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Neurological Knowledge and Complex Behaviors

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Some scholars believe that Cognitive Science is the attempt to achieve in artificial systems what has already been achieved in the brain. Others, by contrast, argue that the study of ideal adaptive mechanisms could go on without reference to the brain. The author points out that the brain may not be the ideal cognitive device because of biological limitations on its capacity. Although it may not be ideal, it is still of major interest to cognitive scientists because of the great interest in the human mind, and, in addition, because at this moment the brain is the most important single reservoir of adaptive mechanisms.

The paper discusses several areas of neuroscience which are likely to shed light on mechanisms of adaptation. The study of simple nervous systems is likely to reveal important design principles of cognitive devices. The study of complex nervous systems will, of course, exert a major influence. The study of such systems leads to certain general principles concerning the neural circuits involved in complex adaptive behaviors: (1) There exist innate specialized systems for the learning of many specific behaviors that at first might appear to be purely cultural. (2) There is no evidence for the existence of any all-purpose computer in the brain. (3) There are many surprising dissociations manifested by the specialized systems in the brain, e.g., a special system for recognition of faces as against other visual patterns. (4) The study of the nervous system enables one to formulate more precise mechanisms for the role of emotion in cognitive function. (5) Some human behaviors can probably be understood poorly or not at all if the neural substrate is not considered. (6) The cognitive systems designed for dealing with attentional processes may be among the most complex neural structures which underlie behavior.

There are certain other properties of brains that should be kept in mind by the cognitive scientists who are studying human behavior: (1) Brains differ from each other in structure, and it is very likely that the strategy for the solution of certain problems differs from person to person. (2) The presence of certain innate cognitive strategies in the brain may prevent that organism from employing other strategies which might be optimal for other problems. (3) Any theory about a particular cognitive strategy will have implications as to the particular mode of breakdown that might be expected after brain lesions. The study of disordered function from brain disease may thus be a valuable way to test certain cognitive theories.

In the months preceding this conference I made a point of asking many of my colleagues for their concept of Cognitive Science. The answers I received were, as one might well expect, remarkably varied, even from some who identified themselves as cognitive scientists. Some argued that it was a new field, whereas one respondent insisted that although he could not define it, the area could not be new since, whatever it was, Helmholtz *must* have practiced it. I soon realized, however, and this realization was, so to speak, punctuated by Professor Simon's elegant opening address, that the novelty of the field was irrelevant. What was important was the novelty of the growing realization that numerous apparently disparate disciplines shared certain concerns in common.

I was equally interested in the response to my second question: If Cognitive Science existed, what did knowledge of the workings of the brain have to do with it? Again the answers were varied. Reading the proceedings of this symposium one finds a similar range of views. There were some who thought that the brain was totally irrelevant, or, worse yet, that the study of nervous mechanisms could only add naive conclusions to befuddle the unwary. Flowers are made of chemicals, but biochemistry exists perfectly happily with the barest nod to floriculture. The brain may be a machine designed for cognitive functions but is hardly the place to study basic principles.

There are, by contrast, others who consciously or otherwise place the brain at the very center of the field. Some would say that ideal cognitive systems are indeed realized—or realizable—in the brain. Others, more extreme, would argue that Cognitive Science will in the end be nothing more than the end product of neurophysiology.

I will, on reflection, accept none of these extreme views, not out of a desire to compromise or from some notion of tolerant intellectual liberalism or distaste for controversy, but simply because none of these arguments convinces me. I hope to make my position clear in the following pages.

If I may borrow from Herbert Simon's presentation, let us accept that Cognitive Science is, above all, a field which concerns itself with strategies of adaptation, particularly to varying and unexpected environmental circumstances.

The realization that the conceptual structure cuts across so many areas should perhaps make us all realize that inspiration may come from areas that appear at first glance to be totally remote. This new society is concerned with psychology, mathematics, computer science, linguistics, philosophy, the social sciences, and, of course, the neurological sciences. But before I deal with the possible role of neurological studies in this hybrid field, let me point out at least two biological fields whose data and theories may in fact have important lessons for Cognitive Science and its concern with systems that can adapt to changes in the environment, and in particular to unexpected changes. How does one design a system to do this? Perhaps the most dramatic example of this type of design is embodied in the immunological systems of the body. How is it that these systems adapt with the most remarkably subtle strategies to environmental alterations that could apparently have never entered into the design of these systems, e.g., immune responses to recently invented chemical structures? There are indeed highly individual responses to thousands of such antigenic substances synthesized only in the past few years. Conversely, many infectious agents, particularly the so-called slow viruses, maintain their infectivity only by ingenious mechanisms which depend precisely on the environments to which they have been subjected. Indeed, immunologists have called attention to the formal similarity of learning and immunological mechanisms.

Another field in which equally remarkable adaptive mechanisms are at play is that of genetics and more broadly the whole process of evolution. Indeed, the existence of genetic codes (I use the plural since we now know that there is no universal code) itself reveals the importance of symbolic systems in biology.

Yet, having pointed out that many biological systems embody cognitive scientific principles, let me hasten to add that I doubt that "*the* brain" is the ideal cognitive device. Indeed, when we say "*the*" brain we imply that there is *only one* model. But we know that brains differ and that some may be far more effective adaptive models than others. Indeed, the problem is further complicated by the fact that some brains adapt well to a certain group of environmental changes but poorly to another group, whereas in other brains the reverse situation may hold. One might even consider the evolution of the brain as a slow progress toward an ideal but nonexistent device.

Why is the brain not ideal? We can only guess. In some instances better paradigms for cognitive tasks have simply never been "wired into" the system by appropriate mutations. But even when superior paradigms are available, not all brains may contain them. The reason for this is probably dependent in major part on the fact that there are many constraints on the brain. It is limited in size at birth by the requirement that it should not exceed the maximum possible diameter of the birth canal. It is limited in final size because it already uses a quarter of the cardiac output. I also suspect that even at its current size not all areas can operate maximally since this would involve increases of blood flow beyond the capacity of the heart in a subject using the rest of his body actively.

The compromises that are made presumably trade superior design in one area for inferior design (one that has too few nerve cells even with the best principles) in another. This distribution of inborn potentials for talent will differ from brain to brain.

If this is true then the brain may be the *best* device in existence (even this is probably not true in all areas), but it is not the *ideal* device. There is no reason why better devices cannot and will not be built as this field advances. Furthermore, as Duncan Luce has pointed out there is no reason to assume that the best man-made devices will be similar in design to the best human brains, any more than an airplane's flight is similar to a bird's.

If that is so, then why should the cognitive scientist study the brain and not just the mind? The answer is given away in the very structure of the question.

Most cognitive scientists are strongly interested in the human mind, i.e., precisely in the output of a particular accidental device. The field has not yet become a purely mathematical one, but continues to find its major inspiration in behavior. The brain may not be the ideal device, but it is almost certainly at this moment far and away the greatest single reservoir of such adaptive mechanisms. The device comes equipped with hundreds of built-in clever devices designed to maintain certain invariants. Thus, despite a wide range of environmental states, body temperature, food intake, and blood pressure are remarkably constant. Body weight is normally held within an astonishingly narrow range, despite the fact that the machine has no built-in method of directly ascertaining its own weight. Yet these adjustments are only the beginning: It has a remarkable capacity to learn, to store knowledge of changes in the environment, to store knowledge of changes in the strategies followed by others, and to acquire new strategies. Evolution has been a highly effective, if slow, strategy for the development of adaptive cognitive devices, and most of the best ones extant are being carried around in human heads. We are still the best machines for pattern recognition, classification, and for the very talent of design of clever devices.

Despite all this, the position of neural science in relation to cognition remains an uneasy one. Indeed, in this very symposium it is very sparsely represented. And whereas one of the original possible state-of-the-art symposia was to be in the neurosciences, this did not materialize. Some will say that this is because neural science has not delivered the goods. Thus the pioneering attempts of McCulloch and Pitts to correlate detailed neural circuity with function did not succeed although the more modest attempts of Lettvin and Maturana and Hubel and Wiesel are dramatic examples that such correlates are possible. Our knowledge of the circuity of mammalian brains is extensive, but astonishingly imprecise in the small details. To make this clear let me point out that there is not a single neuron in the central nervous system of any *mammal* whose connections are fully specified, although such neurons are known in some invertebrates. Despite this fact, the neural sciences and Cognitive Science will perforce be partners. In my view, many of the problems of interfacing the neural sciences with the cognitive sciences are more sociological than scientific, i.e., professional xenophobia and infatuation with one's own discipline are the greatest barriers to adaptation. The average neurological researcher is not likely to be enamored of philosophy, discursive thinking, psychological analysis, or computer models. Conversely, many nonneural cognitive scientists are uncomfortable with the data obtained with the microelectrode from a few neurons or from the derangements in cognitive strategy which follow damage to the nervous system.

Let me now turn to a few hopefully more precise indications of how neurological knowledge can contribute to this growing field. Let me briefly indicate an area remote from my own field of major study—that of the study of behavior in relatively simple invertebrate nervous systems. I heard John von

Neumann in one of his last lectures point out that there existed invertebrates whose nervous systems contained far fewer units than were present in the primitive calculating machines (as they were still called) of that period. Yet, von Neumann added, these primitive animals were in his view far more complex than these calculating devices, more complex in that he thought it would take more axioms to describe their behavior. Von Neumann's message was clear: There are design principles in these simple nervous systems which will enormously expand our capacities to understand adaptive complexity.

The advantage of these simple systems is obvious. They contain small numbers of nerve cells; and furthermore the complete connectivity of many of these cells can be specified and the adaptive intenuity of many of these organisms is often striking. The possibilities for breeding of genetic variants opens the possibility of experimental tests of theories of mechanisms of strategies of adaptation. At this moment this field may appear to be far from the major concerns of cognitive science, but my prediction is that it will increasingly be found to be surprisingly relevant.

Let me now turn to some areas nearer to my own central interests. Simon has said in his presentation that when we think we are studying the properties of the nervous system we really may be studying the effects of past history. A little reflection, however, shows that the opposite situation has been equally frequent—when we thought we were studying the effects of experience we were really studying the properties of the nervous system. The adult chimpanzee usually responds to the sight of a snake with fear. The interpretation that immediately comes to mind is that this fear was instilled during development. Yet Hebb showed many years ago that a baby chimpanzee responds with the same brisk fear to his first sight of a snake.

It would in fact be surprising if it were not the case that the brain had many built-in properties. The need for many built-in systems is one of the lessons that the history of the attempts to build intelligent devices, which Simon has so elegantly summarized, has taught cognitive scientists: Any system which has to learn everything it needs is simply too slow and inefficient. The acquisition of motor skills is an excellent example. Children may take years to tie their shoe laces and even the novice in surgery often takes a remarkably long time to learn to tie knots in sutures. There are, by contrast, complex motor patterns like those of walking that are primarily inborn and are acquired at high speed. In fact, the maturation of an inborn process is often mistaken for learning.

Furthermore, there are some experiments that appear to show learning effects which are, in fact, not really present. Consider the following: Two sets of kittens are raised in the laboratory. In one group the right and left eyes are patched on alternate days so that at no time is the use of both eyes permitted. These animals are compared to controls without eye patches. At six months it is found that in the first group of animals there are few cells in the visual cortex which respond to binocular stimulation, while in the control group there are many. The apparently obvious conclusion is that the animal who has had binocular stimulation has acquired binocular vision. The elegant experiments of Hubel and Wiesel reveal a different sequence. Cells which respond to binocular stimulation are present in the newborn animal. They are *lost* only if binocular stimulation is not supplied over the following months.

There is of course an experiental effect, but it is the *loss* of a built-in program, rather than the acquisition of a new one. But in some cases it is clear that even the apparent loss of a built-in program is illusory. Consider what happens when for the first time a rat is put into the cage of a cat raised in the laboratory. If the cat has in the past seen rats attacked by other cats, he will almost invariably attack. What happens, however, if the cat has never seen a rat (and has of course never seen one attacked)? As shown by several experiments (e.g., Kuo, 1930; Yerkes & Bloomfield, 1910), about half of the cats do not attack. The remainder, however, do attack. Furthermore, the attack is not random. The cats will, in fact, attack in the same manner as the animal in the wild. They will bite at the neck or turn the rat over so as to bite at the throat.

It is clear therefore that built-in systems for attack are present in the cat, or at least in many cats. There are at least three interpretations as to why certain cats do not attack. The first is that these animals do not have built-in systems and would therefore have had to learn to attack. The second is that innate systems for attack were present but were lost as a result of lack of early experience. The third is that the built-in programs are present but inaccessible. As we will now see the third interpretation is probably the correct one.

John Flynn of the Yale Medical School has published a dramatic series of experiments on cats who do not attack spontaneously (e.g., Wasman & Flynn, 1962). The animal, with a rat in its cage, receives stimulation through electrodes implanted in the brain. In most locations this stimulation is without effect. If, however, the electrode is in certain locations, e.g., the lateral hypothalamus, the cat will attack. The manner of attack is again similar to that of the wild animal. The attack is specific in its goal, so that the cat does not attack small objects placed in the cage. Furthermore, although a dead stuffed rat is attacked, the attack is less persistent than on a live animal.

These experiments show that the innate program for attack is still present in these animals. In addition, there must be innate perceptual programs which contain models of the anatomy of a rat which determine the specificity not only of the object of attack, but of portions of that object.

We must conclude that there are elaborate preprogrammed systems of analysis and action which can be modified or triggered by experience. Furthermore, even the capacity to learn itself reflects specializations of neural organization: Whether or not the chimpanzee has language, it is clear that the human learns it better.

The ability to learn is probably not a general one. Instead, there exists systems which have a specialized capacity to learn certain kinds of material. Thus Maureen Dennis of Toronto has studied children with the Sturge-Weber syndrome, a congenital disturbance of the cortex of one side of the brain. Several such children have undergone the total removal of this abnormal cortex from one side of the brain because of uncontrollable seizures. Since these children have all had brain lesions existing from before birth they tolerate these operations very well, and the types of gross language disorders seen in adults do not occur. Yet even in these cases with very early lesions, Dennis has shown clearly that those who have undergone left decortication employ different strategies in certain linguistic tasks from those of children with early right decortications whose strategies are much closer to those of normal controls.

As I have already indicated, we do not yet have detailed knowledge of neural circuits involved in complex adaptive behaviors. I will, however, list certain general principles illustrated by specific examples. Most of these are based on information from the study of humans who have suffered delimited lesions to their nervous system and been well studied, although some of these principles are based on experimentation in nonhuman animals.

1. As already noted, *there exist specialized systems for the learning of many behaviors that at first might appear to be purely cultural.* Thus we have known for over 100 years that there are specialized systems for language. We also know that there are systems which deal with recognition of special stimuli, e.g., faces (i.e., systems which are specialized and not designed for the recognition of other types of visual pattern), and for the expression and recognition of emotion.

2. *There is no evidence for the existence of any all-purpose computer.* Instead, there seems to be a multiplicity of systems for highly special tasks, e.g., the systems for facial recognition appear to be separate from those for other visual recognition tasks. Systems for the recognition and production of music appear to be separate from those for language. There is, at present, no good evidence—although counter-evidence is perhaps equally weak—for the existence of a generalized logical deductive capacity.

3. *There are many surprising dissociations,* some of which have already been indicated. There are many others. Thus Japanese patients with reading difficulties acquired as the result of delimited brain damage typically have more difficulty comprehending words written in the apparently simpler syllabary system (*kana*) than in the more elaborate logographic (or ideographic) system (*kanji*) (Sasanuma & Fujimura, 1971). In some Europeans with reading difficulties the category of written words that is best preserved is that of names of public buildings such as *bank*, *restaurant*, or *post office*. It is obvious in this case that there is some remarkable interaction between damage to the hardware and past experience. In this case it appears as if the patient has learned to read words which are attached to their referents (e.g., the words "bank" or "hotel" are usually on the appropriate buildings) in a different or additional way from those which are not (e.g., "telephone," "plate," "if").

4. The study of the nervous system enables one to formulate more precise

mechanisms for *the role of emotion in cognitive functions*. I cannot elaborate this in detail here, but I would first point out that the portions of the brain involved in memory functions, e.g., the hippocampus, amygdala, mammillary bodies, etc., are all portions of the limbic system which is clearly involved in emotional activities. Furthermore, MacDonnell and Flynn's (1966) work on aggression in cats again shows that stimulation of specific limbic structures "sets" the animal for increased receptiveness to certain inputs and preparedness for certain outputs. One of the striking effects of many brain lesions is the alteration of these emotional components while rational, calculating functioning may be spared.

5. *Some behaviors can probably be understood poorly or not at all if the neural substrate is not considered*. Thus patients with certain lesions (who suffer from what are called *apraxic* disorders) will show certain remarkable dissociations (Geschwind, 1975). They will fail to carry out certain verbal commands which they understand, whereas they succeed in carrying out others. These patients will fail to salute, to pretend to use a hammer, or throw a ball. They will also fail to pretend how to blow out a match or suck through a straw. On the other hand, they will successfully carry out commands to move the eyes, to stand, to walk, bow, or turn. How do we account for the successful ability to carry out certain movements to verbal command while others fail? Let me point out briefly that the commands for movement which succeed do *not* differ from those which fail in linguistic structure, age of acquisition, or in any measure of complexity. The movements which succeed are those which are mediated by motor systems in the brain other than the contralateral pyramidal system. In other words, there are cognitive systems the understanding of which may depend on knowledge of the specific pattern of realization in the "hardware" of the nervous system.

6. The *most complex* cognitive systems in most brains may be those which deal with *attentional* processes. Simon has stressed the serial features of attention, but I would stress that the parallel features are equally important. An attentional system functions properly only if it combines a central focus with a continuous survey of what is not at the center. The work of Broadbent has illustrated this very well.

Let me now describe some other properties of brains that must be kept in mind by the cognitive scientist who is studying human behavior.

1. There is a tendency to ask "What is *the* strategy by which a cognitive problem is solved?" But since we know that brains differ from each other in structure, it is very likely that the strategies for solution of certain problems differ from person to person. Thus patients in whom the corpus callosum is congenitally absent show a marked increase in the size of the anterior commissure. We also know that the brains of left-handers differ on the average from those of right-handers. Thus left-handers have, on the average, less anatomical asymmetry in the brain than right-handers (Galaburda et al., 1978).

2. It is furthermore conceivable that the presence of certain innate cogni-

tive strategies in a brain may prevent that brain from employing other strategies which might be optimal for other problems. This is a situation that evolutionary theory would lead us to expect with considerable frequency. It is even conceivable that disorders such as childhood dyslexia may in some cases be the result of the high development of certain perceptual strategies at the expense of others.

3. Any theory about a cognitive strategy will probably have certain implications as to the particular modes or breakdown that might be expected after brain lesions. This in turn implies that study of disordered cognitive function from brain disease may be a valuable way to test cognitive theories.

Let me close with a prediction—and a plea. I believe that the neurological sciences and the study of syndromes of delimited neurological damage will have a continuing major effect on the future development of Cognitive Science. It is well worth recalling that the profound effect of neurology on psychology which has occurred in the past quarter century was not expected by the majority of psychologists. But this influence will not be unilateral. Indeed, I will not be surprised if the influence of cognitive science on the neurological sciences is not greater than the reverse.

Let me now state my plea that the practitioners of both fields take each other seriously. I state this not as the customary exhortation to cooperate, but as the result of the knowledge that the failure not merely to take account of certain neurological data, but even their active suppression, led to the dramatic neglect of important cognitive findings. Thus many of the effects of the destruction of the corpus callosum were elaborately described and well understood theoretically before the First World War. Their neglect was the direct result of the influence of major figures in the history of cognitive psychology such as Karl Lashley, Henry Head, and Kurt Goldstein (Geschwind, 1964). It is tragic that it took until the 1960s to correct this gross error. It would not be fitting for a science whose object is the study of adaptive cognitive strategies to make an equally maladaptive error.

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