

INTERRELATIONS OF PSYCHOLOGY AND  
THE NEUROLOGICAL DISCIPLINES

KARL H. PRIBRAM

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# INTERRELATIONS OF PSYCHOLOGY AND THE NEUROLOGICAL DISCIPLINES

KARL H. PRIBRAM  
*Department of Psychiatry*  
*Stanford University School of Medicine*

4 With the advancement of practical and scientific knowledge through  
4 the centuries the primitive demonolatrics have been generally (although by  
4 no means universally) abandoned in the domain of inorganic nature. The  
4 primitive spiritistic tradition lingers, however, under various disguises in  
4 many reputable scientific circles, and in the vast domain of human affairs  
4 probably the majority of men today believe as one of their most cherished  
4 articles of faith that the human personality comprises a physical body which  
4 is "natural" and a spirit which is unnatural and in some inscrutable way may  
4 control the movements of the natural body.

4 If it is true that human nature is a blend of the natural and the un-  
4 natural, then natural science is baffled—and the basic problem of psycho-  
4 biology is scientifically insoluble and must be turned over to the metaphysi-  
4 cians. This is the opinion of many philosophers and a considerable number  
4 of biologists. Some of the latter try to evade the issue by exclusion of every-  
4 thing "mentalistic" from their science. That, of course, is a feckless subter-  
4 fuge, for this opinion, like every other, was conceived mentally and has no  
4 existence apart from this mental act. It does no good to declare that con-  
4 sciousness is a negligible epiphenomenon in the face of the fact that the most  
4 significant things people do are consciously motivated and consciously  
4 directed. One does not solve a problem by leaving out the troublesome  
4 factors. . . .

4<sup>c</sup> Defeatism is an unhealthy scientific attitude; it is, in fact, radically  
50 unscientific, for science has a legitimate interest in everything of which we  
50 have veridical experience. We have ample scientific evidence that a man's  
51 mental processes—his thoughts and emotions—are tied in with his physical  
51 behavior in lawfully ordered ways. If we do not know just how this is done,  
51 the thing to do is to try to find out by skillfully designed experiments. The  
51 answer will never be found if the plain facts of common experience are  
51 ignored and the mental factors of behavior are left out of consideration  
51 [C. JUDSON HERRICK, *The Evolution of Human Nature*, pp. 234-236].

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## INTRODUCTION

The task assigned me is to discuss the interrelationships between psychology and the neurological sciences. This task has been largely in the hands of philosophers of science—yet every thoughtful neurologist and many psychologists are concerned, at one time or another, with the traditional mind-brain problem. I believe that the moment is at hand for intensive inquiry into the problem, inquiry that will prove fruitful not only to philosophers but to empirical and experimental scientists and clinicians as well.

The thesis to be presented runs something like this: The widest gap today in the scientific universe of discourse, and indeed of all human discourse, is that between the humanistically oriented social disciplines on the one hand and the physical and biological sciences on the other. The bridge across the gap is held by a handful of individuals of various backgrounds whose investigations and observations are directed to the clarification of the relation between the brain and mental processes. The efforts of these individuals are contributing a body of knowledge—a body of knowledge which is appropriately named “neuropsychology.” This body of knowledge can serve human endeavor much as does that of biochemistry. Today’s discontinuity in the universe of discourse parallels that which existed a century ago between the physiological and physical sciences. The slogan “vitalism” characterized the issue

in the nineteenth century; today "mentalism" has taken this place. A common framework for the physical and physiological sciences resulted from *experiments*—experiments such as the synthesis of urea. I believe that a framework common to the physiological and behavioral sciences is emerging from experiments—especially neurobehavioral experiments.

Neurobehavioral data are already sufficient to allow first attempts at lawful systematization. Systematic presentation of data that unite the now disparate and often discordant views of man's universe can be one of the major achievements of the second half of the twentieth century. Such an achievement can make the further step to socially practical consequences only to the extent that the presentation becomes part of the educational process. Toward this end, the suggestion is offered that neuropsychiatry, which now is practically devoid of training in any basic discipline related to its body of knowledge, make an effort to nourish the lusty neuropsychological infant.

## THE MIND-BRAIN RELATIONSHIP

### What Is Neuropsychology?

The empirical evidence upon which translations between psychological and neurological concepts can be based are the results of neurobehavioral experiments and observations. The systematization of such evidence constitutes the science of *neuropsychology*. By definition, neuropsychology is a reductive discipline. As such, it partakes of the characteristics of its nonreductive siblings, psychology and neurology. The three sciences differ in that the dependent variables used to gather relevant data represent the interaction of different systems of independent variables. Though all three sciences have in common a reference to *environment*—systems of independent variables that can be completely specified by the techniques of the physical sciences<sup>1</sup>—psychological and neurological sciences differ in the systems of variables specified by biological techniques: in the psychological sciences these systems refer to the whole organism, in the neurological sciences to only part of that organism. Neuropsychology, if it is to be effectively reductive, must relate all three systems of variables: environmental, organismic, and neural. When structural concepts are under consideration, these distinctions appear self-evident. However, when function is in question, the temptation arises to confound two of these three classes of independent variables. The neurologist is prone to disregard the distinction between organism and environment—psycholo-

<sup>1</sup> Social environment is excluded here for purposes of simplification. See the section *Bridging Laws and Methods of Systematizing* for an analysis of this aspect of the problem.

gizing of neural processes follows and activity in the brain-stem reticular formation becomes identified with consciousness. On the other hand, the psychologist is prone to disregard the distinction between organism and its parts—neurologizing of the psychological process is a common consequence, and drive becomes synonymous with hypothalamic function.

Three corollaries follow from these basic statements. One is a general proposition regarding the limitations of reductive disciplines. The properties of a system are not given simply by summing the properties of the component subsystems. An understanding of the wetness of water or the fact that it floats when frozen is not derived solely from an understanding of the properties of its constituents, hydrogen and oxygen. The property, coalition, cannot be understood in terms of study of the behavior of the monadic or dyadic components of the group in which the coalition forms. The neural processes uncovered by neuropsychological analysis are thus expected, at best, to have properties that critically determine those of the behavioral system—*never are the neural and the behavioral processes identical.*

Second, according to these basic statements, clinical neurology (and neurosurgery) are neuropsychological as well as neurological disciplines, since many useful concepts are derived from the neurological examination—an examination of the behavior of the patient under specified environmental conditions. Direct examination of the nervous system (e.g., by X rays) is, of course, recognized as such in the clinic, but the behavioral aspects of the neurological examination and the resulting *psychological* concepts (e.g., the visual field) are seldom recognized for what they are. The kinship between clinical neurology and experimental neuropsychology is not a superficial one and the current lack of communication between the two endeavors must be remedied.

Third, there is a danger that the neurologist and neurosurgeon—and the experimental neuropsychologist associated with the clinic—make the error of early introspectionists in psychology. The propositional verbal reports of introspections of patients whose brains have lesions or are being excited in the surgery are insufficient in themselves to provide more than the initial fragments of data for a scientific neuropsychology. Concepts derived from these fragments must be validated by the use of other nonlinguistic behavioral techniques lest ambiguity result. Statements about consciousness, sensations, and the like are sufficiently vague to allow multiple interpretations. Precision is attained when the situations in which verbal reports are obtained are varied, and several nonpropositional verbal or other behavioral dependent variables, preferably measurable ones, are used. Unless this is accomplished, identical data can be variously construed: e.g., electrical stimulation of the temporal isocortex of unanesthetized man results in verbal reports of experiences not unlike

those which can be spontaneously recalled. On this basis, the interpretation has been made at one time that the temporal isocortex serves memory—at another time, that perceptions are located there. Obviously, either the data or the concepts or both are imprecise. And these deficiencies can readily be remedied (see the section *On the Neurology of Intention and Will*).

### Neural Variables Critical to Behavior

This approach to the relation between psychology and neurology places emphasis on a laboratory analysis of problems that are often initially posed introspectively. Such a problem-oriented neuropsychological science need not be technique-bound and is free to search through all sorts of phenomena. After all, scientific endeavor so often begins with childlike wonders, such as the observation that one can “attend” an object other than that upon which the gaze is fixed. This wonder and the many like it are not different in kind from those concerned with the fall of an apple or the whistle of the steaming kettle. But the empirical solution to the problem differs—in the case of the falling apple or the whistling kettle, the relations are between physical objects in the environment of the observer; in the case of attention, between the observer and his environment. To overcome the difficulties posed by this difference, the first step to empirical solution of the psychological problems must be that the observer observes other observers whose reports can be collated with his own observations—the behaviorist’s approach. Just as a precise specification of the variables that determine falling and whistling is possible, so also a precise specification of the variables that determine attending is possible. But only some of these variables turn out to be critical, i.e., in any system of interacting variables, only the properties of some determine the *essential* characteristics of the system as a whole. The molecular properties of water, when heated, and the dimensional properties of the opening in the tea kettle critically determine the whistle; the particular type of heating element, the shape of the kettle, and the wetness of water are irrelevant, though heat, kettle, and water are necessary constituents of the system. In like manner, though much of the organism and certain aspects of environment are necessary constituents of the systems of variables that describe attending, only some of them can be considered critical. For complex mental processes—attending, judgment, attitude, and thought—critical organismic variables appear to be located in the brain: the behavior from which these complex processes are inferred remains essentially unimpaired after a man has suffered a fairly high cervical transection of his spinal axis—an observation which has led to the notion that the head is not hollow [11]. And this notion is supported by evidence that such behaviors are deranged when the brain is

injured or artificially irritated. Experiments can therefore be performed to add precision to the notion. For example, exactly what are the neural mechanisms that make it possible to fix the gaze on one object and yet respond reliably to another? Though the answer to this particular question is not yet available, answers to similar questions can be obtained. An example of such an answer follows.

#### ON THE NEUROLOGY OF INTENTION AND WILL: AN EXAMPLE OF NEUROBEHAVIORAL ANALYSIS

The concept of will has a peculiar place in neuropsychology today. On the one hand, experimentalists of the behaviorist tradition usually dismiss the issues subsumed as recalcitrant to direct observation. On the other, to neurologists, the terms *voluntary movement* and *willed action* are part of the everyday language used uncritically in the clinic and in the surgical amphitheater. The gap between these realms of endeavor and their universes of discourse seems indeed great. Yet *rapprochement* might be more easily achieved than appears at first glance. Properties of the "operant" of the behaviorist have been compared and identified by some with those of voluntary behavior of man [20]. A large body of evidence about intent has been gathered by those interested in clinical and social problems in psychology [24]. And the neurological data that are relevant to these issues are by no means sparse [32, 8]. Experiments undertaken to help bridge the gap are feasible.

The case for will is to be presented somewhat as follows: Certain basal forebrain structures have been shown to control relatively simple appetitive behavior, e.g., eating. The question is asked whether these structures are concerned exclusively with the regulation of such drives or whether their concern extends to other appetitive-like behavior sequences.

Experiments are described which show the effects of lesions of these forebrain systems to be due to interference with an active build-up of an adaptive neural process used by the organism to identify the invariant and recurrent properties of his environment.

Next is pursued the proposal that the organism's own behavior and its consequences become a part of the total stimulus situation which the organism internalizes. The neural nature of this internalization process is examined and its relation to error sensitivity is discussed. The suggestion is made that thus *intentions*—the identification and prediction of the consequences of actions—are initiated.

Finally, another experiment is presented to show that, once they are initiated, intentions often guide behavior somewhat independently of the immediate outcome of any particular action. Adequate or erroneous

performances, even though identified, must be able to modify the existing neural representation in order to gain active control over behavior. This fact is clearly brought out by the experiment which demonstrates that several alternative, though predictable, response patterns serve equally well to reduce error. The most forward part of the frontal cortex of primates is shown to be concerned with this type of choice among the alternative performances. This choice depends not on situational variables per se and therefore not on those aspects of the model built up within the brain that deal with identifications of the properties of the environment. Rather, the choice is guided by some other neural process that determines the distribution of responses. And it is argued that it is these intentional determinants of choice that make plausible the concept of will.

### The Initiation of Intentions

Operant behavior is tested in a situation in which a simple response pattern is used as an indicator of an organism's actions in that situation [10]. The apparatus used in this series of experiments is similar to the one-armed bandit familiar to those who inhabit gambling institutions. And the "willful" behavior of the monkeys, apparently guided only remotely by the outcomes of their actions, is also familiar to those who have watched any but the most "skillful" gamblers in action.

At least three sets of measures were found usefully related to the events that determine this operant behavior. One of these is the number of reinforcements obtained per unit time. Another is the rate at which the response takes place. Finally, the distribution of responses across any time unit can be specified. Perhaps the simplest of the situations in which this measurement can be applied is the so-called "fixed-interval situation." In this, an organism is permitted to make a response and, at some predetermined and equal time interval regulated by a clock, he is given a signal that the response is appropriate (a reward). In such a situation, the organism tends to group his responses in the period just prior to the occurrence of the reward. As a rule, a gradually increasing number of responses is made—the increase reaching its maximum immediately prior to the occasion for reward. When responses are recorded cumulatively over time, a smooth "scallop" describes the behavior of the organism in this situation. The three measures of the behavior already noted can be taken: (1) the number of reinforcements obtained during a training session; (2) the total number of responses per unit time (the rate of response); and (3) the shape of the response curve (the scallop), which can be determined either by recording the time between responses or by graphing the per cent of the total number of responses made during successive portions of the interval. Experiments were undertaken to find

out whether these measures could be independently affected and, if they could, to note which changes in the environment and in the organism were related to each.

**Experiment 1: the effect of food deprivation [35].** In this experiment, 12 rhesus monkeys were trained to press a lever in a situation in which a  $\frac{1}{2}$ -gram food pellet appeared in a tray every 2 min, provided the lever was pressed at least once at or after 2 min had elapsed since the last pellet was obtained. Each training session lasted 2 hr and the monkeys were fed a sufficient amount of laboratory chow immediately after each session to maintain them at approximately 80 per cent of the weight which they had attained after a 3-week period of ad libitum feeding. Daily weighing assured this 80 per cent figure. All animals were tested every other day (except Sunday) for two hours until a stable level of performance was obtained. The occurrence of responses and pellets obtained was recorded in two fashions: (1) on a moving paper tape in which an ink writer stepped perpendicular to the time axis whenever the lever was pressed, and a large, very brief excursion in the direction opposite to the step indicated the delivery of the pellet; (2) on counters so arranged as to accumulate the number of responses made during each of six equal subdivisions of all of the total 2-min intervals of a testing session. In this manner, a performance graph could be constructed for each of the testing sessions or for any multiple number of such sessions to demonstrate the distribution of responses across any number of 2-min intervals. Averages of the responses of groups of animals could be established, and the variations between performances of an animal or between individual animals could be taken into account.

The aim of this experiment was to find out whether the rate or the distribution of responses was the sensitive index of changes that result from manipulations of food deprivation. The experiment consisted of the following procedure. Ten 2-hr sessions were given. Then each animal was subjected to a 72-hr fast and retested for one session, after which the prefast schedule was immediately resumed—again for ten sessions. The entire procedure was repeated once again; this time the monkeys were fasted for 118 hr before the test session.

The results are shown in Fig. 1. As can be seen clearly, the effects of food deprivation are upon the rate of response and not upon the number of reinforcements obtained nor upon the distribution of responses across the interval. Note the minimal variation between sessions and between animals. Rate, *not* response distribution, is altered when monkeys are starved from 3 to 5 days.

**Experiment 2: the effect of amygdalotomy [43].** The aim of this experiment was to discover some of the neural mechanisms that regulate these alterations of the rate of response that are determined by food de-

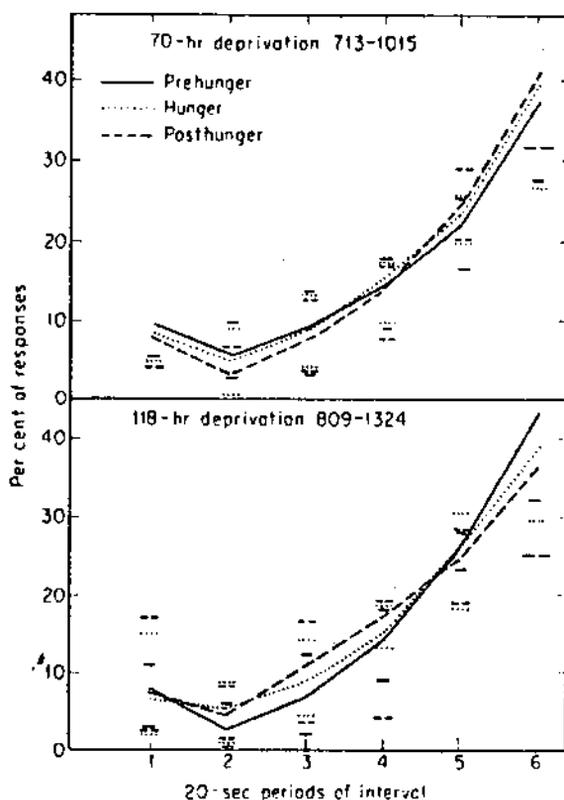


FIG. 1. Graph showing the effect of food deprivation on monkeys' rate of lever-pressing response to food (a small pellet of laboratory chow) which became available every two minutes. The change in total rate is indicated by numbers under the deprivation label. The lack of change in the distribution of responses is shown by the curves. Each curve represents the average of the responses of 10 monkeys; each point represents the average rate during a period of the interval over ten hours of testing. Variance is indicated by the short horizontal bars.

privation. From the results of earlier experiments [50], it appeared likely that bilateral removal of the amygdaloid complex in the basal forebrain would affect the rate-deprivation interaction.

The experimental procedure was essentially the same as that used in Experiment 1, except that two groups of four monkeys each were used. One served as an unoperated control group; the monkeys in the other had been given a one-stage bilateral amygdalectomy some 6 months prior to testing.

The results are plotted in Fig. 2. The changes are those in performance of operated and unoperated monkeys which follow prolonged deprivation of food in the same situations as were described in Experiment 1. When amygdalotomized monkeys are used, they are far less responsive than normal monkeys to the deprivation, although they are by no means insensitive to it. The over-all group difference averaging across sessions is significant by analysis of variants at well beyond the 0.05 level ( $F = 7.25$  for 1 and 6  $dF$ ).

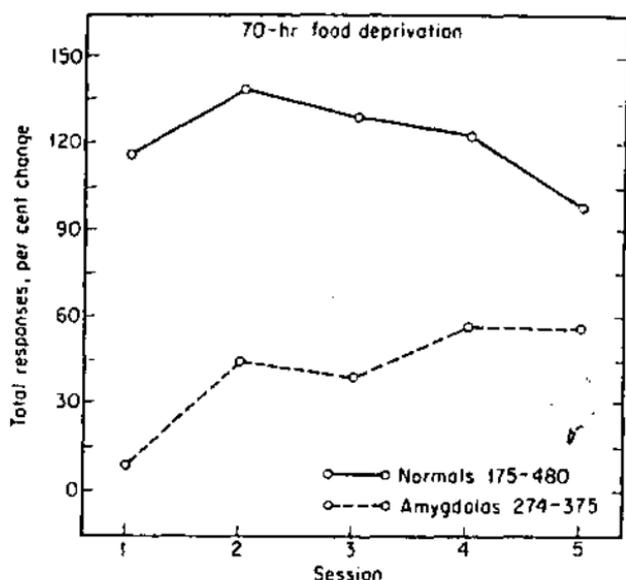


FIG. 2. Mean percentage changes in total responses of test sessions which followed prolonged deprivation of food. The values in the legend refer to the range of total responses for the three preceding control sessions on which the percentage changes are based.

*Interpretation of experiments 1 and 2.* How is this effect of amygdalotomy to be interpreted? Amygdalotomized animals tend to eat more in an ad libitum feeding situation [37, 43]. This would point to an increased food drive in these animals. Yet they also show decreased responsiveness to prolonged deprivation. This consistently lower level of performance with prolonged deprivation could be interpreted to show that under these conditions amygdalotomized monkeys have a lower than normal drive for food. Perhaps a single process could be conceived to be so impaired that the organism knows neither when he is hungry nor when he is sated. Or separate appetitive "start" and satiety "stop" processes could be involved and reciprocally affected by the brain lesion.

There is an alternative way to interpret the more persistent pattern of responding by the amygdalectomized monkeys. Rather than postulate an impairment specific to food hunger and satiety, the more sustained response (whether of not eating or of eating) may be symptomatic of a general disturbance in habituation processes that extends to classes of stimuli other than food. Thus, amygdalectomized animals are reported to be generally more responsive as well as persistent in their response to objects in their environment—the so-called “hypermetamorphic reaction” (of Wernicke) described by Klüver and Bucy. The operated monkeys also show much less of a locomotor-reaction decrement with repeated tests in a novel situation [51]. These findings suggest that general habituation factors are presumably involved in so far as satiation arises from repeated exteroceptive sensory consequences of food, in addition to postingestional consequences. Amygdaloid hyperphagia would, therefore, be interpreted in terms of some form of general defect in satiation or habituation not specific to food consumption.<sup>2</sup>

**Experiment 3: transposition [44].** In this experiment, the notion of a generalized defect in habituation was tested by examining the performance of amygdalectomized monkeys in a transposition situation. The tests were run in a modified Wisconsin General Testing Apparatus. Eight rhesus monkeys were used—four had had bilateral ablations of the amygdaloid region approximately a year prior to this experiment; four served as unoperated control subjects. The Wisconsin General Testing Apparatus was fitted with a specially designed board, 18¼ in. high and 24 in. long, that sloped toward the animal at an angle of 80° to the horizontal plane. The board contained two 4¼<sub>16</sub>-in. square cutouts, spaced 3¾ in. apart and 6 in. above the base, which accommodated a pair of stimulus panels measuring 4 by 4 in. Each stimulus panel was mounted on a frame hinged at the top to the board. Thus the panels could be swung open, allowing access to a small food cup located behind each one. The hinges were designed in such a way that the panels could be easily interchanged. Interposed between the board and the test cage was a movable one-way screen which shielded the board from the animal between trials. A distance of 6 in. separated the panels from the front of the test cage. Two 40-watt fluorescent fixtures mounted above and to the side of the test cage provided the illumination.

The stimuli consisted of three different shades of gray, each painted on a separate panel. The grays were prepared by appropriate mixture of a flat black and a flat white paint so as to appear spaced equally apart. When matched to the Munsell Neutral Value Scale, the following values were obtained: dark gray, N 3.5; medium gray, N 5.0; light gray, N 7.5. The board itself was finished in a flat black to provide a uniform

<sup>2</sup> For further evidence see Schwartzbaum [43].

contrasting background for the stimuli. Both the stimuli and the board were sprayed lightly with a transparent fixative to minimize fingerprints and other stains. Several panels were prepared with each of the grays so that a given panel was never used throughout a test session.

Three days of preliminary training were required to accustom the animals to operate the panels and to retrieve a food reward. In this training the Ss received 30 trials a day with two black panels. Either response was rewarded except when persistent position biases developed. In this event, the opposite response was rewarded selectively until the position habit was broken. The reward used throughout the experiment consisted of half a peanut.

The formal testing began with a simultaneous form of gray discrimination. A dark-gray stimulus was paired with a medium-gray stimulus, with the medium gray as the positive stimulus, i.e., associated with the reward, for all animals. Thirty trials a day were given, using a non-correction technique in which the positions of the stimuli were varied in a balanced order in accordance with a Gellerman procedure. The trials were spaced approximately 25 sec. apart. Training continued to a criterion of at least 90 per cent correct responses on each of 2 consecutive days.

The transposition tests followed the attainment of the criterion on the initial discrimination. The previously positive medium gray was now paired with a light-gray stimulus, and both were made positive. These conditions maximize the contribution of the prior training in the transposition performance. Six tests of transposition were carried out daily for two consecutive days during a continuation of the original discrimination procedure. Thus a total of 12 such observations was obtained. The transposition stimuli appeared in counterbalanced position on every fifth trial of these sessions. This meant that on 24 of the 30 trials the dark-gray and medium-gray stimuli were presented. On 6 of the trials the medium-gray and light-gray stimuli appeared.

The results show that the amygdalectomized monkeys tended to learn the brightness discrimination slightly more slowly than did the normal monkeys, but the differences were small and are not significant statistically. They required an average of 172 trials (range of 150 to 240) and 60 errors (range of 51 to 76) to reach criterion, exclusive of criterion performance. The normal animals averaged 112 trials (range of 90 to 150) and 48 errors (range of 34 to 69). A *t* test of these mean differences yields values of 2.25 and 1.60 for the two measures respectively, which for 6 *df* do not reach the 0.05 level. Indeed, the differences obtained are exaggerated by the fact that, of all the animals, two in the lesion group missed by 1 error, attaining criterion in 120 instead of 150 trials.

The transposition tests, on the other hand, strongly differentiated the two groups. As shown in Table 1, the normal animals markedly transposed their responses to the light gray. The over-all median for the group was 11 transposed responses out of a possible 12, with little variability among animals. In contrast, the amygdalectomized animals did not typically transpose. Their median number of transposed responses was 5.5. Three animals in the group approximated closely the chance level of performance, with perhaps a slight preference for the previously positive medium-gray stimulus. The fourth animal, which took longest to learn the original discrimination, exhibited a normal pattern of transposition.

TABLE 1. NUMBER OF TRANSPOSED RESPONSES MADE ON TRANSPOSITION TESTS

	Normals				Median	Amygdalectomized				Median
	439	441	443	447		397	405	438	442	
Day 1 .....	6	5	6	6		2	5	2	4	
Day 2 .....	5	5	5	6		3	6	2	2	
Total .....	11	10	11	12	11.0	5	11	4	6	5.5

**Experiment 4: sequential discrimination [44].** In order to define better the characteristics of the impaired transposition behavior, an additional test was performed. A sequential form of brightness discrimination was presented in which the medium gray was paired randomly on different trials with either the dark gray or the light gray—in either event, the medium gray remained as the positive stimulus. If the amygdalectomized animals had indeed shown a stronger response tendency toward the previously positive stimulus of the transposition pair, then they would be expected to do relatively well on this sequential discrimination. If, on the other hand, their transposition performance had simply reflected the transient effects of a novel stimulus, then there would be little reason to expect any group differences on a test that involved frequent repetitions of the experimental condition. But in neither case would deleterious effects of the lesion be anticipated. The results obtained, however, tend to rule out both these possibilities.

The same set of Ss that completed the transposition experiment was tested on the sequential-brightness discrimination. They were given 30 trials a day, using a noncorrection technique. Each pair of stimuli appeared in a randomized order on half the trials within a session. The criterion of learning was set at 90 per cent correct responses on each of 2 consecutive days. The sequential discrimination was separated from the last transposition test by 2 additional days of training with the

original pair of stimuli. All animals performed at criterion level on both of these sessions.

The results are summarized in Table 2. The number of trials required by each group to meet criterion on the sequential discrimination demonstrates clearly a deficit in the performance of the amygdallectomized animals, although they were by no means unable to learn the problem. They required about five times as many trials to reach criterion as did the normal animals ( $p = 0.028$  by a two-tailed Mann-Whitney U Test). But the rapidity with which the normal animals mastered the problems must also be noted.

A comparison of the performance on the sequential discrimination with that on the original discrimination, expressed as a percentage change in the number of trials to criterion (Table 2), provides a check

TABLE 2. PERFORMANCE ON SEQUENTIAL BRIGHTNESS DISCRIMINATION

	Normals					Amygdallectomized				
	Number of subject				Mean	Number of subject				Mean
	439	441	443	447		397	405	438	442	
Trials to criterion*	60	30	30	30	38	150	330	210	120	202
Per cent change from initial discrimination	-60	-67	-67	-75	-67	0	38	40	-20	14

\* Excludes criterion trials.

for any initial group differences. Examination of the data shows that the groups still differed markedly on this measure of performance in the sequential discrimination ( $p = 0.028$ ). The normal animals mastered the sequential discrimination in about two-thirds fewer trials than they took to learn the original discrimination. The animals with lesions required about the same number of trials as before.

It can be seen from Table 3 that the difficulty which the amygdallectomized animals encountered on the sequential-brightness discrimination related almost exclusively to the presentations of the transposition pair of stimuli. About 95 per cent of their total errors occurred with the transposition stimuli. This accounts for virtually all of the differences in performance between the groups on the sequential discrimination. Animal AM-405 showed this same form of impairment on the first 150 trials; then its performance pattern broke down into a more generally distributed deficit.

*Interpretation of experiments 3 and 4.* The transposition findings are consistent with the supposition that the effects of amygdallectomy transcend situations specific to the obtaining and consumption of food.

Amygdectomy thus may be conceived to produce some generalized defect in satiation or habituation. And additional evidence for this view comes from an entirely different source—the results of electrophysiological experiments. Stimulations of and recordings made from the basal forebrain structures such as the amygdaloid and hippocampal regions are interpreted, e.g., by Grastyan [13], to indicate that these structures normally function to prevent repeated diversions of attention and to make it possible for conditioning to occur.

The story goes something like this: An organism's exposure to a relatively intense novel environmental stimulus is accomplished by generalized desynchronization in the electrical activity recorded from both

TABLE 3. DISTRIBUTION OF ERRORS ON SEQUENTIAL BRIGHTNESS DISCRIMINATION

	Normals				Median	Amygdalectomized				Median
	Number of subject					Number of subject				
	439	441	443	447		397	405	438	442	
Total errors <sup>o</sup> .....	15	10	4	11	10.5	43	131	49	32	46.0
							60†			
Per cent total errors with transposition stimuli.....	87	100	100	91	95.5	95	76	94	100	94.5
							90†			

<sup>o</sup> Excludes criterion trials.

† Based on first 150 trials.

isocortex and the basal forebrain (e.g., hippocampus). This startle reaction or orienting reflex is accompanied by behavioral arrest of movement except for head and eye (and perhaps body) orientation toward the stimulating event. This initial state may give way to behavioral fight or flight. Of particular interest here, however, are those occasions where the original stimulus remains or is repeated and the organism becomes familiar with the events. In these instances, desynchronization of the isocortically recorded electrical activity continues, but hypersynchronous slow waves are now obtained from the basal forebrain. When such hypersynchrony is experimentally induced in these structures, on-going problem-solving behavior is interfered with [26] in much the same fashion as when these structures are surgically removed [15, 40]. As already noted, animals with such lesions are hyperreactive to novel stimulation. The assumption is therefore made that the slow activity in the basal forebrain reflects the cessation of its usual gating action on the central effects of novel stimuli. (This gating action is conceived to take place via the brain-stem reticular formation.)

This second stage is called the "orienting reaction"—to distinguish it from the orienting reflex—and is characterized by heightened behavioral orientation and attention to *all* aspects of the environment. With repetition of the situation, however, another stage sets in—the organism is said to "habituate." This stage is again characterized by both isocortical and basal forebrain electrical desynchronization—though the isocortical manifestation is now no longer generalized but relatively restricted, in the experimental situation, to the cortex subserving the sensory mode through which the environmental stimulus has been presented.

Even more impressive evidence for the time course of the neural activity involved in this process has been demonstrated by E. Roy John and the Killams [18], who employed electrical tracers in the form of visual-stimulus frequencies to which the brain's electrical activity becomes locked. Their tracer frequencies are found generally in recordings made from electrodes implanted in allo- and isocortical formulations during the initial stages of the problem-solving behavior of cats. As the experiment proceeds, these tracer frequencies become more limited in distribution until, during error-free performance, they are recorded only from the isocortical systems (geniculostriate) concerned with vision.

Furthermore, when such a problem-solving situation is used, W. Ross Adey [1, 2, 3] has shown, by a beautiful series of experiments, that basal forebrain electrical activity (recorded from the hippocampus) is also characteristically different in the initial startle stage of the experiment and the final conditioned stage. As habituation proceeds, a shift is recorded in electrical phase of the activity of the several layers of the hippocampal cortex. In the initial stage, the electrical activity of the layers that are connected with the brain-stem core (including the reticular formation) precedes that recorded from the layers more immediately connected to the isocortex; in the final stage, the phase relationship is reversed. In this final stage in the problem-solving situation, the behavior of the habituated organism is appropriate to the task, i.e., performance is approximately errorless. When occasional errors do occur, they are accompanied by the recrudescence of slow activity in the electrical record made from the basal forebrain!

But what is this "habituation" [45] that thus becomes so all-important? Is it merely the fatiguing-out of the neural mechanism of attention? The indications from the animal experiments already reviewed are to the contrary. Sokolov [46], in another exquisitely designed and performed series of experiments, has further demonstrated that habituation in man results to the extent that a neural representation of the stimulus is built up in the nervous system. When the input (e.g., a tone) matches this representation, no characteristic behavioral, autonomic

effector, or electroencephalographic (EEG) responses can be recorded; when the input departs from prior inputs (e.g., diminution of the intensity of the tone, or making the tone shorter or longer), it reevokes the orienting responses (behavioral, autonomic effector, and EEG). This reevocation is limited to the specific occasions on, and durations over, which current input is disparate from prior inputs!

Taken together, the electrophysiological and neuropsychological evidence points to a series of specifiable stages that can be summarized as follows:

1. When exposed to a novel event, an organism takes this in—and this stage is accompanied by desynchronization of the electrical activity of both the isocortical and basal allocortical formations of the endbrain. The only behavioral concomitants of this stage are “reflex” orientation movements that focus the stimulating event. Lacey [21] has noted that this stage corresponds pretty much to “primary attention” as this was defined in introspective psychology.

2. Should this novel event recur repeatedly, remain unchanged, or change relatively slowly, another process supervenes. This is characterized by continued desynchronization in the electrical activity recorded from the isocortex, but a change in the activity recorded from allocortical structures (especially of Ammon’s formation). From this neural location, slow waves (i.e., hypersynchrony) can now be recorded. Behaviorally, searching characterizes the activity of the organism. This is the orienting reaction—the organism follows the stimulating event, searches when changes occur and especially once habituation is under way. In many respects, this is similar to the secondary attention described by the introspectionists.

3. After repeated exposure to the unchanging or recurrent event, habituation has resulted. The desynchronous electrical activity recorded from isocortex has become restricted to relevant input channels and slow activity has disappeared from allocortical structures. Here, electrical phase has shifted from precedence of brain-stem input to precedence of input from isocortex. And any noted change in the situation is immediately and specifically accompanied by recrudescence of the electrical activities in both the iso- and allocortex characteristic of stage 2 (the orienting stage).

During stage 3, the actions of the organism directed toward the stimulating event may be included in the habituation process. This obtains in conditioning and other problem-solving situations. So, orienting responses reemerge when the organism’s actions result in changes from the recurrent regularities that characterize the total stimulus event—e.g., when the oft-obtained reinforcing food stimulus fails to appear, or when the for-the-most-part-eliminated shock stimulus fails to be avoided.

In other words, the organism now has a mechanism for sensing error or incongruity!<sup>9</sup> The rudiment necessary to intentional behavior is laid down.

Note that sensitivity to incongruity thus depends on maintenance of a modifiable neural representational process built up during habituation. Resection of the amygdaloid region of the forebrain could produce the reported behavior in the transposition experiments if, during the habituation that accompanied learning of the discrimination, an overly rigid representational process had been laid down. Something like a sharpening of the generalization gradient would be evident: i.e., the organism's behavior would remain invariant over a more restricted range of input events—he would be *more* sensitive to incongruity. So conceived, the normally functioning amygdaloid region is deemed necessary to the continued modification of neural representations—in the absence of the amygdaloid region, representation would become fixed, and these fixed representations would narrowly proscribe limits beyond which the organism would respond as if the situation were novel. This interpretation of the data is in essential agreement with an interpretation of the effects of amygdectomy made earlier, but at that time restricted to behavior controlled by hypothalamic mechanisms [38]. The suggestion was forwarded then that amygdectomy removes normal regulation on homeostatic processes—that, as a result of the surgery, the homeostat can get stuck either in a hyper- or hypo- position, depending on the current state of the organism, its experiential history, as well as the environmental situation. A considerable amount of evidence has accumulated to lend support to this suggestion [7]. However, the results of the experiments reported here emphasize that the effect is not restricted to feeding or other situations usually thought to involve the organism's control of its internal environment. If this analysis proves viable, the amygdaloid region and its neighbors contain neural mechanisms that operate to control the organism's behavior with respect to his external environment as well.

Specific aspects of this mechanism can be further tested in the laboratory. The effect of amygdectomy on measures of the orienting response (autonomic effector, EEG, and behavioral) can readily be determined. The effect of such lesions, and those of other basal forebrain structures, on an organism's sensitivity to error and other forms of incongruity in various situations can be explored and the element common to this increased sensitivity can be traced. And so on.

Intent is thus initiated. The mechanism to identify environmental regularity is set up in the organism by virtue of a neural representation of

<sup>9</sup> The prediction can be made from this formulation that only when a reinforcing event occurs at a perceptibly different level or in a perceptibly different pattern from average prior occurrences will it guide subsequent behavior.

the prior recurrences of these invariances. Whenever this representation includes regularities in the organism's own behavior and the outcome of that behavior, identification of the outcomes of actions can become possible—i.e., intentions can be initiated. Flexibility with respect to the representational process that initiates these intentions appears related to the functions of basal forebrain structures.

### The Execution of Intentions

So much for the initiation of intentions. Now for their execution. A quotation serves as an appropriate introduction to the relationship between the initiants of intention and their execution, between the behavioral operant and willed action. In his chapter on Lewinian theory Hilgard states that—

... this is not unlike Skinner's later notion of the role of the discriminative stimulus in operant behavior: the stimulus does not elicit the behavior, but it does set the occasion for it. The sequence of events from perception [attention] to satiation [habituation] is as follows. The perception of an object or event may give rise to a psychological tension or it may communicate with a state of tension already existing, in such a way that this tension system thereupon assumes control over motor behavior. The aroused "valences" act as environmental forces steering subsequent behavior. This behavior then leads to satiation or to the resolution of tension so that a state of equilibrium is approached [14, p. 212].

As a result of the recently reported experiments already summarized, a somewhat more neuropsychological statement can be made to describe this sequence. When an organism observes a novel event, an orienting response occurs. This initiates or modifies (Lewin says "gives rise to or communicates with") a representational process in the brain of the organism (Lewin terms this "psychological tension"). In a task situation, this representation comes to include the organism's own behavior and its outcome ("assumes control over motor behavior," according to Lewin). Any deviation from recurrent behavioral regularities and invariant outcomes signals error. (Lewin speaks of "aroused valences that steer subsequent behavior.") The resultant modification of the behavior with respect to the representational process leads to habituation ("satiation," "the resolution of tension," in Lewin's words).

Some further notion as to what occurs as the representational process assumes control over behavior can be obtained from additional neurobehavioral analysis of the fixed-interval operant. As already noted, performance in this situation is reflected not only in the over-all rate of reward and response, but also in the distributions of responses across the interval between reinforcing events. Obviously, neither the over-all rate

of response nor the scallop is a measure of the *accuracy* with which the behavior meets the contingency of the situation—only rate of reward measures this. Other descriptive notions must be employed to cover differences in response rate and distribution. For instance, a conservative approach would characteristically lead to a lower over-all rate of response, whereas a rapid banging-away-at-the-bar might be more fun. Both would be equally effective, as measured by the total number of rewards obtained during the experimental session. When changes in food deprivation alter the response rate, accuracy of response to the situation is not affected. Habituation takes place, i.e., the neural representational process is altered except in animals that have had amygdectomy. One could describe the events as follows: Food deprivation disposes the animal to behave less conservatively; this shows up, in the experimental as well as in other situations, in heightened generalized reactivity. But note that the distribution of responses across the interval remains unaltered and the animals invariably get their due. Though amygdectomy helped with the analysis of habituation, some other neural mechanism must be sought if the relation between the representational process and the regulation of response *distribution* is to be understood. Study of the scallop should give clues about the manner in which the representational process steers the organism's actions. The scallop is, in a way, an externalization in behavior of the mechanism by which the organism's neural representation of the situation and his actions in that situation are connected—*viz*: an externalization of his intentions with respect to the situation.

**Experiment 5 [35].** The procedure of this experiment was identical to that in Experiments 1 and 2, except that the monkeys were always fed regularly every 24 hr. Twelve monkeys were used—three received bilateral frontal eugranular cortex removals; three others were given control lesions that consisted of bilateral resection of the inferior portion of the temporal isocortex; the remaining six monkeys served as unoperated controls.

The results shown in Fig. 3 reveal no differences between the over-all rates of response between groups as a result of the experimental procedures. On the other hand, the frontal lesion selectively alters one aspect of the behavior—the scallop. That is, the crescendo-like distribution of responses across the interval is flattened in this group of monkeys, whereas no such effect is obtained in the two control groups. This effect of frontal ablation on fixed-interval behavior is thus considerably different from the effects of starvation and of amygdectomy.

*Interpretation of experiment 5.* In view of the fact that the frontally lesioned animals invariably and selectively press the lever when the reinforcing occasion demands, the change in their behavior cannot be attributed to a loss in their ability to make temporal discriminations—they continue to identify the occasion when reward is to appear. The

neural representation of the problem situation seems adequate. So to what can the alteration in behavior be ascribed?

Those who have used operant conditioning techniques have emphasized that very different patterns of responding are elicited in the operant situation by different schedules of the occasions for reinforcement. But this is not all. Equally important is the observation that a variety of manipulations, especially manipulations of the organism by means of drugs and brain stimulations and ablations, result in *different* response patterns in the *same* situation. To take this into account, the operant, just as the gambling situation, can best be looked at as a task or a game. The rules that describe the situation are programed as schedules of discriminative and reinforcing stimuli. But nothing happens unless the organism works at the task or plays the game. He thus must acquire sufficient know-how in the situation to meet the schedules and to *make* the events happen with some recurrent regularity. He must actively develop a strategy to guide his behavior and, as already noted, a variety of strategies can be used to meet the rules of the situation. Thus one must infer not only that a neural representation of the rules of the situation is set up in the organism, but that, as habituation to the task or game occurs, processes for guiding behavior with respect to the representation become available to the organism. In short, a variety of successful performances is possible; they have properties that can be described and measured; and further, the several varieties are not

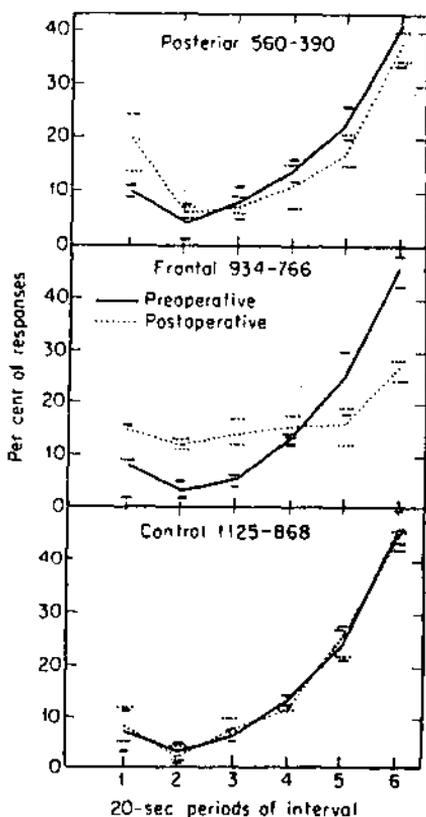


FIG. 3. Graph showing the change in distribution on monkeys' response rate following frontal intrinsic-sector ablation (three monkeys). Note that the distribution of rate over the interval is not affected in the controls (four monkeys) and after posterior intrinsic-sector ablations, (three monkeys). Also note that the total rate of response did not increase; rather, rate was somewhat decreased, probably due to the ad-libitum feeding period which all groups were given prior to operation—approximately two weeks before postoperative testing. (Compare with Fig. 2 and see legend to that figure.)

uniquely specified by the rules that describe the situation. A process separate from that used to identify the rules must therefore operate to determine the varieties. "Response chaining" has been suggested to fill this need but offers little more than a descriptive label of the observed behavior and does not get at the regularities in the differences between response chains. What is necessary is a description of the processes that map the neural representation into action. These processes have been called "plans"—the strategies and tactics that connect a neural representation of the invariant properties of a situation with action [28]. Whereas the error-sensing, congruity-incongruity processes determine the organism's identifications of the properties of his environment, plans determine his choice among alternative actions appropriate to these identifications.

The results of Experiment 5 can thus be readily interpreted. Bilateral ablation of the frontal eugranular cortex alters the crescendo distribution of responses. However, it leaves intact both the over-all rate of response and the accuracy of responding with respect to the occasions for reward. Therefore, the effects of the lesion cannot be attributed either to an impairment in the build-up of the neural representational process nor to an impairment of its modifiability. The effect must then be on those other processes that connect the neural representation with action—plans. A wealth of other neuropsychological data can now be properly gathered to support the propriety of this interpretation.

Clinical neurologists have taken for granted that removals of the precentral motor cortex affect both willed and skilled actions [8]. Will and skill can, on the basis of neuropsychological evidence and analysis, be given separate operational definitions. Intention and volition will refer to processes whose properties are largely determined by plans. Skills, on the other hand, refer to processes whose properties are much more intimately meshed into identifications of the environmental situation and especially to the outcomes of action. In the case of skills, the regularities in behavior sequences are sensitive to patterns and schedules of environmental contingencies. This is essentially the same as the classical view of operant conditioning. In the case of will, however, behavioral regularities cannot be ascribed to the rules, but are attributed to the development by the organism of a variety of intentions or strategies to meet the rules. The results of Experiment 5 suggest that interference with intention, or will thus defined, is a function of the anterior frontal eugranular and not of the precentral agranular isocortex. The results of other experiments have also shown that, in spite of resulting clumsiness, the sequence of actions used in opening a latch box is not impaired by precentral-motor-cortex ablations whereas these sequences are disrupted by anterior frontal lesions [16, 17, 39]. The prediction can therefore be ventured and tested that anterior frontal lesions would, and precentral lesions would not,

affect the scallop—the distribution of responses in the fixed-interval situation. And such a result would strengthen the body of evidence—obtained from introspection, from ordinary social communication about the problem, and from social and clinical observation and experimentation—that will and skill can be usefully distinguished.

The contribution of neurobehavioral experiments can thus be placed in proper perspective. Such experiments—as in the examples shown—are often useful in defining issues in the behavioral sciences. The data obtained bridge the gap between the realms of discourse used in social and psychological—yes, even philosophical and humanistic—communications and those used in the biological and physical sciences. And in addition, a great deal of understanding is obtained about how the brain functions to regulate behavior.

The novelty of recently attained understanding generates enthusiasm, and therefore some overstatements and overgeneralizations occasionally are made. (In spite of caution, these may have crept into the preceding pages, for it is a new and provocative story that is told.) And whenever such errors in judgment result, the scientific process (of error sensing) counters the excesses—and scientists, through disillusionment, hard<sup>a</sup> work, and *critical* analysis, restore balance between their neural representations and the state of their art. The following sections deal with these equally important but somewhat less substantive problems of the relation between psychology and neurological disciplines.

## INTERRELATIONS AMONG METHODS

### Fact and Fantasy

One of the major deterrents to precision in interdisciplinary investigations is confusion between data and theory. Both fact (from the Latin *factum*, to make) and theory are scientific constructions based on the observation and manipulation of variables. Facts can be distinguished as being either theoretically neutral or systematic. The distinction is, however, as can be seen from the pursuit of the example detailed in the section *On the Neurology of Intention and Will*, a complicated one. In the early phases of a science, experimentally produced relations—data—are readily communicable as such. However, as the data-gathering process proceeds, a short-cut terminology begins to dominate communication: *neuron, reflex, inhibition, synapse, PTO cortex, association areas, limbic systems* on the neurological side; *SD, cue, reinforcement, fixed-interval schedule, stimulus, expectation, action, utility, outcome* on the behavioral side. To the group conversant in the terminology—usually the group that has created it—each word or phrase denotes a specific body of knowl-

edge; there is often very little theory involved. But when the base of communication is broadened, differences in the connotative meaning of the phrases arise, since the communicants are not all equally conversant with the data denoted by the phrases. As long as the terminology is denotative, i.e., short-cut for data, the difficulties in communication are surmountable. Explication using longer descriptions, pictorial and graphic material, and direct demonstration can be resorted to. When, however, in addition to the short-cut jargon of the laboratory, intervening constructs are postulated and these postulations or hypotheses are referred to by short-cut phrases (e.g., "habit strength," "drive," "excitatory potential") that become intermingled with those denoting data, communication, though not impossible, becomes cumbersome. Paradoxically, this lack of true communication is often glossed over by a communicative effort that appears to succeed—the translation of the phrases that refer to one system of hypotheses into the phraseology that refers to another such system (e.g., Hullian into Freudian). Such translations, many times removed from data, provide a feeling of satisfaction much as does autistic thinking; unfortunately, little of enduring value is accomplished by either.

Thus, one distinction between theoretically neutral and systematic variables can be made. Whenever laboratory argot refers to data (the relation between independent and dependent variables) one might say that language is theoretically neutral; when intervening variables, and especially systems of such variables are referred to, then the language is certainly not theoretically neutral. But this is not the whole story. All words have connotative as well as denotative meanings. Most words are chosen by the scientist, intentionally or not, because of the body of knowledge a phrase connotes, as well as for the appropriateness to the data to be denoted. Operational definitions overcome this difficulty as long as the communicative base is restricted. Such definitions fail to convey meaning when the defining operations are far beyond the acquaintance of the communicants. In these instances, and they are the ones of interest in a discussion of the interrelationships between sciences, *the phrases used denotatively in one science become constructs to the investigator in another science.* The behaviorist can either accept or reject the neurologically derived concept of a "hypothalamus" or a "neuron"; the neurologist can either accept or reject the behaviorally constructed conception of "reinforcement," or "expectation." Neither the psychologist nor the neurologist can modify, on the basis of data, the total conception put forth by his colleague. I believe, therefore, that these conceptions are not, per se, theoretically neutral; neutrality is restricted to the occasions when those who use the concepts are intimately acquainted with the data the conceptions denote.

A final step, by way of a relating experiment, is necessary before the facts of one science become more than hypothetical constructs for other sciences. Once this is accomplished, phrases, though they denote systematic variables, again become theoretically apparently neutral to the communicants involved. "Willed action" or "voluntary movement," already discussed, and "visual field" are cases in point. The concept—visual field—originates introspectively (as it is differentiated from one's visual world) [12], but is in the main derived from behavioral data. The visual field is that extent of the environment to which an organism can respond with one eye without moving that eye. The phrase "visual field" stands for the systematization of (1) extent of environment as independently specified by physical techniques, e.g., measuring arc; (2) fixation of vision—having an organism look at one point but attend to the surround of that point; (3) make a verbal or instrumental discriminative response that is to represent seeing, i.e., looking and attending; and (4) in the final analysis, the phenomena subsumed under "I see." Further, the clinician uses details of the specification of the critical organismic variables that determine the visual field to good purpose. For example, a bitemporal hemianopia suggests a chiasmatic lesion. One hardly stops to ponder the theoretical neutrality or lack thereof of the psychological term *vision* or the physical term *field*. There are sufficient data denoted by phrases such as "arc," "lens," "eye," "optic nerve," "chiasm," "optic tract," "geniculostriate system," to make the term *visual field* theoretically neutral, and, to all intents and purposes, fact. This neutrality is shaken, however, when new groups of investigators gather new data and choose to apply this supposedly neutral term to denote their data. Thus, recent discoveries regarding the effects of extrageniculostriate cerebral lesions on the discriminations from which visual fields are inferred have led to confusions [5, 30]. These confusions will not be resolved until new limits are accepted—limits that specify which operations usefully define the concept of visual field.

### Data-gathering Methods

The interrelations between psychology and the neurological sciences can be conceived in another fashion. Interrelationships of method are again basic to the development of any reductive discipline. But methods are of two sorts: (1) data gathering, and (2) systematic. So we are back to earlier discussions. In order to be of immediate relevance, data must be so gathered that both neurological and behavioral concepts can be constructed from them. In other words, neural and behavioral variables (as well as environmental, of course) determine the data.

When methods are used to study only the behavior of the total organism in its environment, the resulting data have only an indirect bearing

on neuropsychological formulation—judicious guesses perhaps dignified as hypothetical constructs. Such guesses provide considerable satisfaction to large numbers of behavioral scientists. Of course, when neurobehavioral data become sufficient (as in the determination of the visual field) precise inferences regarding the neural variables can be made from the behavioral data and these can, in turn, lead to further empirical neurological explorations. As already noted, clinical neurology and neurosurgery utilize this method to good advantage. But neurobehavioral data are rarely available in such quantity to the behavioral scientist—thus the necessity for neurobehavioral experiment.

The neurological clinician and scientist usually faces a somewhat different problem. Neurobehavioral data are available to him, but they are imprecise. Both the neural and the behavioral variables have, as a rule, been only grossly specified. Lesions are in the "front" or in the "back" parts of the brain; consciousness is perhaps "altered"; intelligence is "impaired." Refinements are proceeding in several clinics and their laboratories. Progress is in part dependent on increasing the precision of introspectively specified concepts by improving the instrumental tasks used in the neuropsychological analyses—recent advances in this regard have been rapid [25, 29, 31, 47, 49].

Some examples may serve to highlight a few of these empirical problems associated with interdisciplinary research. The assessment, control, and manipulation of neural variables in neurobehavioral experiments often lead to heated discussion. Proponents of electrical stimulation decry the limitations of lesion and ablation techniques, and vice versa. The specific value of the neural-ablation technique is that a relatively stable preparation results. Such stability with respect to the neural variables allows, *par excellence*, the exploration of the experiential and immediate environmental variables that also determine behavior. Stimulation, electrical and chemical, of nervous tissue provides a different asset—relative reversibility of the process. This reversibility allows repeated controls to check the reliability of the phenomenon observed. However, each laboratory and clinical procedure is restrictive in one way or another—laws and concepts are attained *only* from judicious combinations of the available procedures. For the behavioral as for the physical sciences, experiments in which obvious limitations are imposed often provide the key to understanding. The laws of mechanics were possible only after experiments were accomplished in which the behavior of a sphere or an inclined plane could be observed—watching birds in flight could initiate the problem, not solve it. Neuropsychological laws are more likely to result from experiments in which the behavior of brain-lesioned monkeys is observed in an operant conditioning or a discrete trial-discrimination apparatus than from watching people with normal brains behave in unstructured situations.

Since neural function in behavior is the prime interest of the neuro-psychologist, the body of data must include the results of an extensive exploration of relevant environmental variables. In the past, failure to realize this has led to so-called "inconsistencies of neurobehavioral fact." As recently as a decade ago, the notion was prevalent that neurobehavioral data, both animal and human, were destined to be totally unreliable. Results of experiments and observations were difficult to replicate. The history of frontal lobotomy and the now-famous story of the amygdalotomized Baltimore cats [4] versus the Washington cats [42]

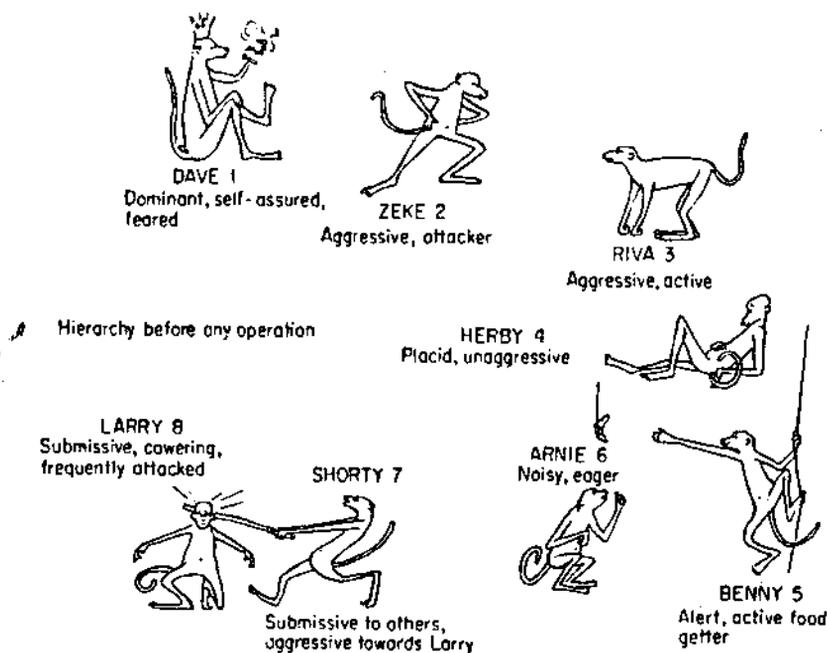


FIG. 4. Dominance hierarchy of a colony of eight preadolescent male rhesus monkeys before any surgical intervention.

are cases in point. Amygdalotomy resulted in rage in one situation, in taming in the other; in hyposexuality in the hands of the Hopkins investigators, in hypersexuality when the Walter Reed group studied their animals. The cat controversy—as most of the others—has yielded to greater precision in method. The recently reported Hollywood cats [9] show that when adequate control of *both* the environmental and the organismic variables is accomplished and a sufficient behavioral repertoire is explored, discrepancies all but vanish: The behavior of amygdalotomized cats is that of normal cats in home territory; after surgery cats can no longer make the distinction between home and foreign territories.

Another example of the clarification of the diversity of the effects of brain operations on social behavior is illustrated in Figs. 4, 5, 6, and

7 [33]. As can be seen, the effects of amygdalotomy are as much a consequence of the immediately postoperative dominance situation as they are the consequence of locus of the lesion.

Such are some examples of the special ambiguities that brain ablation and clinical neurological lesion studies encounter unless precautions are undertaken to ensure precision. These special problems have been dealt with at length in an earlier paper [33]. It is sufficient to note here

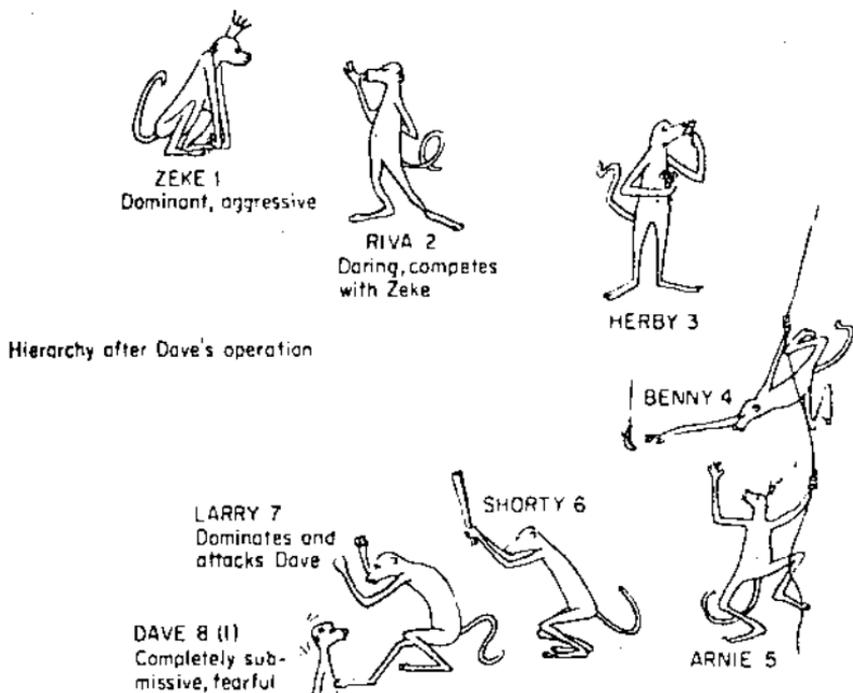


FIG. 5. Same as Fig. 4 after bilateral amygdalotomy had been performed on Dave. Note his drop to the bottom of the hierarchy.

that *properly* used, satisfying precision can be attained today from these much maligned surgically and pathologically produced data from which stem practically all of our knowledge about the relations between brain and behavior.

### Bridging Laws and Methods of Systematizing

But methods are not all observational and experimental. Analysis and treatment of data are as important as data gathering. Neurologists especially have been prone to consider precision in data analysis to be unnecessarily statistical or a matter of semantics. After all, does it really matter whether we say "afferent" or "sensory"; "efferent" or "motor"?

Afferent and efferent refer to neural data; motor and sensory refer to behavioral data. The terms began to lose their distinction as a result of the overgeneralization of the law of Bell and Magendie. The result—considerable confusion and, in addition, suppression of findings such as direct afferents reaching the precentral motor cortex; eye movements obtained from stimulation of the occipital visual area [27, 34].

Of equal importance is the resistance of some psychologists to an attempt at rigorous formulation of psychological concepts. Protests that one is defining the problem out of existence are voiced when an attempt

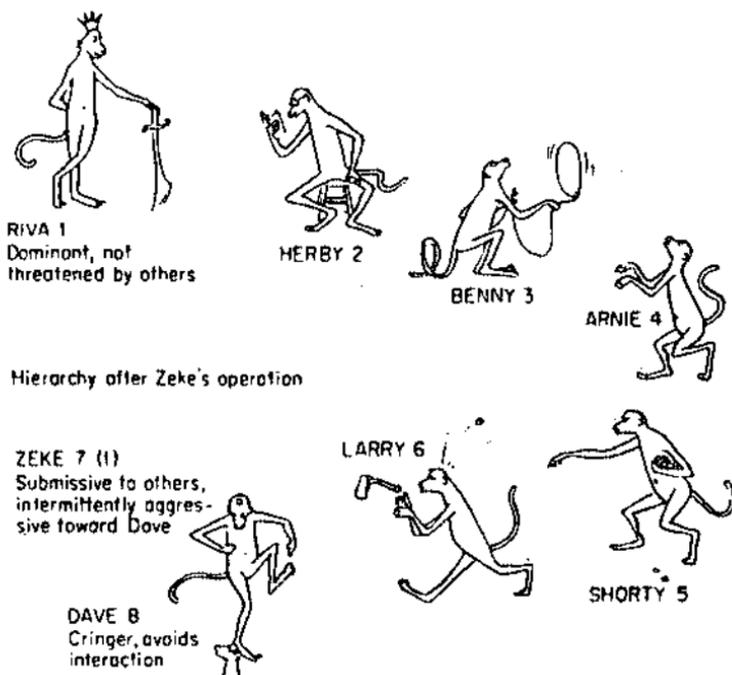


FIG. 6. Same as Figs. 4 and 5, except that both Dave and Zeke have received bilateral amygdalectomies.

is made to give operational definitions of concepts such as those of will, intention, reinforcement, anxiety, or stimulus, even when the difficulty of the problem becomes apparent only after such attempts at definition [22, 23, 36, 48]. In a similar manner, there is often heard an outcry against the use of precise models—physical or mathematical. The accusation runs that analogical thinking is fuzzy thinking, but this is not necessarily so. According to the view taken here, all behaviorally derived concepts are in the final analysis (or the initial one, or both) analogically compared with introspectively derived concepts. The properties of the

one are in some ways similar and in some ways different from those of the other. The more precise the conceptual tools, the more precise can be the comparison. But comparison it is, whether or not it is precise. The visual process can be specified in terms of images or in terms of transformations on environmental events. In one case, the model is apt to be a camera; in the other, systems of electromechanical devices or mathematical equations. Models are practically always used (implicitly

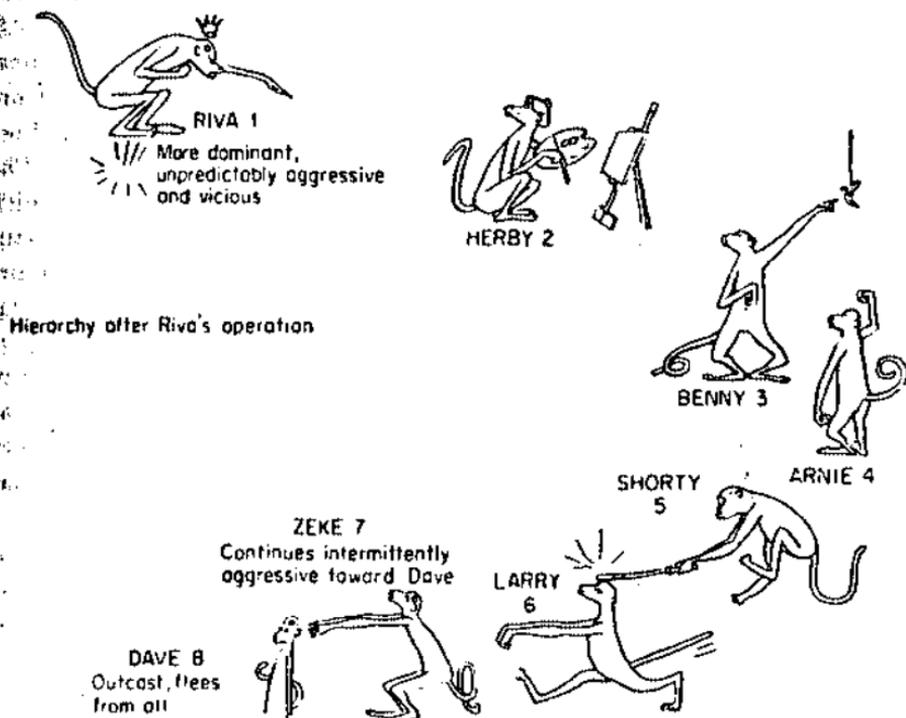


FIG. 7. Final social hierarchy after Dave, Zeke, and Riva have all had bilateral amygdalotomies. Minimal differences in extent and locus of the resections do not correlate with differences in the behavioral results. Herby's nonaggressive "personality" in the second position of the hierarchy accounts for the disparate effects of similar lesions.

or explicitly) to subsume any fairly extensive body of data; at different stages of the science, different levels of precision in models are possible and useful in generating testable hypotheses. In some areas, neuropsychology is ready for precise models, e.g., vision [12], reinforcement [36]; in other areas somewhat less rigorously formulated models are more useful, e.g., intention, affect.

But one caution must be voiced. Models are not laws, though ideally they are based on the determination of lawful relationships between variables. When models are constructed from systematizations of hypo-

tical constructs or intervening variables, they are apt to lose power, since almost any set of phenomena seemingly can be explained if a sufficient number of hypotheses are admitted. This was the error of the early mathematical biophysicists, and of the Hullians, and currently it may be the flaw in the arguments of the statistical-learning theorists. Truly useful models make analogies between sets of lawful empirical relationships, not between the hypotheses derived from such relationships. Such models are hard to come by, since a great deal of empirical work must precede them.

And, of course, though some kind of systematization of data is necessary, not all systematization need be formally theoretical. Aside from supporting data, spans of communal interest in problems shared by those working on either side of the neuropsychological chasm may suffice. The complexity of the systems studied in the behavioral sciences (of which neuropsychology is one) makes possible the collection of a vast amount of data, which, if irrelevant to any kind of systematization, is lost because information transmission becomes too costly. But the systematization can be of two sorts: relevance to a practical problem (e.g., psychosurgery) or to a theoretical formulation (e.g., information-measurement theory). These two approaches to systematization are not as mutually exclusive as some of the proponents of one as against the other will have us think. (Psychosurgery alters information processing by patients; models of information processing are used by computer engineers.) Only the scientist's temperament raises the issue of incompatibility. My own preference is that proposed by Shaw in his *Don Juan*: when bored with heavenly theory, the problems of the flesh are refreshing; when, in turn, these pall, a return to the upper reaches of abstraction is welcome.

### A Psychological, Subjective Behaviorism

This flexible, though precise, approach to the mind-brain issue has implications that transcend neuropsychology per se [28]. Once a truly monistic (yet paradoxically pluralistic) approach to *simultaneous* interdisciplinary study at *several* levels is attained, some old problems can be seen in a new light—experiments can be designed and patients can be approached in ways up to now plagued by confusion. Briefly, the approach here taken can be summarized as follows: All scientific inquiry begins with introspection. A first step toward precision is made when these introspections can be verbalized or in some other way communicated to others. The philosophy of science is concerned with such consensual validations [41]. The job of psychologists is to give precision to one aspect of the scientific universe of discourse. Briefly, the proposal

accepted begins much as does that of classical behaviorism. Any concepts derived from behavioral data irrespective of their terminological cloak are considered to be psychological concepts. Such concepts are of two sorts: those derived from propositional statements about introspections, and those derived from instrumental behavior or from the nonpropositional aspects of verbal statements. The extreme behaviorists have, as a rule, been made uncomfortable if the psychological concepts derived from these disparate sources of behavioral observation are treated as identical. And there is good reason for the discomfort, since at least two levels of discourse are involved. Yet psychology is of a piece. How is a unitary body of knowledge to be attained? The position taken here is that concepts derived from instrumental and like behavior are to be juxtaposed to those derived from statements about introspections. Then similarities and differences are to be ascertained. Two extremes must be avoided in this process. When the behaviorally derived concepts, because of a lack of empirical evidence, are indistinguishable from those derived from introspection, confusion results. When the two classes of concepts are so distinct that no relation between them is recognizable, the behaviorally derived concept is apt to be trivial.

This problem of collating concepts derived from observed data with those introspectively derived is logically the obverse in the psychological and the physical sciences. An apparent handicap to the solution of the problem for psychology is that psychological concepts do not have the attribute of projection. Whereas a table appears to be "out there," my perception of it appears to be "in here." But this very attribute has been blamed for the difficulties encountered in the philosophy of the physical world. So perhaps psychological philosophy has the easier task at that.

According to this view, the philosopher and the psychologist have the common task of relating observational and experimental data to phenomenological data. The philosopher of science, aided by the burgeoning developments in linguistics, is especially concerned with the verbal analysis of language as this describes phenomenal interrelations. The behavioral psychologist on the other hand, is especially interested in instrumental and other nonverbal behavior and in the nonlinguistic uses of verbal behavior. As already noted, concepts that result from observations of behavior are likely to be trivial unless they are initially close to those derived from introspection. Even the most rigorous behavioristic psychology must not be completely sundered from the phenomena which originally gave birth to psychological inquiry. However, when the behaviorally derived concepts are not distinguishable from the phenomenological, an objective psychology is thwarted.

Thus the distinction must be gradually attained. Some introspectively conceived phenomenon must be tentatively externalized by the sugges-

tion that an observed behavioral process is representative of the phenomenon. For instance, the discomfort which one recurrently feels and which leads to eating behavior is labeled as "hunger." When its characteristics are more clearly specified, "appetite" may be the more appropriate label. The eating characteristics of other organisms may be observed and these also found to be cyclical. The cycles can be accentuated by manipulations of the accessibility of food and the specific characteristics examined by the manipulations of the accessibility of different types of edible objects.

This externalization of the introspectively derived concept can then be approached in a variety of ways. Deprivation variables, neural variables, endocrine variables, situational variables, and somatic variables can be studied as these affect the eating process. And gradually lawful relationships between the consuming of food and some of these variables can be noted. Furthermore, obstacles can be placed in the path of consuming behavior. And again, the interaction of the various classes of variables with each other can be lawfully described. During this series of experiments some of these lawful relationships may be identified with hunger. For instance, it may be suggested that the number of hours of deprivation correlates specifically with the state of the organism which is introspectively defined as hunger. But when this identification is made, it very quickly becomes inadequate. For, introspectively, the hunger mechanism is cyclic and hours of deprivation do not correlate linearly with what is felt.

Experiments are again undertaken to demonstrate this added dimension of the phenomenon. On the other hand, the laboratory may show, as it actually has, that there is not a one-to-one correlation between how much an organism will eat when food is easily accessible and how much work that organism will do to overcome an obstacle in order to eat. This kind of observation can then be referred back to introspection and differences can be distinguished between hunger and appetite, between zest for food and zest for search, and between search and a tendency to complete actions once they are initiated. Each of these in turn is only vaguely specified by the introspection. Some of the distinctions may be erroneous; others incorrectly drawn. Again, externalization into the laboratory can sharpen the distinctions.

Gradually a more and more precise description and empirical analysis of the behavioral process can be attained and a scientific, multilevel understanding of the conception under scrutiny follows this ever-widening and deepening spiral of definition and empirical evidence. In this stepwise process, the reductive empirical and experimental analysis of the mind-brain problem has an important place.

What are the limitations of this approach? The most significant limi-

tation is that introspectively derived concepts and those derived from instrumental behavior are never expected to match completely. This may lead to considerable dissatisfaction on the part of those who need to have their world neat and tidy. But the discordance between what people say and what they do and what people do and what animals do, is one of the facts of life of the behavioral sciences. This discordance cannot be explained away by verbal magic. Nor is the classical dualistic frame more than a giving-up. For aside from the paucity of empirically precise data soundly systematized, the most important deterrent to fruitful inquiry into the relation between psychology and neurology has been philosophic dualism. Such dualism has taken two especially pernicious forms, extreme psychophysical parallelism and pseudomonism. Many eminent philosophers, neurologists, and neurophysiologists, and some psychologists, have settled into a comfortable journey along the parallel rails of the physical and the psychical. Rails that are wishfully believed to extend to some future infinity called "correspondence." Pseudomonists on the other hand, whether they be extreme materialists or extreme phenomenologists fail to face the issue squarely. By volubly denying the alternatives, pseudomonists in fact accept them by default. Neurological concepts are founded in neural data, psychological concepts in behavioral data. Words by themselves cannot transform the one into the other. Experimental and observational evidence is needed before useful translations between the results of systematizations of neural and of behavioral data are accomplished.

If either of these dualistic approaches is accepted, the bridge between the behavioral sciences and humanities on the one hand, and the physical and biological sciences on the other, can never be constructed. As a result, both worlds will be impoverished because of lack of communication between them. Recognition that the problem, though difficult, can be approached and solutions approximated, though never completely attained, should give rise to a new way of scientific inquiry. If the methods of early classical physics as we see them from this distance are slavishly followed, then *rapprochement* between science and man's humanistic endeavors can never be attained. The proposal set forth in a recent publication [28] is that the scientist has often followed and should explicitly follow a path hitherto reserved for the artist: i.e., enactment and reenactment of his internalized representation of the world. Analysis and systematization have an important part in enactment but they are not the whole story. Explication by the judicious use of highly perfected technical analogy is admitted. Already this has occurred in the efforts of the physical and mathematical model builders. When these analogies are specifically labeled for what they are and a continuous check between model and systematically analyzed data is made, the process—far from

being pernicious—is the only hope for a unified universe of discourse among all of man's endeavors. This approach includes systematizations with respect to models, with respect to theories, with respect to urgent problems such as those provided by the clinic. The horizons hitherto reserved for the poet and the humanist become the frontier of behavioral science. And at the core of these endeavors lie such multilevel reductive disciplines as neuropsychology. Without giving up precision, a wider range of problems is thus admitted to the scientific enterprise. This approach, dubbed psychological or subjective behaviorism, reaffirms that a recently neglected subject matter for a scientific and experimental psychology is after all appropriate—the study of man by man.

#### WHERE THE NEUROPSYCHOLOGIST?

Unless the reductive scientist is especially wary, his problem-oriented discipline can become dangerously superficial. The hard work of methodological mastery is doubly his. Techniques must be acquired wherever the problems lead him—and, as is already evident, the problems lead him far. The tools of data gathering are not always easy to acquire. To obtain data relevant to a particular problem from operant equipment is at times a frustrating task. To have an animal die for lack of surgical judgment or tender postoperative care after that animal has been subjected to many months of rigorous behavioral testing can be a devastating experience. Or, having accomplished successfully the data-gathering process, the statistical, mathematical, and logical tools may not be at hand to make maximum use of the findings. Where are the neurologists who today are sufficiently acquainted with linguistics or with statistical behavioristics to interpret within a broader scientific frame the uniquely available opportunities to study language disturbances? Where is the psychologist who knows enough about the brain to discuss the mechanisms of the *creation* of information—information which he so glibly communicates and so noisily discriminates? Certainly competent neuropsychologists are not to be found in abundance. The reasons for this are several, not the least of which is the lag in institutional acceptance of new areas of scientific development.

Efforts are being made by many institutions to provide a place for neuropsychologists to work and teach. Departments of psychology at universities are showing a new interest in physiological psychology. Departments of anatomy and of physiology more and more frequently find their neuroanatomists and neurophysiologists engaged in neurobehavioral work. Departments of pharmacology and the pharmaceutical houses are increasingly staffed by scientists of this new discipline. But in the spot where he is most needed, the neuropsychologist is as yet practically un-

known. Departments of psychiatry in medical schools and psychiatric institutes are slow to recognize the advantages that the new discipline offers. The historical fact that psychiatry stems from faculties of medicine, while psychology stems from faculties of philosophy has estranged the medical discipline from its basic sciences. One might expect clinical psychology to provide the necessary impetus toward the development of a *rapprochement*. But that this has not happened is fact; just as it is fact that pathology has had only a limited effect in providing such *rapprochement* in medicine and surgery. Neuropsychology is concerned with medical physiology and chemistry; neuropsychology is a behavioral science. The neuropsychologist is at home as much with the effects of altering the chemical structure of an antihistamine to produce chlorpromazine as he is with the concept of role. He is as much at home with the organization of the ventrobasal nuclear complex of the mammalian thalamus as he is with the process of consensual validation. Does today's psychiatric training have comparable reach? How else is the trainee to cope with the new developments in his field which come from the social, the intrapersonal, and the neurochemical directions? The impressive advances in the caliber of medical training during the past half century have been associated with the interest generated in the basic sciences concerned with the medical disciplines. Psychiatry is sure to profit from the example [6]. Now that the most ardent psychoanalysts are beginning to remember that Freud first formulated his conceptions from neurobehavioral as well as from strictly behavioral evidence [19], now that the organicists have been sufficiently exposed to the importance of experiential and other socioenvironmental factors so as not to ignore them completely, perhaps neuropsychiatry is ready to welcome and give adequate berth to one of its most essential basic disciplines—neuropsychology.

But irrespective of where he finds himself—and the locations may be many—the neuropsychologist has an interesting task ahead. And his is perhaps the most important in this latter half of the twentieth century: to help establish the dignity of man as a scientific as well as a political tenet.

In our own culture the cleavage of the "spiritual" from the "natural," which is a survival from the most ancient mythologies, has fostered popular ideologies of religious fanaticism, class rivalries, and political antagonisms that are biologically unfit and even suicidal because they result in social disintegration. Our ultimate survival is endangered as long as ideological fantasies that are incompatible with things as they are control individual and national patterns of behavior. We must somehow manage to heal this artificial dismemberment of the human personality before we can hope for a permanent cure of the present disorder [c. JUDSON HERRICK, *The evolution of human nature*, pp. 416-420].

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