a single observation. But something stood out: there was more than an ordinary discrepancy of fact here. As a behaviorist, I should place my faith in the observed excessive eating behavior of my patient. I could actually weigh the results of her altered behavior. Verbal reports of introspections are notoriously untrustworthy—why should they be less so here? The evidence stood overwhelmingly in favor of the reliability and validity of the instrumental response—other patients had shown the same disturbances; so had two varieties of monkeys; we had produced a similar picture in dogs (Fuller, Rosvold, & Pribram, 1957); the literature showed the effect to be true of cats; and besides, this part of the forebrain is heavily connected with that region in the hypothalamus where injury classically leads to overeating. What, then, is the problem? Simply that I had believed the woman when she told me that she felt no hunger. Further, it makes a difference whether one ignores the verbal report or whether one comes to terms with it. And since it makes a difference, one cannot choose to ignore.

What is this difference? Primarily, the decision determines whether a body of evidence is shut out or admitted to inquiry. Such reports as those of Penfield (1958), where correlations are made between excitation of brain cortex and verbal reports of introspections, are inadmissable as evidence if the verbal report, with all its recognized limitations, is not good for *something*. On the other hand, if verbal report is indeed to be listened to, such nefarious borderline activities as medical psychoanalysis must, after all, fall within the province of the scientific study of behavior.

Since it did make a difference, I could no longer in good conscience ignore what patients told me—and the neuro-behaviorist was forced to become the neuropsychologist. Analysis of the woes and responsibility of neuropsychology—and its promise—are brought out elsewhere (Pribram, 1962a). And some of the consequences of this subjective-behavioristic approach to the problems of psychology have also been detailed, in collaboration with two equally troubled authors (Miller, Galanter, & Pribram, 1960). My stance as a systems neurophysiologist, neuropsychologist, and subjective behaviorist thus assured, I apparently had the temerity to accept the current assignment.

Why pragmatism? Pragmatism has, for many, come to mean, ". . . first, a method; and second, a genetic theory of what is meant by truth" (James, 1931, p. 65). Seen as a compromise between the tough-minded empiricist and the tender-minded rationalist, pragmatism has maintained the tough spirit in its methods and the tender heart in its aims.

The essence of the pragmatic method has been summarized by James:

> There can *be* no difference anywhere that doesn't *make* a difference elsewhere —no difference in abstract truth that doesn't express itself in a difference in concrete fact and in conduct consequent upon that fact, imposed on somebody, somehow, somewhere, somewhen (p. 50).

In method, then, pragmatism is a radical empiricism:

> Pragmatism represents a perfectly familiar attitude in philosophy, the empiricist attitude, but it represents it, as it seems to me, both in a more radical and in a less objectionable form than it has ever yet assumed. A pragmatist

> turns his back resolutely and once for all upon a lot of inveterate habits dear to professional philosophers. He turns away from abstraction and insufficiency, from verbal solutions, from bad a priori reasons, from fixed principles, closed systems, and pretended absolutes and origins. He turns towards concreteness and adequacy, towards facts, towards action and towards power. That means the empiricist temper regnant and the rationalist temper sincerely given up. It means the open air and possibilities of nature, as against dogma, artificiality, and the pretense of finality in truth.
>
> At the same time it does not stand for any special results. It is a method only. But the general triumph of that method would mean an enormous change in . . . the "temperament" of philosophy. Teachers of the ultra-rationalistic type would be frozen out, as the ultramontane type of priest is frozen out in protestant lands. Science and metaphysics would come much nearer together, would in fact work absolutely hand in hand (p. 51).

Why not pursue this line of reasoning, as has been done by others, to what would be its obvious conclusion: an operational logical positivism? The reason for hesitancy in adopting this course lies with the first and second sections of the body of this chapter. In a sense, these sections provide an ultraradical empiricism: data about the data forming process; facts about fact-making; the manner and form of the etchings on the *tabula rasa*. But a new quality is added: the empiricism takes on a nativistic flavor; hallowed distinctions give way to neurons. Both biology and philosophy gain. In a recent discussion of Milner and Olds' discovery that organisms will seek self-stimulation when electrodes are implanted in certain parts of the brain, I stated:

> No longer can we say simply, "here is a pleasure center, here is a pain center in the brain," for stimulation of one and the same spot may produce behavior quite different depending upon the situation in which the organism finds itself. The arguments of the philosophers are taken out of the realm of the speculative and into the laboratory. The arguments remain the same, but now tissue is involved and the behavior of organisms studied. This new solidity has a two-fold effect. First, it shows that the arguments of the philosophers were not just "hot air," and secondly, it shows that the naïve materialism which has served the biologist so well thus far must be amplified, if not totally discarded, if his data are to make any sense to him or to anyone else (Pribram, 1959a, p. 5).

The step taken when brain tissue becomes involved in these issues has the feel about it that one has when going from a metalanguage to an object language. The techniques of logical analysis remain the same, but an additional dimension has been added. It is a matter of levels of discourse, of validity and of structure; and this is the substance of the third section.

In psychology, the operational approach has led to descriptive learning theory; to mathematical models of the psychological process; to postulates of variables and constructs that might intervene between observables to account for their lawful interrelation. But descriptive behavior theory has shown its weakness in definitions which are of necessity so narrowly circular that they lack meaning, that is, platforms for hypotheses or significant experiments; the mathematical modelers find themselves in the awkward opposite position that the very stimulus elements that compose their models defy even the loosest sort of definition; and intervening variable theorists admit their inadequacies when they grant that their variables really aspire to become constructs which in due time will become respectable, that is, physiological. This is not to say that these approaches have failed to contribute—it is only that they have stopped short. Many of their arguments ring hollow once it is recognized that the head is not.

Since there is this marked difference between the research on learning and memory generated by positivistic operationism and that generated by questions posed through a neuropsychological approach, the two cannot be identical. Both continue to be effectively pursued; what remains to be stated is the conviction that the pragmatic test, "Is there a difference that makes a difference," has proved a method more generally applicable than that which characterizes either biological science per se, or operationism per se.

But in spirit, pragmatism has never been merely a radical empiricism. The Jamesian pragmatist holds that knowledge is not just etched on a *tabula rasa*—knowledge is continuously *made* by a sentient being who acts on his universe. This genetic theory of what is meant by knowledge is the second characteristic of pragmatism:

> This new idea is then adopted as the true one. It preserves the older stock of truths with a minimum of modification, stretching them just enough to make them admit the novelty, but conceiving that in ways as familiar as the case leaves possible. An *outrée* explanation, violating all our preconceptions, would never pass for a true account of a novelty. We should scratch round industriously till we found something less eccentric. The most violent revolutions in an individual's beliefs leave most of his old order standing. Time and space, cause and effect, nature and history, and one's own biography remain untouched. *New truth is always a go-between, a smoother-over of transitions. It marries old opinion to new fact so as ever to show a minimum of jolt, a maximum of continuity.* We hold a theory true just in proportion to its success in solving this "problem of maxima and minima." But success in solving this problem is eminently a matter of approximation. We say this theory solves it on the whole more satisfactorily than that theory; but that means more satisfactorily to ourselves, and individuals will emphasize their points of satisfaction differently. To a certain degree, therefore, everything here is plastic (James, 1931, pp. 60-61) [italics mine].

Further, "theories thus become instruments, not answers to enigmas, in which we can rest. We don't lie back on them, we move forward, and on occasion make nature over again by their aid. Pragmatism unstiffens all our theories, limbers them up and sets each one at work" (James, 1931, p. 53).

How would the author of these remarks receive the results of the experiments on the orienting response, habituation and novelty? I am sure he would agree that at this juncture any differences between his statements and those made by neuropsychologists (and even some of those of Carnap on credence) would prove to be no differences at all. On this score, then, the label pragmatism is at least as good as any other; and the richness of the similarities between James' statement of the genetic theory of the meaning of truth and the evidence for the neurophysiological process now makes up for whatever lack of precision characterized the earlier formulation.

Moreover, the congruence of at least three different approaches—the introspective, the logical, and the neurological—must be attended. If it truly makes no difference whether the operations that define induction are made in the verbal, the mathematical, or the laboratory mode, how does this come about? What then is the essence of this congruence? The philosopher of science has met this circumstance before.

> It is sufficient to say that what physics ultimately finds in the atom, or indeed in any other entity studied by physical methods, is *the structure of a set of operations*. We can describe a structure without specifying the materials used; thus the operations that compose the structure can remain unknown. Individually each operation might be anything; it is the way they interlock that concerns us (Eddington, 1959, p. 262).

Inductive inference is, according to this analysis, a structure. And our pragmatism has taken a step.

That is why a *structural* pragmatism. The issue of structure is, of course, implicit in the examination of the mind-brain problem viewed as the relation between psychological and neurological science. This issue becomes explicit when the organization of behavior by the neural process is the subject of inquiry (Hebb, 1949), and comes to a focus in the problem of serially ordered behavior (Miller, Galanter, & Pribram, 1960). Whether or not the particular analysis presented in the third and fourth sections proves acceptable, the power of structure as a tool toward understanding is keenly felt. Pluralism is given form; monism loses its monolithic shapelessness; the reasons for a dual (mirror) appearance of the universe become evident. Infinite complexity can be approximated as a scientific idea; we are not stuck with just infinite chaos.

And this difference makes a difference. In the fourth section, the difference appears in the ethics of the classical *versus* that of the structural pragmatism. Classical pragmatism holds that:

> *"The true," to put it very briefly, is only the expedient in the way of our thinking, just as "the right" is only the expedient in the way of our behaving.*

> Expedient in almost any fashion; and expedient in the long run and on the whole of course; for what meets expediently all the experience in sight won't necessarily meet all further experiences equally satisfactorily (James, 1931, p. 222).

Really, Prof. James, isn't this a hell of a fix for an ethics, and especially a pragmatic ethics which must draw its strength from its own impact, to get itself into? Small wonder American education and foreign policy, in their intuitive and often surprisingly effective pragmatism, have had so few explicit guideposts on which to base decision.

Structural pragmatism accepts the classical pragmatic method and the genetic view of the meaning of "truth" and "right." But in recognizing "structure," additional strength, that is, orderliness, is provided. Once structure is admitted, "truth" and "right," reliably established within the system in which they are formulated, become valid to the extent that they transcend that particular universe of discourse. In another connection, I used this example:

> Recently there has been, in North America, a shift in popular connotation away from attitudinal determinants [of the meaning of words]: e.g. the term "honesty" no longer refers exclusively to "telling the truth," "respecting others' property" and such, but also to "behaving according to *how* one 'feels' and 'sees' the situation," even if this entails occasional lying or stealing (Riesman, Glazer, & Denny, 1950) (Pribram, 1959b, p. 284).

Each connotative meaning is true in its own right; each connotation right in its own truth. Confusion (and there is confusion; ask any perceptive and thoughtful teen-ager) results from (1) a failure to make explicit the distinction between the levels of discourse over which each meaning of truth and right holds; and (2) the assertion that one meaning, therefore one level of discourse, has a monopoly on the "really true and right." The structural pragmatist takes the difficult but eminently practical course that truth and right must be reliably and thoroughly established at each level separately and that only then, and in each instance that the issue is met, validity is attained through actions that mesh these levels.

For example, our courts have on occasion sanctioned a theft when this has occurred as an isolated act performed by a distraught parent who could find no other way to feed his children. Stealing has not become universally "good" as a result; nor has the more usual prohibition of robbery made it "right" to starve one's brood. The ordinary action that meshes these two levels of "goodness" and "rightness" is to work for pay which is spent for food. Through "work" and "money" a solid social structure is meshed. Both meanings of "good" and "right" have attained validity (have maintained their usefulness) through the operations "work" and "money." When these operations fail, others are invoked to maintain the mesh: the parent is declared to have been temporarily irresponsible for his actions due to duress. And we proceed to a more enduring engagement between the needs of the unemployed family and the society in which it functions: we act to remove the occasions for duress by appropriate social welfare legislation.

James is so close. The sentence following the passage quoted above reads: "Experience, as we know, has ways of *boiling over,* and making us correct our present formulas" (James, 1931, p. 222).

Neuropsychological researches and theories have recently provided several instances of such boiling over. As so often happens, the implications of this activity have gone unrecognized, since neurophysiologists and physiological psychologists work for the most part outside the main body of philosophic endeavor. Despite this, the neuropsychological effort has considerably aided in establishing man's altered view of his universe and even of himself as an ethical and moral entity. To make explicit the nature and extent of this effect is the job of philosophers. But an indication of the loci of "boil" and a preliminary analysis of their reach can be accomplished by the neuropsychologist.

ON THE NEUROBIOLOGY OF MEMORY STORAGE AND RETRIEVAL

> Where or how does the brain store its memories? That is the great mystery. How can learning persist unreproduced, being affected by other learning while it waits? On the proper occasion what was learned reappears somewhat modified. Where was it in the meantime? The Gestalt psychologists speak of traces which may be altered before they are reproduced. The psychoanalysts speak of the unconscious or the foreconscious where the ideas await call in what Herbart described as a "state of tendency." *The physiology of memory has been so baffling a problem that most psychologists in facing it have gone positivistic, being content with hypothesized intervening variables or with empty correlations* (Boring, 1950, p. 670) [italics mine].

Of most immediate interest here are the rapid advances occurring in identification of a variety of material processes subsumed under the rubric "memory mechanisms." Techniques are now available to examine in detail the substrate that allows time binding, that is, an organism's capacity and infirmity to react to "experienced" events that cannot be identified in his momentary surround. As always when experimental techniques are brought to bear, there is a sharpening of the questions asked which in itself produces consequences in theory and practice.

First, an orientation. Evidence has accumulated during the past century and a half to support the proposition that the brain is the major organ controlling complex behavior and, in man, its immediate antecedents—thoughts and feelings. Much of this evidence has come from the clinic: injuries to brain substance are followed by behavior disturbances; electrical excitation of man's brain effects reports of alterations in introspections and of ongoing actions. In the laboratory, these techniques were refined to give further precision by extending the analysis of the brain's role in the organization of instrumental, nonverbal, complex behavior. But for the most part, the experimental laboratory was concerned with the electrical output of neural tissue. And the electrical events that could be studied were largely evanescent, transient phenomena. Examination of the more enduring structural changes assumed to underlie the memory mechanism could not be touched by these techniques. As a result of all this work, however, when neurochemical knowhow did offer an entrée into the problem, there was not much doubt as to where to focus attention: the brain (with its accessory organs) is the prime candidate for study.

Brain tissue can be divided into two types of components: neural and glial. Many nerves are characterized by the great physical length of their processes (in man, some are as long as three feet) and by the fact that electrical, chemical, and mechanical excitations are quickly propagated along this great length (in a matter of milliseconds). This striking characteristic preempted the attention of neurophysiologists for some time. Recently other more slowly changing attributes of neural tissue have been examined, and at the same time the affinities between neural and glial tissue have been explored. Changes having a time course of an hour or a week are now described, and even more permanent quantitative effects are selectively related to the conditions experienced by the organism:

I have elsewhere collated some of this recent work on the memory storage mechanism (Pribram, in press), and other views (Briggs & Kitto, 1962; Gaito, 1961) and reviews (Bach, 1962; Deutsch, 1962; Pribram, 1961) on the matter are available. Essentially, two major paths are presently discernible in this research.

(1) Most of the impetus for sustained interest has come from the experimental results that implicate ribonucleic acid (RNA) metabolism in neural-glial activity. Neurons have been shown to secrete RNA when active as a result of electrical excitation or physiological stimulation (Hydèn, 1961; Morrell, 1961). In fact, nerve cells have a vastly greater capacity to contain and to produce nucleic acids and proteins than do other cells in the body, so that this characteristic of nerve tissue is as conspicuous as is its ability to generate and transmit electrical potential changes (Hydèn, 1961). The well known role of the RNA molecule, together with its more stable sister substance, DNA, in the mechanisms of genetic "memory," stimulated the suggestion that RNA is somehow involved in the mechanisms of neural memory. Further, when the time course of minute chemical events is followed into the period after specific excitation has ceased, reciprocal changes in RNA concentration are observed to occur between neuron and enveloping glia (Hydèn, 1961).

Hydèn gently teases apart the neurons and the glia of the vestibular nucleus. He finds that the increased production of RNA in nerve cells concomitant with their excitation is coupled to a simultaneous decrease in RNA concentration in oligodendroglia. During this period of excitation, glia could provide the nerve cell with energy-rich compounds since the glia apparently resort, at least in part, to anaerobic metabolic routes such as glycolysis and lipid breakdown. In addition, however, Hydèn finds that after excitation ceases, the glia in turn increase their RNA production while that of the adjacent neurons diminishes. On the basis of other experiments, Hydèn suggests that the aerobic-anaerobic balance is maintained through competition for inorganic phosphates (the Pasteur effect), with the respiratory phase of the process dominant over the fermentative glucose degradation, and the phases in the neuron dominant over those in the glia. This phase lock-in mechanism is assumed to operate through pinocytosis. There is ample evidence of possible pinocytosis from high resolution analyses of the structural arrangements of the glial-neural border. In addition, pinocytosis has been observed in glia and nerve cell tissue cultures, where it can be induced by electrical stimulation.

Why this fuss about a glial-neural couplet? There are several reasons. For one, glial cells reproduce, while neurons do not. Should the memory storage mechanism turn out to be related to protein synthesis guided by RNA production, such stored protein could be replicated by glial cell division.

Second, nerve cells must remain constantly ready for new excitation. The time course of the effects of excitation is short, even when nerve nets rather than neurons per se are considered. In simulated nets, the difficulty has been to adjust the time an element "remembers" in such a fashion that "learning" can take place. Either the net remembers everything too much and so very quickly ceases to be sensitive to new inputs, or else, in the process of retaining sensitivity, so little is remembered that learning can hardly have been said to occur. This difficulty can be overcome in simulated "memistors" by adding a longer time-course storage device which sets a bias on the reception of new inputs and is in turn itself altered by those inputs (Widrow & Hoff, 1960). The glia could function in this fashion. Even their electrical responsivity is some thousandfold longer in duration than that recorded as impulsive activity from neurons. There is every reason to suppose that such graded electrical activity would influence the transmitted excitations of the adjacent neural net, which in turn, through the phase-lock-in biochemical mechanism, could alter the state of the glia.

Is there any evidence to support directly these notions? Most persuasive are the as-yet meager results of histological and histochemical analyses of neural tissue obtained from animals raised under conditions of sensory deprivation. Weiskrantz (1958) has shown that in the retinas of dark-reared kittens, Mueller fibers are scarce—and Mueller fibers are glia. Brattgård (1952), Liberman (1962), and Rasch, Swift, Riesen and Chow (1961) have all shown deficiencies in RNA production of the retinal ganglion cells in such dark-reared subjects.

Meanwhile, experiments by a group of "worm-runners" have added fuel to the RNA fire. Flat worms (planaria) were trained by McConnell, Jacobson and Kimble (1959) in a water filled trough illuminated from above. The animals were placed in the trough until they showed no reaction to the turning on and off of the light. Then each illumination of 3 seconds' duration was accompanied for the last second by an electric shock passed through the water. Initially the worms contracted and turned only when the shock was on; gradually, the frequency of such responses increased during the first 2 seconds, when only illumination was presented. Once a worm had reached criterion it was immediately cut in half transversely, the halves isolated and allowed to regenerate. About a month later, when regeneration was complete, all subjects were retained to the original criterion; whereas original training averaged 134 trials, subsequent to transverse sectioning the original head ends averaged 40 and the original tail ends, 43.2 trials. (A trained but uncut group showed about the same amount of savings; a group trained after the cut took more trials than did the original group's initial training, thus a sensitization effect was ruled out.)

On the basis of these and other similar results, McConnell and his collaborators suggest that whatever the physiological change responsible for this memory process, it must occur throughout the worm's body. Corning and John (1961) tested the hypothesis that RNA may somehow be involved in this mechanism. They immersed the halves of the trained worms in a weak solution of ribonuclease in order to destroy the RNA. The heads regenerated in ribonuclease showed savings as great as control heads; on the other hand, tails regenerated in ribonuclease showed no such savings. The brain-stored memory mechanism was apparently resistant to this exposure to ribonuclease, whereas the somatically mediated "worm memory" was destroyed.

So much for evidence that RNA is somehow involved in the memory storage mechanism. The suggestion is essentially that neural activity results in the rearrangement of the sequence of monomers on the nucleic acid molecule; or at least that a more or less permanent change takes place in the concentration of one or another of the specifically identifiable types of RNA. There is some reason to suspect that nucleic acids are insufficiently rich in alternatives and that their modification proceeds too slowly to handle all that is needed in the way of event storage—for example, to account for the results of experiments on the recall of tachistoscopically presented data. Yet it can be argued that the change in nucleic acids underlies the formation of polystable proteins, and that changes in these are then responsible for the changes in subsequent neuronal activity, and thus behavior.

(2) An independent, though also chemical, approach stems from the classical view of memory as the result of change in resistance to synaptic conduction. Here attention is focused on a neural transmitter substance, acetylcholine. Variations in concentration of acetylcholinesterase activity (presumably related to the amount of production of acetylcholine) are found related to learning by rats (Krech, Rosenzweig, & Bennett, 1956). Specifically, acetylcholinesterase activity has been found to correlate with genetic strain selected for "maze dull" and "maze bright" performance (Krech, Rosenzweig, & Bennett, 1959). Further, acetylcholinesterase activity is related to amount of varied experience available during early postnatal development. Finally, evidence has been presented that those parts of the brain known to serve a specific sensory mode through which experience is channeled, selectively show this increase in acetylcholinesterase activity.

Closely related to this series of studies are some recent results obtained in the neurohistological laboratory. Though the brain's nerve cells do not divide, they can grow new branches. This has been dramatically demonstrated (Rose, Malis, & Baker, 1961) in a study of the effects on brain of high energy radiations produced by a cyclotron. Minute, sharply demarcated laminar destructions (often limited to a single cell layer, and this not necessarily the most superficial one) were produced in rabbit cerebral cortex when high energy beams were stopped short by the soft tissue. The course of destruction and restitution was studied histologically. Intact nerve cells were seen to send branches into the injured area; these branches became progressively more organized until, from all that could be observed through a microscope or measured electrically, the tissue had been repaired.

The organization of the branches of nerve cells could well be guided by the glia that pervasively surround these branches. Such directive influences are known to be essential in the regeneration of peripheral nerves. Schwann cells, close relatives of glia, form a column into which the budding fibers must grow if they are not to get tangled in a matted mess of their own making.

The assumption could well be entertained that glial cell division is somehow spurred by those same activities recounted above as important to memory storage. The resulting pattern of the glial bed would form the matrix into which nerve cell fiber growth occurs. Thus guided, fiber growth is directed by its own excitation—the whole mechanism based, however, on the long-lasting intervention of glia. This dual mechanism for memory storage—RNA and synaptic facilitation—would account for the interfering effects obtained after the administration of electroconvulsive shock (Brady, 1951; Duncan, 1949; Madsen & McGaugh, 1961; Pearlman, Sharpless, & Jarvik, 1961; Poschel, 1957) and in the occurrence of spontaneous "restitution" as well: the growing nerve cell fiber is ameboid and can temporarily retract its tip, which is made up of a helical winding of small globular protein molecules. After the convulsive insult is over, first tentative, then more vigorous probings can again be resumed in some "random-walk" fashion by the nerve fiber tip, as has been suggested for normal growth by Von Foerster (1948). The glial substrate, assumed undamaged, will perform its guiding function to effect the apparent restitution.

These are but some of the data accumulating in neurobiological and neuropsychological laboratories. Work is proceeding on lasting changes produced on nerve membranes by activity (Robertson, 1961; Sjöstrand, 1960); on changes in relation between facilitation and inhibition as a result of continuous activity in small looped networks (Curtis & Eccles, 1960; Eccles, 1953; Landahl, McCulloch, & Pitts, 1943; MacKay & McCulloch, 1952; McCulloch, 1957; McCulloch & Pitts, 1943; Von Foerster, 1948; Wall, 1961); in the speeding of consolidation (Abt, Essman, & Jarvik, 1961; Breen & McGaugh, 1961; Madsen & McGaugh, 1961; McGaugh, 1961; McGaugh & Petrinovich, 1959; McGaugh & Thomson, 1962; McGaugh, Thomson, Westbrook, & Hudspeth, 1962; McGaugh, Westbrook, & Burt, 1961; McGaugh, Westbrook, & Thomson, 1962; Pearlman & Jarvik, 1961; Pearlman, Sharpless, & Jarvik, 1961); on delineating the differences between the neural mechanisms involved in learning from those involved in remembering (Kraft, Obrist, & Pribram, 1960; Stamm & Pribram, 1960, 1961; Stamm & Warren, 1961; Weiskrantz, in press).

There is little doubt that the questions asked have become specific: we are beginning to chart this area of ignorance with precision. No longer must we *assume* etchings on a *tabula rasa;* rather, we ask what the specific neurochemical and neurohistological processes involved are. No longer are we concerned whether the memory trace is indeed laid down in the brain; rather, we ask how many kinds of memory traces there are. No longer do we worry whether an act is rooted in an inherited biology or in an unobservable result of mental experience; the effects of experience are recorded in a biological process probably so akin to the instruments of inheritance that the geneticists are among the most actively interested in the memory mechanism.

We, neurological nativists by interest and empirical scientists by method, need no longer live as split personalities in our search for the laws that govern learning. The biological laboratory, having achieved the means for study of the modes through which experience is registered, asks immediately how experience registered might again be known, that is, appropriately retrieved. And, having posed this question, the need is to know more about other processes that determine knowledge.

ON THE NEUROLOGY OF KNOWLEDGE

> I would propose that all forms of effective surprise grow out of combinatorial activity—a placing of things in new perspectives. But it is somehow not simply a taking of known elements and running them together by algorithm into a welter of permutations . . . (p. 20).
>
> One final point about the combinatorial acts that produce effective surprise: they almost always succeed through the exercise of technique (Bruner, 1962, p. 22).

As already indicated, in his role of experimentalist the neuropsychologist at this stage of the development of his science works largely within an empiricistic frame. He makes much use of the inductive method to acquire knowledge, and is therefore apt to tackle the problem of knowledge by an interest in the process of inductive inference. In his own work, he asks simply phrased questions. These are often based on neuroanatomical considerations and/or introspections consensually validated. Greater precision is attained when the questions can be reformulated on the basis of manipulations of the variables that reliably alter some observable response of the neural tissue or of the behavior of the organism. He is wary of what he calls generalizations—and rarely resorts to deductions of any complexity. But the power of the inductive method is hardly questioned. Even though he recalls David Hume's injunctions, the experimentalist is somewhat surprised that the problem of inductive inference is still a thorny one for the philosopher. Further, when the experimentalist hears the philosopher of science solve the "causal" dilemma by invoking the notion of "subjective probability," his interest and concern are indeed piqued:

> It seems to me that the view of almost all writers on induction in the past and including the great majority of contemporary writers, contains one basic mistake. They regard inductive reasoning as an *inference* leading from some known propositions, called the premisses or evidence, to a new proposition, called the conclusion, usually a law or a singular prediction. From this point of view the result of any particular inductive reasoning is the *acceptance* of a new proposition (or its rejection, or its suspension until further evidence is found, as the case may be). This seems to me wrong. On the basis of this view it would be impossible to refute Hume's dictum that there are no rational reasons for induction. . . . I would think instead that inductive reasoning about a proposition should lead, not to acceptance or rejection, but to the assignment of a number to the proposition, viz., its [Credence] value. This difference may perhaps appear slight; in fact, however, it is essential (Carnap, 1962, pp. 316-317).

In this connection, Carnap defines "Credence" in terms of "the nonobservable microstate of [a person's] central nervous system, not his consciousness, let alone his overt behavior" (p. 306). Already alerted, the neuropsychologist leaps at the words "non-observable microstate of the central nervous system." The philosopher has been forced by his logic to contend with the very thing the neuropsychologist is studying. The die is cast. As we will see, it is the neuropsychologist who must add credence to the philosopher's persuasive argument for credence.

Initially, the search for this nonobservable microstate must lead to the exposition of neural events coincident with phenomena heretofore treated as subjective (based on verbally reported introspections). The neuropsychologist, as well as the philosopher, has been faced with the problem that an individual's behavior is not easily described or predicted solely in terms of the probabilities of those occurrences in his environment which can be objectively analyzed. So often his behavior reaches concordance with such objective "reality" only by stages (Gibson & Gibson, 1955). This step by step procedure has been put to use by psychologists of the descriptive persuasion in the "shaping" of behavior (Ferster & Skinner, 1957).

The disparity between observed behavior and its more obvious determinants is most readily accounted for if one assumes the presence of a memory process that guides behavior, that memory process lawfully influenced by other ongoing processes in the organism and open to gradual, graded change by the objective events (Bruner, 1957; MacKay, 1956; Pribram, 1960b). But as long as such explanations rest on assumption, counter-arguments based on purely observable events, though less powerful explanatory tools, have the advantage of reliability. Once objective indices of "subjectivity" were available, this advantage would be lost. The neural sciences are now providing data to validate the presence of subjective states that intervene between experienced observables and observable actions determined by that experience.

First, the evidence that brain events take place concurrent with identifiable "states" in the absence of observable behavior. The most frequent and reliable data regard electrical records made from the precentral "motor" cortex of the brain. A change in electrical activity can be observed to accompany a subject's subsequently reported "thoughts" about preparing to move an extremity or a portion of it, even when the most careful observation (using electromyography) shows no muscular contractions to be taking place. The change in electrical activity is usually limited to the part of the brain cortex that controls the actions of the part "thought" about (Gurevich, 1961; Jasper & Penfield, 1949).

Such states can also be identified in animals (Adey, 1961). The subjects are cats. Fine wires are inserted into the depth of the brain and tied to the skull so that they can do no harm. The cats are placed facing a Y-shaped raised drawbridge. At the ends of the arms of the "Y" are two boxes about a yard apart, one of which contains food. As a flashing light is turned on above the box with the food, the drawbridge is lowered to form a path to each box.

During the first exposure to this situation, electrical recordings made from the brain of the cat disclose the characteristic pattern of alerting. With repeated exposure the recordings show increasing habituation. Since the cat begins to "expect" food when she reaches a box, the alerting pattern occurs only when she has chosen the empty box. The cat's performance can be judged as reliably from the recordings as from her observed behavior.

Occasionally, however, the electrical recording from the brain shows something is askew *before* the animal proceeds across the bridge. Whenever this record is observed, performance is again found to be at the chance level. In this instance, the brain record reflects uncertainty and *anticipates* the performance change: the "crucial microstate of the central nervous system" has been discerned.

But to go on with Carnap's logical analysis of credence. The quotation about the nonobservable microstate of the nervous system begun above continues:

> . . . Since his behavior is influenced by his state, *we can directly determine characteristics of his state from his behavior.* Thus experimental methods have been developed for the determination of some values and some general characteristics of the utility function and the credence function (subjective probability) of a person on the basis of his behavior with respect to bets and similar situations. Interesting investigations of this kind have been made by F. Mosteller, and P. Nogee and more recently by D. Davidson and P. Suppes and others (Carnap, 1962, p. 306) [italics mine].

If characteristics of the microstate can be obtained *directly* through operations on the behavior of the organism, what need is there for invoking the state at all—let behavior be a function of the operations imposed on the organism as the operationally inclined descriptive psychologists suggest. Gone is the very richness of the credence idea expressed in the sentence before: a microstate of the central nervous system, "*Not* his consciousness, *let alone* his overt behavior" [italics mine]. And lost are all of the fascinating determinants of microstate that are states in themselves, and not operations externally imposed on the behavior of the organism. Surely, Carnap does not want to lose as problems such possible determinants of betting as "level of activation" (Hebb, 1955; Lindsley, 1951; Magoun, 1958 and "tendency to explore" (q.v. below), both of which have ample neurological referents. And he need not. After all, it is this difficulty of what determines state which raised the issue of subjectivity in the first place—it is this issue which repeatedly drives psychologists into the neurological laboratory (for example, Neal Miller). And it is the work from the neurological laboratory that describes the formation and transformations of these important state variables.

Take, for instance, tendency to explore. Removal of a certain part of the brain cortex of monkeys has been shown to restrict their sampling of the environment (Pribram, 1959b). Normal subjects make choices out of a set of events that is, to a considerable extent, determined by their experience with that or a similar set. Damage to this particular part of the brain impairs the control which experienced events ordinarily exercise in delimiting such sets. The most extreme and therefore the clearest example of this process and its derangement comes from an analysis of the mechanisms that govern the appreciation of novelty.

An event is novel to an organism when it differs from prior events sufficiently to result in identifiable physiological and behavioral responses grouped together as the "orienting reaction." Repetition of the event leads to a gradual disappearance of the orienting reaction. The organism is said to habituate to the stimulus. However, habituation is not due to loss of reactivity: when, e.g., during repetition the intensity of a tone is suddenly diminished, the orienting responses reappear full blown. Also, when the duration of such a tone is suddenly shortened after habituation has occurred, an orienting reaction appears, but only after the tone has ceased, i.e., during the "silent period" which marks the difference between the length of this and the prior events (Sokolov, 1960).

These experiments and others leave beyond doubt that habituation of the orienting reaction reflects an active process guided by neural events that are now under study. This active process involves a continuous matching of the current sensory (sense organ) input to some state that is the result of prior inputs. This state has sufficient precision of detail encoded from these prior inputs to warrant the label "model" or "representation." It thus serves as the ground against which events attain sign(al)ificance—the set from which the environment can be sampled.

But there is more. This state, the psychologist's "set," which can be modified both by other organismic states and by the current input, has identifiable neurological determinants. Two basic mechanisms interact at the several levels of the nervous system. The first leads to progressive differentiation by the convergence of signals from disparate sources onto a common neuronal pool. The action of these pools is to admit and relay "this input or that," canceling out the other. The laws which govern such switching mechanisms are for the most part still to be formulated. The result of their action is better known: behavior comes for a period under the control of one or another set of input variables.

The second mechanism is possibly the more primitive. Even at the receptor, contrast is enhanced by inhibitory interaction extending laterally among neuron sheets. When the energy form, to which the receptor is sensitive, is distributed along a gradient, such lateral inhibition markedly steepens the gradient by effecting a diminution of the normal, spontaneous excitation of all elements except those most centrally located in the field (Hartline, Wagner, & Ratliff, 1956).

Some of the richness gained by the operation of these mechanisms even at the brain stem level of the nervous system (of relatively uncomplicated frogs) can be appreciated from the following experiments:

> *"Newness" neurons:* These cells have receptive fields about 30 degrees in diameter. . . . They are distributed so as to map continuously the visual field with much overlap. Such a neuron responds a little to sharp changes in illumination. If an object moves across the receptive field, there is a response whose frequency depends on the jerkiness, velocity, and direction of the movement, as well as on the size of the object. There is never an enduring response (p. 773).
>
> *"Sameness" neurons:* Let us begin with an empty gray hemisphere for the visual field. There is usually no response of the cell to turning on and off the illumination. It is silent. We bring in a small dark object, say 1 to 2 degrees in diameter, and at a certain point in its travel, almost anywhere in the field, the cell suddenly "notices" it. Thereafter, wherever that object is moved it is tracked by the cell. Every time it moves, with even the faintest jerk, there is a burst of impulses that dies down to a mutter that continues as long as the object is visible. If the object is kept moving, the bursts signal discontinuities in the movement, such as the turning of corners, reversals, and so forth, and these bursts occur against a continuous background mutter that tells us the object is visible to the cell.
>
> When the target is removed, the discharge dies down. If the target is kept absolutely stationary for about two minutes, the mutter also disappears. Then one can sneak the target around a bit, slowly, and produce no response, until the cell "notices" it again and locks on (Lettvin, Maturana, Pitts, & McCulloch, 1961, p. 774).

The ubiquity of the mechanisms is attested by the following quotations:

> The organization of visual neurons in the cortex may be explained by two principles of inhibition, which were first described in the retina: (a) reciprocal inhibition of antagonistic neurons in the same region; and (b) lateral inhibition of synergistic neurons in neighboring regions (Jung, 1961, p. 668).

> Evidence has been presented to support the conception that the posterior and the frontal intrinsic systems serve different aspects of the problem-solving process. The argument has been forwarded that two major classes of behavior can be distinguished, differential and intentional. . . . The distinction between neural mechanisms that serve differentiation and those that subserve intention is not a new one. Sherrington makes this distinction in his description of the coordination of reflexes (1947): The "singleness of action from moment to moment is the keystone in the construction of the individual." This singleness of action comes about in two ways—"interference" between and "allied combinations" of reflexes. In his analysis of "interference" (or antagonism) between reflexes, Sherrington forwards concepts such as inhibition, induction and spinal contrast—concepts which have relevance to dis-

> criminative behavior (for example . . . the use of the concept "induction" by Skinner [1938] for the occurrence of the "hump" in the graphical representation of complex discrimination learning). Sherrington uses these concepts to provide an understanding of the differences between reflex behaviors to different inputs. On the other hand, Sherrington's discussions of "allied combinations" of reflexes are an attempt to understand behavior regulated by outcomes: "the new reflex breaks in upon a condition of equilibrium, which latter is itself a reflex," a notion which has been enlarged upon by Cannon (1941) and more recently by Wiener (1949). In discussing allied combinations of reflexes, concepts such as reinforcement, convergence, summation and facilitation are used by Sherrington—concepts which have relevance to intentional behavior (Pribram, 1960a, p. 1340).

One thing stands out: a most important effect of these interacting mechanisms is continuous redundancy reduction (Barlow, 1961). The nervous system seems to ask, at every level, "Is this news?" As a whole, its activity has been compared to that of an editor whose function it is to communicate only that which is newsworthy. But news must be "fit" to communicate, that is, it must fit within the context of the encoded residuals of prior inputs, yet be insufficiently "same" to result in a signal of "mismatch." The accepted must not be too far beyond the expected.

These data have a fundamental bearing on the choice of theory used to subsume the data that have been accumulated by experimenting empiricists dealing with the problems of knowledge: with perception, learning, and decision. Again and again such theorists have found it necessary to postulate some mediating states, events intermediating between those to be perceived or learned and the responses made by the organism to these events. But always this need has come up against the operationist's unease with nonobservables. Only theorists of the "cognitive" persuasion have steadfastly and clearly maintained that there is no other way out of the dilemmas posed by the richness and orderliness of individuals' variability in reactions to apparently identical situations. And now, the new neurology suddenly places these cognitivists with their sets, expectancies, plans, and credences, on solid operational ground. Meanwhile, descriptive behavior theorists and professed nontheorists have reached the awkward position of inability even to properly define "stimulus events," much less their state variables and, most importantly, the reinforcers without which they cannot work. Neuropsychology here has the role not so much of theory-building, as of selection among otherwise equally persuasive (and each in its own way defective) approaches to the same body of knowledge. Without question the decision, for this season at least, goes to the cognitive theorists—provided they adhere to the golden operational rules of the behaviorist. This is after all what a neuropsychologically based, empiricistic, subjective behaviorism is about (Miller, Galanter, & Pribram, 1960).

> The old puzzle of induction consists in the following dilemma. On the one hand we see that inductive reasoning is used by the scientist and the man in the street every day without apparent scruples; and we have the feeling that it is valid and indispensable. On the other hand, once Hume awakens our intellectual conscience, we find no answer to his objection. Who is right, the man of common sense or the critical philosopher? We see that, as so often, both are partially right. Hume's criticism of the customary forms of induction was correct. But still the basic idea of common sense thinking is vindicated: induction, if properly reformulated, can be shown to be valid by rational criteria (Carnap, 1961, p. 318).

ON BRAIN AND THE STRUCTURE OF MIND

> There has been a great deal of speculation in traditional philosophy which might have been avoided if the importance of structure, and the difficulty of getting behind it, had been realized (Russell, 1956, p. 61).

These considerations of neuropsychologically based subjective behaviorism lead directly into a discussion of the ever vexing dichotomous formulation of mind versus brain. I have dealt with this subject extensively elsewhere (Pribram, 1962a). Some additional comments can now be made, however. The argument was forwarded that the mind-brain gap would be closed by experimental results obtained when variables in two adjacent universes of discourse (for instance, the neural and the behavioral) are simultaneously manipulated. Reference terms between these universes result. The caution was voiced that communication would never amount to complete transliteration. The limitations encountered in any communication (even within the same universe of discourse) have been ably discussed by Quine (1960). These limitations apply to an even greater extent when the levels at which the discourse is directed are disparate. But it is in the very recognition of these limitations that the problem becomes resolved: pseudo-monistic identity of the material with the mental process (or the converse) and dualistic parallelism are no longer possible solutions. Once levels of discourse are recognized as such, and the potentialities and limitations of communication between them are accepted, the only recourse is to a truly monistic, seemingly pluralistic, multilevel *structural* mindbrain. As one scientist-philosopher (Rioch, personal communication) aptly put it, to have mind there must be at least two brains.

Mental terms are primarily derived from propositional verbal reports of introspection; these verbal reports must be analyzed in the linguistic social context within which the speaker and listener communicate, and interpreted in conjunction with nonpropositional aspects such as the kinesics of the verbal report and other instrumental behaviors supplied by the reporter. But validity is a level loving thing; when levels can become meshed we are apt to consider a report valid. So, to the extent that neural (or other organ system) data extend validity into the biological realm of discourse, mental terms become respectable even to the tough minded physicalist. Ask any physical or biological scientist to discuss vision and he won't bat an eyelid, though this term is no less mental than is its generic concept, perception; and if we recognize perception, what about emotion, cognition, or volition? The difference is, of course, the degree to which meshing of levels of discourse has taken place. In the case of vision, the physical descriptions of the energies that activate the eye, the minute structure of the eye, the afferent paths into and through the central nervous system, and the central control over the optic mechanism are all thoroughly in hand, as are some of the relations between these structures. Furthermore, these descriptions go into the structure of the perceptual events in detail; knowledge at different levels is available about color, pattern, brightness, and visual field. Finally, level by level reference terms are daily encountered, not only in the ophthalmological and neurological clinics, but as well in the daily experience of everyone who does bat his eyelids to demonstrate the relation between "I see" and "eye."

Structure, hierarchically arranged by reference terms among levels: this is what the biologist usually refers to as process or mechanism. When mechanism is so conceived, it does not violate logic and experience as does the usual extreme mechanistic, reductionist position. The Beethoven symphony to which I am at the moment listening is not in one sense reducible to the mechanics of the score, nor of the recording, receiver, amplifier, and speaker system which is emitting it; nor is it completely described by the contortions set up in my auditory apparatus by the describable wave patterns impinging on my ears. All these and more are components—but something more than this constitutes the symphony. This something more is not mystical. Musicians call it structure.

I do not consider the mystery of the symphony the more (nor the less) mysterious for the fact that one very crucial element in the structure of its reproduction is a piece of light cardboard shaped in a cone, whose crucial characteristics are difficult to pin down. I do not invoke the epithet "mentalist" at the British Industries Corporation, nor call them less competent engineers because they say:

> Your own ear is the best judge of the ability of a speaker system to recreate the emotional impact of the original musical performance. Technical details can not be expected to answer the question "Does it sound natural?" Each person must listen and judge for himself (British Industries Corporation, 1962, p. 77).

I merely validate their experience with my own—which if possible includes running pure tones, harmonics, and complex sounds through portions of the equipment, to satisfy my desire for minimal distortion. But

I also listen to the symphony. And, in the same way, I also unashamedly listen to my own introspections and to verbal reports of others, as well as to the records of instrumental behavior and to the responses of neurons, to build my multilevel monistic structure of the neuropsychological apparatus.

And your reply, rightly, may well be, "Bully for you, but why should I accept your view of the universe and the way it ought to be constructed?" Or, to put it another way, can the search for constants or invariants in the exact natural sciences be properly extended to include the problems faced by the social disciplines? As a *neuro*psychologist my answer is a resounding yes. I would not deny Eve her root biological entity, her identity and unity. Yet the many faces shown by the social Eve are nonetheless real for their evanescence. Physics has gracefully accepted the principles of complementarity and of indeterminacy: one way of looking at the natural world complements, not necessarily supplements, another; what at one level of analysis appears structurally stable and ordered may, at another level, reveal a goodly amount of chaos—and structure is often shown to emerge from the very probabilities that describe the amount of this chaos.

Are matters so utterly different in the biological-social science enterprise which comes to a focus in neuropsychology? If the answer were a simple "no" it should have been given easily by now. Wherein lies the difficulty? I believe that the complication lies in the fact that the behavioral, biological-social scientist interested in the mind–body problem finds his universe to be a mirror image of the universe constructed by the physical scientist who deals with the same problem. And it should not come as a surprise when each of these isomers, the one produced by the physicist and the one produced by the behavioral scientist, on occasion displays properties that differ considerably from one another, much as do optical isomers in organic chemistry.

I believe these images are mirrors because of differences in the direction generally pursued from each investigator's effective starting point, his own observation. The physical scientist, for the most part, constructs his universe by ever more refined analysis of systems of input variables, that is, sensory stimuli to which he reacts. The form of the reaction (cathode-ray tube, solid-state device, chromatography, or galvanometer) is unimportant, except that it provides a sufficiently broad communicative base. Constancies are gradually retrieved from manipulations and observations of these input variables under a variety of conditions. As these constants achieve stability, the "correctness" of the views that produced them is asserted: the physical universe is properly described.

In the social disciplines the direction pursued is often just the reverse. Analysis is made of *action* systems (cf. Parsons & Bales, 1953). The exact nature of the input to the actor (including the observing scientist) is of little consequence, provided it has sufficient communicative base; the effect of action on the system is the subject of analysis. It matters little (perhaps because the cause is usually multiple and/or indeterminable) if a currency is deflated because of fear of inflation, depression, personal whim, or misguided economic theory. The effects of deflation can be studied, are knowable. And once known, the action *becomes* corrective; the resulting stabilization, constancy, is interpreted as evidence for the "correctness" of the action that produced the correction. Appropriate norms for the social universe become established.

One striking difference between the two images thus formed is immediately apparent. The physicist's macroscopic universe is the more stable, predictable one: "It does not hurt the moon to look at it" (Eddington, 1958, p. 227). For the most part, it is as he moves to ever more microscopic worlds that uncertainties are asserted. The scientist concerned with social matters finds it just the other way round: it seemingly does little harm to the man to look at him; but seriously look at his family, his friendships, or his political-economic systems and what you had started out to look at changes with the looking. Here indeterminacy comes to plague the macrostructure; it is in the stabilities of microanalysis that the mirage of safety appears.

The philosopher of science and the neuropsychologist, interested as they must be in the mind-brain problem, stand by necessity squarely between these two mirror images. If they deny the evidence that there are two images by showing interest in only one, or by denying the "reality" of the other, they are in dangerous waters and liable to shipwreck in the strong currents of mentalism, physicalism, and dualism. Their searches for the one "real" world and its mirror image may well be interminable, since an alternative possibility is equally likely to be a correct one.

The problem can be grasped, however, if it is dealt with in terms of isomeric forms of the same event universe—isomers differing in that their *structures* mirror each other. Put another way, the problem resolves itself into a meshing of the descriptive and the normative sciences. The suggestion is that structure in descriptive science ordinarily emerges from the analysis of the relations between systems and their subsystems; that in the normative sciences, it is largely the other way round: structure emerges when the relation between a system and its "supersystem" is studied.

If this view is correct, we should find normative statements about the nature of the physical world when these are constructed from the examination of relations between a set of systems and a higher order system. Is not relativity just this sort of statement? This is not a social scientist speaking about the "criterion problem":

> The modest observer . . . [is] faced with the task of choosing between a number of frames of space with nothing to guide his choice. They are different in

the sense that they frame the material objects of the world, including the observer himself, differently; but they are indistinguishable in the sense that the world as framed in one space conducts itself according to precisely the same laws as the world framed in another space. Owing to the accident of having been born on a particular planet our observer has hitherto unthinkingly adopted one of the frames; but he realises that this is no ground for obstinately asserting that it must be the right frame. Which is the right frame?

At this juncture Einstein comes forward with a suggestion—

"You are seeking a frame of space which you call the *right* frame. In what does its *rightness* consist?"

You are standing with a label in your hand before a row of packages all precisely similar. You are worried because there is nothing to help you to decide which of the packages it should be attached to. Look at the label and see what is written on it. Nothing.

"Right" as applied to frames of space is a blank label. It implies that there is something distinguishing a right frame from a wrong frame; but when we ask what is this distinguishing property, the only answer we receive is "Rightness," which does not make the meaning clearer or convince us that there is a meaning (Eddington, 1958, p. 20).

Obversely, we should find descriptive statements about the nature of the social world when these derive from a study of the relations between a system and its subsystems. Doesn't the following passage fit this requirement?

Role behavior depends first of all on the role positions that society establishes; that is certain ways of behaving toward others are defined by different positions (Hilgard, 1962, p. 482).

Aren't statements about roles unambiguously descriptive?

Attention to structure has left the neuropsychologist, perhaps a bit dizzily, contemplating two mirror images of a universe. By looking to the right, he has profited greatly from the researches of his neurobiological colleagues in matters concerning memory mechanisms. Is there any substantial insight to be reaped from a look to the left?

ON AN EFFECTIVE ETHIC —A NEUROPSYCHOLOGICAL DIVIDEND

Considered in the main, the best communities are those which have the best men for their members, and the best men are the members of the best communities. Circle as this is, it is not a vicious circle. The two problems of the best man and best state are two sides, two distinguishable aspects of the one problem, how to realize in human nature the perfect unity of homogeneity and specification; and when we see that each of these without the other is unreal, then we see that (speaking in general) the welfare of the state and the welfare of its individuals are questions which it is mistaken and ruinous

> to separate. Personal morality and political and social institutions cannot exist apart, and (in general) the better the one the better the other. The community is moral because it realizes personal morality; personal morality is moral because and in so far as it realizes the moral whole (Bradley, 1951, p. 123 [1876]).

The argument presented, if it has merit, should prove supportable by evidence. The nature of this evidence ought to demonstrate an effective influence felt in the social disciplines as a result of knowledge held on the neuropsychological level. Is there any such evidence?

There is, and it comes from an unexpected and controversial quarter. The fact of controversy in itself attests to effective influence; the unexpectedness demonstrates the little recognized importance of the neuropsychological aspects of the contribution.

I am of course speaking of Sigmund Freud and of psychoanalysis. I have elsewhere (Pribram, 1962b) reviewed the *Zeitgeist* in Vienna, the setting in which Freud made his contributions. This setting included the activities of the Viennese functionalists, especially in the person of Brentano, heirs to the issue of the elusive activity of thinking versus the determinable content of the thought process, which had been raised by the Würzburg school.

In one sense, psychoanalysis is a *technique* whereby the activity of thinking can be explored: whereas the Würzburgers provided a set, in the form of a problem to be explored by the thinker, the psychoanalytic method allows exploration among many possible sets. The Würzburgers thus arrived at lawful descriptions of content within the sets they had presented; the psychoanalysts, by contrast, have attempted to describe a lawful, developmental process of formation of sets (and subsets) by eliciting a variety of partitions, graded according to accessibility, to which content can be subject. Basic to the success of the psychoanalytical approach was an uncompromising faith in the lawfulness of the relation between accessibility (availability to conscious introspection and thus to verbal report) and the structure of psychological processes. This faith presupposed a thorough acquaintance both with the mechanisms by which experience leaves its mark and by which it is utilized: in other words, with the mechanisms that determine memory storage and retrieval.

Only within the past decade has it become generally known that Freud indeed relied heavily on a model of the way in which experience leaves its mark on the nervous system. As a rule, Freud's contributions to basic neurology have been ignored except to point out that he left them behind to go on to endeavors felt to be really "important" or "misguided" according to whether the viewer came from a soft or a hard science background. Careful examination of Freud's *Project for a Scientific Psychology* (1954) and perusal of later works (in collaboration with Dr. Merton Gill) shows that despite protestations to the contrary, Freud repeatedly turned to neurology for his model—that indeed his model, though altered in detail and emphasis, remained in most essentials the model first conceived as "The Project." (As Gill has pointed out, the difference between the structural and topographic models is one of emphasis: during the structural phase Freud deals primarily with the relative accessibility to consciousness of experienced events; during the topographic phase he deals primarily with the relation between drive and satisfaction, irrespective of accessibility. The model changes little. It is viewed from different vantages. And it continues to be neurological.)

The question remains to be answered as to whether this model performed some service other than to sustain Freud's morale (in the paradoxical sense that he could blame many of his creative woes on its inadequacy). Another way to put this question is to ask whether the neurological model is essentially, crucially, though in unrecognized form, involved in psychoanalytic dogma.

Neurology has little to contribute to the study of consciousness even today. Whatever neurologizing is done depends on a very few facts: the *déjà vu* phenomenon when epilepsy stems from an anterior temporal lobe focus; loss of awareness when the structures around the midline cerebral ventricles are manipulated; interruption of ongoing actions (including verbal) by excitations of certain locations on the brain cortex. These are not the threads from which any richly woven theory of consciousness, psychoanalytic or other, can be derived. Freud had to base his ideas in this area on the behavioral reports obtained from his patients, his knowledge of hysterical phenomena, and on his introspections.

Drive is certainly biologically conceived, and most psychologists, whether of analytical persuasion or not, feel that Freud's neurological background led to his emphasis on *Triebe* factors in the development and determination of behavior. Close reading of the Project shows this to be a half-truth. Drive is mentioned only once as such. Freud did speculate on the mechanism of *Unlust*, but to do this, he had to postulate a neurology of internal receptors and key (secretory) neurons only sparsely supported by the facts of his or our day. The neurological model is consonant with a mechanism of drive—but "drive" cannot be constructed from the neurological facts. Behavioral observation, especially of the infant, had a great deal to do with the construction of this part of the model in the Project.

Not the "unconscious"; not "instinct or drive"; what then is peculiarly neurological about psychoanalytic theory? Surprisingly, it is the mechanisms of defense. Composed as they are of the processes that determine memory and motive, which in turn were derived by Freud from the neural properties of resistance (to neural impulse conduction at the synapse) and cathexes (the graded potential changes to which neural tissue is subject), defenses are structures which would not have been conceived as such except by someone deeply concerned and conversant with the neural regulation of behavior.

The neurological nature of the conception "defense" can be recapitulated as follows: *At the tissue level,* Freud accepts the neuron doctrine—nervous tissue is made up of cells separated from one another by contact barriers (these were later called synapses by Sherrington). Contact barriers have the property, resistance. Resistance hinders the transmission of propagated, impulsive neural excitation—the nerve impulse—across the synapse. Neurons have two properties: (1) they transmit impulses along their extent and (2) they change their excitatory state in another, local, nontransmitted fashion. This second property, cathexis, plays a major role in all of Freud's thinking and, as I have indicated elsewhere, is a scientifically well-established, though until recently neglected, aspect of nervous tissue function.

At this level, Freud considers the memory trace as formed by selective lowering of synaptic resistances at locations subject to large and/or repeated excitations of the impulse type.

This has consequences at the brain system level. Within the core of the brain there are diffusely organized systems, within which neurons branch profusely and make contact more or less randomly with many other neurons (Freud provides a drawing). These he labels ψ or "nuclear." From the fact that behavior is selective, Freud reasons that the lowering of resistance must occur selectively among these contacts. Preferred paths develop along which excitation is propagated: the structure of the sum-total of these paths provides the retentive and selective control exercised over behavior by the nervous system, that is, this structure is the mechanism for memory and motive.

Two groups of factors determine the form of the memory-motive structure. Its location at the core of the brain makes it especially receptive to stimuli concerned in the regulation of the body's internal milieu (the work begun by Claude Bernard which led later to the enunciation by Cannon of the notion of homeostasis). These are Freud's drive factors and he spells out their presumed operation in detail. The second group of factors influences the ψ nuclear systems through another set, the projection systems, concerned more directly with sensory-motor relations between the organism and its external environment.

The reasoning then proceeds: since little can be done to alter the internal environment when this becomes necessary (for example, when the organism is hungry) except through changes made in the external, the two sets of factors come to converge (by simultaneity) in their effect on the ψ nuclear systems. This is especially the case in the human infant, dependent as he is on a caretaking person to effect those changes in his external environment necessary to his internal stability (or well being). Man's memory-motive structure, therefore, is laden with these doubly-determined pathways which "defend" his well being, that is, allow him to make adjustments in his external environment such that his milieu interieur does not suffer radical disequilibration. This part of the memory-motive structure is referred to by Freud as the defense mechanism—neural configurations based on experience that give selective direction to behavior.

At least three levels of defense are recognized. (1) Reflex defense, which is really no defense at all: this consists of an infant's built-in mechanism that distributes excitation generally throughout the nervous system, and so to effectors whose activity calls the attention of the caretaking person to the infant's distress. (2) Primary defense, which is a partial defense: this mechanism is operative when a memory trace has become established consequent to the experience of pain or "unlust." During primary defense, these memory traces are rapidly decathected, that is, they lose their excitatory potential. This is due to a precipitous discharge (much as that of a condensor) when the excitation in passage becomes overly great. The memory trace thus becomes temporarily inoperative (for example, it cannot affect awareness, thus memory; guide action, and so on). Nor is distress (pain, *Unlust*) prevented—but, since excitation shunts past the memory mechanism, this at least does not add to the generation and maintenance of the disequilibratory process. (3) The third, ordinarily operative, adult defense: here the network of memory traces consequent on the distressful experience has grown to sufficient proportions that excitation is "bound" within the network. Precipitous discharge does not occur and the cathected memory-trace network can exert a delaying and selective influence on more localized and patterned neural discharges (nerve impulses), and so control behavior.

This, then, is the structural nature of defenses. To complete the argument, the question must be posed as to whether this particular facet of psychoanalytic theory has had social impact. Much has been made recently of the replacement or dilution of the Protestant ethic by what is called the Freudian (Rieff, 1959). A case can be made for the proposition that the two ethics differ essentially in the way they conceive the interaction of the two types of determinants that compose the structure of human motive. Paradoxically, the Protestant ethic considers that (internal) drive factors need be defended *against* by socially (externally) imparted directives; the Freudian, because he believes that drive and social directive are symbiotic in establishing motive structure in the nervous system, comes to a less defensive view of defenses. In the Protestant ethic, control of behavior is based largely on the role of social determinants; control of behavior for the Freudian is exercised by a memory-motive structure which results from inexorable intermeshing of drive and social determinants. This structure is defensive only in that it defends against breakdown of the organism's homeostasis, with a consequent threat to the social fabric.

In the extreme sense, this structure neither defends society against man's drives (his "baser" nature) nor man against his society—though both claims have been made for it. Yet, in a special sense, the Freudian conception of defense does both, by the strength of recognition of each and of the mesh between the two. This is the strength that has had the power to make itself felt as an ethic; this is the strength that has led to effective impact. And this strength results to a remarkable extent from the highly detailed and accurate neurological origins of the conception.

"Considered in the main, the best communities are those which have the best men for their members, and the best men are the members of the best communities." And these best men possess the best defenses—best in that they mesh, through the neurological process, their biological and social systems. In the same sense, the best communities are also those which possess the best defenses—best in that they mesh, through accurate awareness of the structure of the political-economic process, their internal and international systems. Neuropsychology provides this model for an effective ethic.

> In the preface to that admirable collection of essays of his called "Heretics," Mr. Chesterton writes these words: "There are some people—and I am one of them—who think that the most practical and important thing about a man is still his view of the universe. We think that for a landlady considering a lodger it is important to know his income, but still more important to know his philosophy. We think that for a general about to fight an enemy it is important to know the enemy's numbers, but still more important to know the enemy's philosophy. We think the question is not whether the theory of the cosmos affects matters, but whether in the long run anything else affects them." I think with Mr. Chesterton in this matter (James, 1931, p. 3).

REFERENCES

ABT, J. P., ESSMAN, W. B., & JARVIK, M. E. Ether induced retrograde amnesia for one-trial conditioning in mice. *Science*, 1961, 133, 1477-1478.

ADEY, W. R. Studies of hippocampal mechanisms in learning. In *Brain mechanisms and learning*. Oxford: Blackwell Scientific Publications, 1961.

BACH, L. M. N. Regional physiology of the central nervous system. In E. A. Spiegel (Ed.), *Progress in neurology and psychiatry*. Vol. 17. New York: Grune & Stratton, 1962. Pp. 43-71.

BARLOW, H. B. Possible principles underlying the transformations of sensory messages. In W. A. Rosenblith (Ed.), *Sensory communication*. New York: M.I.T. Press and Wiley, 1961. Pp. 217-234.

BORING, E. G. *A history of experimental psychology*. (2nd ed.) New York: Appleton-Century-Crofts, 1950.

BRADLEY, F. H. My station and its duties (1876). In F. H. Bradley, *Ethical studies*. New York: Liberal Arts Press, 1951.

BRADY, J. V. The effect of electroconvulsive shock on a conditioned emotional response: the permanence of the effect. *J. comp. physiol. Psychol.*, 1951, **41**, 507-511.

BRATTGÅRD, S. The importance of adequate stimulation for the chemical composition of retinal ganglion cells during early post-natal development. *Acta Radiol.* Suppl. 96, 1952.

BREEN, R. A., & MCGAUGH, J. Facilitation of maze learning with posttrial injections of picrotoxin. *J. comp. physiol. Psychol.*, 1961, **54**, 498-501.

BRIGGS, M. H., & KITTO, G. B. The molecular basis of memory and learning. *Psychol. Rev.*, 1962, **69**, 537-541.

BRITISH INDUSTRIES CORPORATION. Advertisement for the Wharfedale W60 achromatic speaker system. *High fidelity*, 1962, **77**, 77.

BRUNER, J. S. On perceptual readiness. *Psychol. Rev.*, 1957, **64**, 123-152.

BRUNER, J. S. *On knowing.* Cambridge: Harvard Univ. Press, 1962.

CANNON, W. B. The body physiologic and the body politic. *Science*, 1941, **93**, 1-10.

CARNAP, R. The aim of inductive logic. In E. Nagel, P. Suppes, and A. Tarski (Eds.), *Logic, methodology, and philosophy of science.* Stanford, Calif.: Stanford Univ. Press, 1962.

CORNING, W. C., & JOHN, E. R. The effects of ribonuclease on retention of conditioned response in regenerated planarian. *Science*, 1961, **134**, 1363-1365.

CURTIS, D. R., & ECCLES, J. C. Synaptic action during and after repetitive stimulation. *J. Physiol.*, 1960, **150**, 374-398.

DEUTSCH, J. A. Higher nervous function: the physiological bases of memory. *Annual Rev. Physiol.*, 1962, **24**, 259-286.

DUNCAN, C. P. The retroactive effect of electro-shock on learning. *J. comp. physiol. Psychol.*, 1949, **42**, 32-44.

ECCLES, J. C. Prolonged functional changes (plasticity) in the nervous system. In J. C. Eccles, *The neurophysiological basis of mind.* Oxford: Clarendon Press, 1953. Pp. 193-227.

EDDINGTON, A. *The nature of the physical world* (1927). Ann Arbor: Univ. Michigan Press, 1958.

EDDINGTON, A. *New pathways in science* (1934). Ann Arbor: Univ. Michigan Press, 1959.

FERSTER, C. B., & SKINNER, B. F. *Schedules of reinforcement.* New York: Appleton-Century-Crofts, 1957.

FREUD, S. Project for a scientific psychology. Appendix in *The origins of psychoanalysis: letters to Wilhelm Fliess, drafts and notes, 1887-1902.* New York: Basic Books, 1954.

FULLER, J. L., ROSVOLD, H. E., & PRIBRAM, K. H. The effect on affective and cognitive behavior in the dog of lesions of the pyriform-amygdala-hippocampal complex. *J. comp. physiol. Psychol.*, 1957, **50**, 89-96.

GAITO, J. A biochemical approach to learning and memory. *Psychol. Rev.*, 1961, **68**, 288-292.

GIBSON, J. J., & GIBSON, E. J. Perceptual learning: differentiation or enrichment. *Psychol. Rev.*, 1955, **62**, 32-41.

GUREVICH, B. KH. Electrical indicators of the "tuning-regulatory" activity of the parietal association areas. *Doklady Akademii Nauk USSR*, 1961, **141**, 505-508.

HARTLINE, H. K., WAGNER, H. G., & RATLIFF, F. Inhibition in the eye of limulus. *J. gen. Physiol.*, 1956, **39**, 651-673.

HEBB, D. O. *The organization of behavior*. New York: Wiley, 1949.

HEBB, D. O. Drives and the CNS (conceptual nervous system). *Psychol. Rev.*, 1955, **62**, 243-254.

HILGARD, E. *Introduction to psychology*. (3rd ed.) New York: Harcourt, Brace & World, 1962.

HYDÈN, H. Biochemical aspects of brain activity. In S. M. Farber and R. H. L. Wilson (Eds.), *Man and civilization, control of the mind*. New York: McGraw-Hill, 1961.

JAMES, W. *Pragmatism—a new name for some old ways of thinking*. (1907). New York: Longmans, 1931.

JASPER, H., & PENFIELD, W. Electrocorticograms in man: effect of voluntary movement upon the electrical activity of the precentral gyrus. *Arch. Psychiat. Z. Neurol.*, 1949, **183**, 163-174.

JUNG, R. Neuronal integration in the visual cortex and its significance for visual information. In W. Rosenblith (Ed.), *Sensory communication*. New York: M.I.T. Press and Wiley, 1961. Pp. 627-674.

KRAFT, MARCIA S., OBRIST, W. D., & PRIBRAM, K. H. The effect of irritative lesions of the striate cortex on learning of visual discriminations in monkeys. *J. comp. physiol. Psychol.*, 1960, **53**, 17-22.

KRECH, D., ROSENZWEIG, M. R., & BENNETT, E. L. Dimensions of discrimination and the level of cholinesterase activity in the cerebral cortex of the rat. *J. comp. physiol. Psychol.*, 1956, **49**, 261-268.

KRECH, D., ROSENZWEIG, M. R., & BENNETT, E. L. Correlation between brain cholinesterase and brain weight within two strains of rats. *Amer. J. Physiol.*, 1959, **196**, 31-32.

LANDAHL, H. D., MCCULLOCH, W. S., & PITTS, W. A. statistical consequence of the logical calculus of nervous nets. *Bull. Mathematical Biophysics*, 1943, **5**, 135-137.

LETTVIN, J. Y., MATURANA, H. R., PITTS, W. H., & MCCULLOCH, W. S. Two remarks on the visual system of the frog. In W. Rosenblith (Ed.), *Sensory communication*. New York: M.I.T. Press and Wiley, 1961. Pp. 757-776.

LIBERMAN, R. Retinal cholinesterase and glycolysis in rats raised in darkness. *Science*, 1962, **135**, 372-373.

LINDSLEY, D. B. Emotion. In S. S. Stevens (Ed.), *Handbook of experimental psychology*. New York: Wiley, 1951. Pp. 473-516.

MACKAY, D. M. The epistemological problem for automata. *Automata studies*. Princeton Univ. Press, 1956. Pp. 235-252.

MACKAY, D. M., & MCCULLOCH, W. S. The limiting information capacity of a neuronal link. *Bull. math. Biophysics*, 1952, **14**, 127-135.

MADSEN, M. C., & MCGAUGH, J. L. The effect of ECS on one-trial avoidance learning. *J. comp. physiol. Psychol.*, 1961, **54**, 522-523.

MAGOUN, H. W. *The waking brain*. Springfield, Ill.: Charles C. Thomas, 1958.

MALIS, L. I., PRIBRAM, K. H., & KRUGER, L. Action potentials in "motor" cortex evoked by peripheral nerve stimulation. *J. Neurophysiol.*, 1953, **16**, 161-167.

McConnell, J. O., Jacobson, A. L., & Kimble, D. P. The effects of regeneration upon retention of a conditioned response in the planarian. *J. comp. physiol. Psychol.*, 1959, **52**, 1-5.

McCulloch, W. S. The stability of biological systems. In *Homeostatic mechanisms* (Vol. 10, *Brookhaven Symposia in Biology*). Upton, N.Y.: 1957. Pp. 207-214.

McCulloch, W. S., & Pitts, W. Logical calculus of the ideas immanent in nervous activity. *Bull. math. Biophysics*, 1943, **5**, 115-133.

McGaugh, J. L. Facilitative and disruptive effects of strychnine sulphate on maze learning. *Psychol. Rep.*, 1961, **8**, 99-104.

McGaugh, J. L., & Petrinovich, L. The effect of strychnine sulphate on maze-learning. *Amer. J. Psychol.*, 1959, **72**, 99-102.

McGaugh, J. L., & Thomson, C. W. Facilitation of simultaneous discrimination learning with strychnine sulphate. *Psychopharmacologia*, 1962, **3**, 166-172.

McGaugh, J. L., Thomson, C. W., Westbrook, W., & Hudspeth, W. A further study of learning facilitation with strychnine sulphate. *Psychopharmacologia*, 1962, **3**, 352-360.

McGaugh, J. L., Westbrook, W., & Burt, G. Strain differences in the facilitative effects of 5-7-diphenyl-1-3-diazadamantan-6-ol (1757 I.S.) on maze learning. *J. comp. physiol. Psychol.*, 1961, **54**, 502-505.

McGaugh, J. L., Westbrook, W., & Thomson, C. W. Facilitation of maze learning with posttrial injections of 5-7-diphenyl-1-3-diazadamantan-6-ol (1757 I.S.). *J. comp. physiol. Psychol.*, 1962, **55**, 710-713.

Miller, G. A., Galanter, E., & Pribram, K. H. *Plans and the structure of behavior.* New York: Henry Holt, 1960.

Morrell, F. Lasting changes in synaptic organization produced by continuous neural bombardment. In UNESCO Symposium (1959), *Brain mechanisms and learning.* Oxford: Blackwell Scientific Publications, 1961. Pp. 375-392.

Parsons, T., & Bales, R. F. The dimensions of action-space. In T. Parsons, R. Bales, & E. Shils (Eds.), *Working papers in the theory of action.* New York: Free Press, 1953. Pp. 63-110.

Pearlman, C. A., & Jarvik, M. E. Retrograde amnesia produced by spreading cortical depression. *Federal Proc.*, 1961, **20**, 340. (Abstract)

Pearlman, C. A., Sharpless, S. K., & Jarvik, M. E. Retrograde amnesia produced by anesthetic and convulsant agents. *J. comp. physiol. Psychol.*, 1961, **54**, 109-112.

Penfield, W. Functional localization in temporal and deep sylvian areas. In H. Solomon, S. Cobb, & W. Penfield (Eds.), *The brain and human behavior* (Assn. Nerv. Ment. Dis., Vol. 36). Baltimore: Williams & Wilkins, 1958. Pp. 210-226.

Poschel, B. P. H. Proactive and retroactive effects of electroconvulsive shock on approach–avoidance conflict. *J. comp. physiol. Psychol.*, 1957, **50**, 392-396.

Pribram, K. H. Toward a science of neuropsychology: method and data. In *Current trends in psychology and the behavioral sciences.* Univ. Pittsburgh Press, 1954. Pp. 115-142.

PRIBRAM, K. H. Neocortical function in behavior. In *Biological and biochemical bases of behavior*. Madison: Univ. Wisconsin Press, 1958. Pp. 151-172.

PRIBRAM, K. H. Neuropsychology in America. In *The Voice of America forum lectures* (Behavioral Science Series 9). Washington, D.C.: U. S. Information Agency, 1959. (a)

PRIBRAM, K. H. On the neurology of thinking. *Behav. Sci.*, 1959, **4**, 265-287. (b)

PRIBRAM, K. H. The intrinsic systems of the forebrain: an alternative to the concept of cortical association areas. In *Handbook of physiology, neuropsychology, II*. American Physiological Society, 1960. Pp. 1323-1344. (a)

PRIBRAM, K. H. A review of theory in physiological psychology. In *Annual review of psychology*. Palo Alto, Calif.: Annual Reviews, Inc., 1960. Pp. 1-40. (b)

PRIBRAM, K. H. Regional physiology of the central nervous system (the search for the engram—decade of decision). In E. A. Spiegel (Ed.), *Progress in neurology and psychiatry*. Vol. XVII. New York: Grune & Stratton, 1961. Pp. 45-57.

PRIBRAM, K. H. Interrelations of psychology and the neurological disciplines. In S. Koch (Ed.), *Psychology: a study of science*. Vol. 4. *Biologically oriented fields: their place in psychology and in biological sciences*. New York: McGraw-Hill, 1962. Pp. 119-157. (a)

PRIBRAM, K. H. The neuropsychology of Sigmund Freud. In A. J. Bachrach (Ed.), *Experimental foundations of clinical psychology*. New York: Basic Books, 1962. (b)

PRIBRAM, K. H. Neural mechanisms in memory and thought. In G. H. Glaser (Ed.), *EEG and behavior*. New York: Basic Books, 1963. Pp. 149-173.

PRIBRAM, K. H., & BAGSHAW, MURIEL. Further analysis of the temporal lobe syndrome utilizing fronto-temporal ablations. *J. comp. Neurol.*, 1953, **99**, 347-375.

PRIBRAM, K. H., KRUGER, L., ROBINSON, F., & BERMAN, A. J. The effects of precentral lesions on the behavior of monkeys. *Yale J. Biol. Med.*, 1955-1956, **28**, 428-443.

QUINE, W. O. *Word and object*. New York: Wiley, 1960.

RASCH, E., SWIFT, H., RIESEN, A. H., & CHOW, K. L. Altered structure and composition of retinal cells in dark-reared mammals. *Exp. Cell Res.*, 1961, **25**, 348-363.

RIEFF, P. *Freud; the mind of the moralist*. New York: Viking Press, 1959.

RIESMAN, D., GLAZER, N., & DENNY, R. *The lonely crowd: a study of the changing American character*. New Haven: Yale Univ. Press, 1950.

ROBERTSON, J. D. Cell membranes and the origin of mitochondria. In S. Kety & J. Elkes (Eds.), *Regional neurochemistry*. New York: Pergamon Press, 1961. Pp. 497-534.

ROSE, J. E., MALIS, L. I., & BAKER, C. P. Neural growth in the cerebral cortex after lesions produced by monoenergetic denterons. In W. Rosenblith (Ed.), *Sensory communication*. New York: M.I.T. Press and Wiley, 1961. Pp. 279-301.

RUSSELL, B. *Introduction to mathematical philosophy*. 1919. London: Allen & Unwin, 1956.

SHERRINGTON, C. *The integrative action of the nervous system.* 1906. New Haven: Yale Univ. Press, 1947.

SJÖSTRAND, F. S. Electron microscopy of myelin and of nerve cells and tissue. In J. N. Cumings (Ed.), *Modern scientific aspects of neurology.* London: Edward Arnold, 1960. Pp. 188-231.

SKINNER, B. F. *The behavior of organisms: an experimental analysis.* New York: Appleton-Century-Crofts, 1938.

SOKOLOV, E. N. In M. A. B. Brazier (Ed.), *The central nervous system and behavior: transactions of the third conference.* New York: Josiah Macy, Jr., Foundation, 1960.

STAMM, J. S., & PRIBRAM, K. H. Effects of epileptogenic lesions in frontal cortex on learning and retention in monkeys. *J. Neurophysiol.,* 1960, **23,** 552-563.

STAMM, J. S., & PRIBAM, K. H. Effects of epileptogenic lesions in inferotemporal cortex on learning and retention in monkeys. *J. comp. physiol. Psychol.,* 1961, **54,** 614-618.

STAMM, J. S., & WARREN, ANN. Learning and retention by monkeys with epileptogenic implants in posterior parietal cortex. *Epilepsia,* 1961, **2,** 229-242.

VON FOERSTER, H. *Das Gedächtnis.* Vienna: Franz Deuticke, 1948.

WALL, P. D. Two transmission systems for skin sensations. In W. Rosenblith (Ed.), *Sensory communication.* New York: M.I.T. Press and Wiley, 1961. Pp. 475-496.

WEISKRANTZ, L. Sensory deprivation and the cat's optic nervous system. *Nature,* 1958, **181,** 1047-1050.

WEISKRANTZ, L. Presentation at Conference on RNA and Brain Function, Los Angeles, November 1962, in press.

WIDROW, B., & HOFF, M. E. *Adaptive switching circuits.* Stanford, Calif.: Stanford Electronics Laboratories, 1960.

WIENER, N. *Cybernetics.* New York: Wiley, 1949.