Preface

The current volumes of *The Hippocampus* reflect the prodigious amount of work aimed at discovering the functions of this structure over the past decade. The hippocampus ideally lends itself to two types of study: (1) because of its regular and relatively simple cytoarchitecture, it can serve as a model for cortical processing in general, and (2) because of its size and central location, the role of the hippocampus in the total ecology of brain function poses an important challenge.

We attempted to divide the contributions to Volumes 3 and 4 according to these two types of experimental aims. As always, however, when one makes dichotomies, one finds them inadequate in treating certain data and our attempt is no exception. There are contributions that do not fit the classification and there are others which fit both.

In addition, there are manuscripts which we wanted to include but which the authors were not ready to submit at this time. Larry Squire, Mortimer Mishkin, and others are making important contributions which do not appear in these volumes except among references throughout. But this was also the case for Volumes 1 and 2 where we sorely miss the irreplaceable contributions of James Olds, Ross Adey, and Brenda Milner.

Volume 4 is concerned primarily with the role of the hippocampus in the ecology of the brain in regulating behavior and experience. The contributions have the potential to raise the level of our understanding considerably. However, to do so we must clearly differentiate the "levels" of processing which are addressed and come to some deeper recognition of the meaning of the terms used to describe the behavioral tests which are being used. Thus, for example, the term *hippocampus* is used throughout this volume to refer to the hippocampal formation which includes most of the hippocampal gyrus. Closer examination of chapters such as those of Jarrard (Chapter 4) and of Mahut and Moss (Chapter 8), however, indicates that several of the more pervasive effects of "hippocampectomy" are due to damage to one of the components of the hippocampal formation, the subiculum, and cannot be attributed to removal of the "hippocampus," *per se*.

Not only is the hippocampal formation a multiform structure, but it is part of a brain which has a variety of mechanisms available to solve any

specific set of problems. Thus, as detailed by Mahut and Moss (Chapter 8), performing hippocampectomy in infants and in adults has different effects, a difference which they attribute to the effects of experience since the infant lesions have more severe consequences. The important conclusion they reach is that one must be wary since extrahippocampal processes can mask the effects of hippocampal resections.

In this introduction my effort is aimed at integrating conceptualizations derived from the experiments detailed in the various chapters and at indicating the points at which integration fails. Of course, these points of failure furnish guides for undertaking further research. This approach is the one taken by Gray and Rawlins (Chapter 6) for their own theoretical frames in their chapter, which therefore serves as a model for what I believe to be a useful exercise in coming to grips with the contributions of this volume.

A good place to begin is to scan the titles of the contributions for words that hold the key to what is contained in each chapter: "executive function," "modulation," "gating," "comparator and buffer memory," "memory for temporal context," "memory for unique instances," "recombinant processing". Two distinct themes emerge from this scan. One theme concentrates on the type of processing, the other on the type of memory affected by hippocampal manipulations. These two themes converge when "memory" is interpreted as "remembering," a retrieval of an appropriately coded event. At the behavioral level, executive functions and recombinant processing are compatible with mechanisms at the neural level such as a comparator and a gate. These concepts are, in turn, compatible with an intermediate level of modeling such as that involved in buffer memory, memory for temporal context, and for unique instances.

At the same time a gate and a comparator are not identical even though they are related concepts. Nor is temporal (or spatial, as suggested by O'Keefe and Nadel, 1978) context necessarily a unique instance. Which of these distinguishably different conceptualizations converge, which can be eliminated because it does not cover the entire range of data?

To begin with the model presented by Gray and Rawlins, I have already extensively reviewed the convergence of Gray's "anxiety" interpretation of the data with the "effort" interpretation presented by Pribram and McGuinness (1982) and in my chapter in this volume (Chapter 11) the essentials of this convergence are summarized. What then of Gray's and Rawlins' finding that hippocampectomized rats have difficulty whenever there is "temporal discontiguity" in the task irrespective of whether that task is a test of "working" or of "reference" memory? My reaction to this statement is that something has occurred in the enterprise in which rats are being tested that some gross distortion of interpretation has taken place in the definition of working and reference memory. As we initially defined the distinction in *Plans and the Structure of Behavior* (Miller *et al.*, 1960) and as Honig and Olton have subsequently refined that distinction, temporal discontiguity *is* the

hallmark of working memory: "In some learning situations, animals need to remember an event, or a set of events, to perform efficiently during a given trial. They also need to terminate the memory of these events in order to perform well on a later trial." (Honig, p. 4, in Hulse *et al.*, 1978).

It is this definition of working memory which fits the delayed alternation task used by both Olton, in rat research, and originally by Nissen, in primate studies. Nissen defined the task as involving "one trial learning" (Nissen *et al.*, 1938). More recently Mishkin has noted that such tasks involve "trial unique learning" (Mishkin and Petri, 1984), and Gerbrandt, Pico, and Ivy make an excellent case (Chapter 10) that indeed it is memory for unique instances that is affected by hippocampal manipulation.

What then of Rawlins' observations? Can one misjudge a task as involving what appears to be reference memory? In primate research such a task is the delayed response problem, especially its indirect form, which is usually called delayed matching from sample. Such a task involves temporal discontiguity, although it is often labeled as a discrimination problem. But what if the match is repeated, or in the alternation problem, if instead of single alternation, double, triple, quadruple alternations are presented? A discontinuity (between working and reference memory processing?) develops for normal subjects somewhere between three and five repetitions (Pribram, 1961). In short, one must be extremely careful in attributing one or another type of memory process to the performance of a task until one has performed a parametric study that shows the limits (the discontinuities with respect to the task to which it is being contrasted) over which the inference holds.

I would urge that the concept of working memory does cover the facts that Rawlins presents and, to go even further, on the basis of evidence such as that presented in Chapter 10 by Gerbrandt, Pico, and Ivy to suggest that these facts can also be subsumed under the rubric "episodic memory" which Tulving (1972) has so ably defined on the the basis of studies with humans. If this is the case, then the definition of working memory must be modified: What seems to have to be forgotten in order to perform adequately on a subsequent trial must not really be forgotten but, as it were, put on a back burner for future reference. After all, monkeys and rats probably as well, do form learning sets with respect to delayed response and alternation problems. These sets are those within which the particular trial is faced and response is performed. There are thus two aspects to the tasks which are under consideration: a trial unique aspect and an aspect which forms the context within which the trial unique performance occurs. The term "working memory" applies to the trial unique aspects of the performance and the term "episodic memory" to the contextual aspects.

I have for a number of years contrasted the polysensory, contextual amnesias resulting from frontolimbic lesions with sensory-specific agnosias, i.e., deficits in reference memory (Pribram 1954, 1958*a,b*, 1966, 1972*a,b*, 1984*a,b*). In such a scheme there is a correspondence between reference and semantic memory, the latter being a human derivative from the former.

What such a view also entails is that episodic memory ought to form the context within which semantic memory develops. I do not know of any existence that this is so, nor, however, do I know of any evidence against such a view.

The idea that the trial unique aspects of a process operate within the context of a particular episode is not new to neuropsychology. Neurologists have, since the days of Jackson (1925) dealt with these same concepts under the aegis of "novelty and familiarity". Seizures emanating from a focus centered on the amygdala and the uncus of the hippocampus are often accompanied by *déja* and *jamais vus* phenomena, i.e., inappropriate feelings of familiarity and novelty. It is easy to see, in these terms, the validity of Gray's suggestion that a comparator mechanism must underlie hippocampal function in memory—something is novel only in the context of (i.e., in comparison to) the familiar.

By recognizing the similarity between the concepts of working and episodic memory on the one hand and novelty and familiarity on the other, an important additional insight is gained, which applies to the formulations put forward by Mahut and Moss (Mahut, 1985; Chapter 8, this volume) and by Mishkin (Mishkin and Petri, 1984). These investigators have placed great stock in the task devised by Gaffan (1974) which is essentially an animal version of a human "recognition" task. In such a task the subject is exposed to a set of sensory images and then is exposed to them a second time when they are interspersed with another set to which he/she has not been exposed. The subject is asked to state whether he/she has observed each of the images on a previous occasion. In neurological parlance one would ask whether the image was a novel one or whether it was familiar on the basis of previous exposure.

This is to be contrasted with what neurologists call a test of recognition. As Freud and Henry Head (see Head, 1920) defined the term, it meant the identification of an image or object in terms of its use or external relationships (an ostensive definition, which, by the terms used above, would be classified as reference memory). We are thus faced with the bizarre situation in which neurologists find that resections of the posterior cortical convexity produce agnosias, deficits in recognition, while experimental psychologists find that resections of the medial temporal lobe and, (since the same deficits are found after anterior frontal damage) of the frontal lobe, as well, produce deficits in recognition memory. The confusion is compounded when the term memory is restricted to these sorts of (novelty/familiarity) processes and referencing knowledge (which in its failure is an agnosia) is attributed to "habit". Is the inference to be made that habits and skills do not involve memory? Or, as I suggest in Chapter 11, is there some more subtle unexpressed insight here that we should restrict the term memory to experience which involve reflection, at least a feeling of novelty or familiarity?

In any case, when these definitional issues are taken into account, it is clear that a simple memory consolidation model of hippocampal function

will not do unless there are two entirely different consolidation mechanisms, one for working/episodic memory and the other for reference/skill memory. Further, as detailed in Chapter 11, the evidence shows that it is the amygdala and related systems, not the hippocampus, which is involved in consolidation and registration of a novel event. At the same time, another process, akin to that involved in habituation and in extinction, is dependent on hippocampal functioning. This also can be thought of as a "consolidation" process: a storing (repression?) of nonreinforced, negative, frustrating experiences. Consolidation is no more a unitary process than is learning or remembering.

Once it is understood that "recognition memory" deals with novelty and familiarity and that, as I have indicated in my chapter, there is a convergence of data between those working on consolidation and our results on the effects of amygdalectomy on "registering" a novel input, the neural mechanism in which the hippocampus is involved becomes clear. The outlines of such a mechanism were provided in the summary chapter of Volume 2 of this series (Pribram and Isaacson, 1975). At the behavioral level the consequences of the operation of this mechanism are as follows: To register a novel input is a first step toward familiarity. When that input fails to be accompanied by a reinforcing consequence (which includes punishment but not frustration) maintaining interest, a second process akin to habituation and extinction ensues by virtue of the hippocampal mechanism. When another event occurs, its neural effects are compared with those remaining as a result of the habituated, extinguished process. Thus, familiarity becomes the context to which a current input becomes compared. It is declared familiary if there is a match, novel if there is a mismatch, much in the way Sokolov (1963), whose work inspired ours, described the process.

What is not at all clear is why Gaffan, Mishkin, Hirsch, Mahut, and others have relegated what Douglas and I called nonlimbic learning (1966) to an associative process that results in perceptual and motor skills (habits). The functions of the systems of the posterior cortical convexity are involved not only in processes which become habitual and skilled but they are also involved in the identification of the meaning of events and objects, that is, in cognition. It is in these systems that, as noted above, traditional neurology and neuropsychology has invested with cognitive processing. The dimension novelty/familiarity is more personal and more intimately related to emotion and motivation (see Pribram 1971) than to cognition *per se*. Furthermore, so-called associative processes, association by contiguity, does not exist in the central nervous system as such. A current event becomes associated to a previous one only if it fits into some context which has been formed by the previous event (Pribram 1963; 1971; 1980). It is thus more likely that the mechanism inferred by Gray as a comparator is "associative" than is the one served by the systems of the posterior cortical convexity.

Finally, Mahut and Moss (Chapter 8), as have Mishkin (1984) and I (1977), noted the role which the basal ganglia might play in this panoply of processes. All of the evidence suggests that the basal ganglia (of which the

amygdala is one) do not serve some unitary function. Rather their functions must be subdivided according to the cerebral systems to which they belong. This is not surprising since the cerebral cortex is an outmigration of cells from the basal ganglia; thus one would expect to find, as one does, that differences in the effects of cortical resections resemble those produced by resections of the related basal ganglion.

Thus, the inference that a comparator must be operating to relate a trial unique episode to a familiar context is amply supported. At the neural level the question arises as to what systems are involved in mediating this comparison. In Chapter 11, I suggest that the amygdala and the systems related to it, are involved in constructing the familiar context within which novelty becomes processed. The chapters by Vertes (Chapter 2) and by Gabriel, Sparenborg, and Stolar (Chapter 1) describe the brain systems (brainstem and thalamic) by way of which current input can be entered into this comparator mechanism. Isaacson, Springer, and Ryan detail the most likely neurochemical pathways involved (Chapter 5). Winson (Chapter 3) and Berger, Berry, and Thompson (Chapter 7) describe the behavioral dependencies that determine the operation of the comparator. And in Chapter 11, I detail the evidence that relates the contribution of the hippocampal system to the total information-redundancy processing competency of the brain.

In summary, I believe that the chapters of the volume indicate that considerable convergence between views can be achieved when the nomenclature they use is clarified, when data are carefully attended, and when some effort is made to heed what another laboratory has produced. I do not claim to have done this adequately in this short attempt. But I hope to have indicated that it is feasible. Of course, there will be sticking points, such as the possible relation, in humans, of episodic memory as a context within which semantic memory develops; such as the lack of parametric studies with rats, showing the limit that demarcates working from reference memory. But, of course, it is these very sticking points that we can discover by such an attempt to converge models and so have the opportunity to address by experiment.

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