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Précis of *Elements of episodic memory*

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Abstract: *Elements of episodic memory* (Tulving 1983b) consists of three parts. Part I argues for the distinction between episodic and semantic memory as functionally separate albeit closely interacting systems. It begins with a review of the 1972 essay on the topic (Tulving 1972) and its shortcomings, presents a somewhat more complete characterization of the two forms of memory than the one that was possible in 1972, and proceeds to discuss empirical and theoretical reasons for a tentative acceptance of the functional distinction between the two systems and its possible extensions. Part II describes a framework for the study of episodic memory, dubbed General Abstract Processing System (GAPS). The basic unit in such study is an act of remembering. It begins with the witnessing of an event and ends with recollective experience of the event, with related memory performance, or both. The framework specifies a number of components (elements) of the act of remembering and their interrelations, classified under two broad categories of encoding and retrieval. Part III discusses experimental research under the label of "synergistic ecphory." Ecphory is one of the central elements of retrieval; "synergistic" refers to the joint influence that the stored episodic information and the cognitively present retrieval information exert on the construction of the product of ecphory, the so-called ecphoric information. The concept of encoding specificity and the phenomenon of recognition failure of recallable words figure prominently in Part III. The final chapter of the book describes a model, named the synergistic ecphory model of retrieval, that relates qualitative characteristics of recollective experience and quantitative measures of memory performance in recall and recognition to the conjunction of episodic-memory traces and semantic-memory retrieval cues.

Keywords: amnesia; encoding; episodic memory; knowledge; memory; recall; recognition; recollection; retrieval; semantic memory

Part I: The episodic/semantic distinction[1]

Inchoate distinction

I wrote my 1972 chapter in reaction to papers by Rumelhart, Lindsay, and Norman (1972), Kintsch (1972), and Collins and Quillian (1972), that had been given at a conference at the University of Pittsburgh in March 1971. These authors were concerned with what I thought were the processes involved in the understanding of language, whereas they suggested that they were studying memory in a broader sense than had been the case in the past. They all used the term "semantic memory" to describe their work, borrowing the term from Quillian (1966). I thought that the extension of the concept of memory to comprehension of language, question answering, making of inferences, and other such cognitive skills was inappropriate. Inspired by Bergson (1911), Reiff and Scheerer (1959), and Munsat (1966), as well as by others who had discussed similar issues, I wrote the essay on the distinction between episodic and semantic memory.

Episodic memory, I suggested, is a system that receives and stores information about temporally dated episodes or events, and temporal–spatial relations among them. Semantic memory, on the other hand, "is the memory necessary for the use of language. It is a mental thesaurus, organized knowledge a person possesses about words and other verbal symbols, their meaning and referents, about relations among them, and about rules, formulas, and algorithms for the manipulation of the symbols, concepts, and relations" (Tulving 1972, p. 386).

I contrasted the two forms of memory with respect to five issues: (a) the nature of stored information; (b) autobiographical versus cognitive referents; (c) conditions and consequences of retrieval; (d) vulnerability to interference; and (e) interdependence of the two kinds of memory. I assumed that the two forms of memory were interdependent, interacting closely most of the time, each influencing the other in many situations. But I also thought that such interdependence was optional rather than obligatory: it was possible for a person to acquire knowledge about a particular dated cooccurrence of novel and meaningless stimulus events; similarly, it seemed reasonable to assume that mere cooccurrence of two stimuli or language units would not change the structure of semantic memory.

These different conceptualizations of a person's knowledge of an A–B "association" corresponded to the distinction between recollection of events and recall of facts, discussed in textbooks of memory (e.g., Boring, Langfeld & Weld 1948). But it deviated from the commonly accepted assumption that the learning of an A–B association, as in a paired-associate list, essentially consists of strengthening or updating the existing association between the two items of the kind revealed by free-association tests. Donald Thomson and I questioned the validity of this assumption, on the basis of experiments showing effects of context changes on recall and recognition of studied words (e.g., Thomson & Tulving 1970; Tulving & Thom-

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son 1971). We thought that word-recall experiments were concerned with subjects' remembering of events rather than with the establishment of new associations, or the strengthening of old ones, between transsituationally invariant units of semantic memory. The distinction between episodic and semantic memory naturally fitted into this picture.

The 1972 distinction was inchoate: rudimentary, imperfect, incomplete, and somewhat disorganized. The summary statement of the essay was correspondingly cautious. What I had done in the essay was to "present a case for the possible heuristic usefulness of a taxonomic distinction between episodic and semantic memory as two parallel and partially overlapping information processing systems" (Tulving 1972, p. 401). With the wisdom of hindsight it is easy to see the weaknesses and shortcomings of this distinction. Lack of relevant empirical evidence was the major problem, but there were others. One of these had to do with the absence of emphasis on the similarities of the two systems; another concerned the implied exhaustiveness of the primitive taxonomy implicit in the distinction; the third difficulty had to do with the names of the two systems; and perhaps the most serious problem involved the lack of clear and definite ideas regarding the relation between autobiographical episodes and what we might call their "contents." I had somewhat hastily classified a large majority of laboratory experiments on memory that had been done up to that time as experiments on episodic memory: In these experiments subjects were tested for their knowledge of what they had seen or heard at a particular time in a particular situation. It seemed self-evident that a subject would have to remember the event of seeing a particular word in a list in order to be able to recall the word when given episodic instructions. In light of both old and new relevant data, identifying the recall of the contents of an event with the remembering of the event appears to have been unwarranted, however.

Argument for differences

Episodic and semantic memory are two systems of *propositional* memory: Their function is to acquire, retain, and make available information that represents the reality external to the organism, information that can be expressed in the form of propositions. They can be contrasted with memory systems concerned with the acquisition and utilization of skills and procedures, or systems of procedural memory (cf. Winograd 1975b). Propositional and procedural memory systems differ in several ways: (a) Information handled by propositional memory systems has truth value, whereas that handled by procedural systems does not; (b) information retrieved from propositional memory can be contemplated introspectively or attended to internally, whereas procedural knowledge cannot; (c) propositional knowledge about something can be communicated to others in different ways through language or some other symbol system, whereas knowledge of a particular procedure can only be demonstrated through highly specific behavior; (d) propositional knowledge about something can frequently be acquired in a single act of perception or thought, whereas acquisition of skills and procedures usually requires intensive practice.

Thus, episodic and semantic memories are similar in that both are subdivisions of propositional memory. Despite this similarity, casual observations reveal a number of differences between episodic and semantic memory. These differences can be classified under three broad categories: differences in information, differences in operations, and differences in "applications," or the role that memory plays in a broad range of human affairs. A summary of these differences is given in Table 1.

Table 1. *Summary of differences between episodic and semantic memory*

Diagnostic feature	Episodic	Semantic
Information		
Source	Sensation	Comprehension
Units	Events; episodes	Facts; ideas; concepts
Organization	Temporal	Conceptual
Reference	Self	Universe
Veridicality	Personal belief	Social agreement
Operations		
Registration	Experiential	Symbolic
Temporal coding	Present; direct	Absent; indirect
Affect	More important	Less important
Inferential capability	Limited	Rich
Context dependency	More pronounced	Less pronounced
Vulnerability	Great	Small
Access	Deliberate	Automatic
Retrieval queries	Time? Place?	What?
Retrieval consequences	Change system	System unchanged
Retrieval mechanisms	Synergy	Unfolding
Recollective experience	Remembered past	Actualized knowledge
Retrieval report	Remember	Know
Developmental sequence	Late	Early
Childhood amnesia	Affected	Unaffected
Applications		
Education	Irrelevant	Relevant
General utility	Less useful	More useful
Artificial intelligence	Questionable	Excellent
Human intelligence	Unrelated	Related
Empirical evidence	Forgetting	Analysis of language
Laboratory tasks	Particular episodes	General knowledge
Legal testimony	Admissible; eyewitness	Inadmissible; expert
Amnesia	Involved	Not involved
Bicameral men	No	Yes

Source: Tulving 1983b, Table 3.1, p. 35.

Differences in information. The two systems differ in the immediate source of the information they handle. The mere sensation of a stimulus can serve as a source of information in the episodic system, whereas comprehension is necessary for the semantic system. The prototypical unit of information in episodic memory is an event or an episode. In semantic memory, there is no single "basic" unit, but facts, ideas, concepts, rules, propositions, schemata, scripts, and other related terms have been used by philosophers, psychologists, and cognitive scientists in discussing the nature of people's knowledge of the world. Organization of knowledge in the episodic system is temporal: One event precedes, cooccurs, or succeeds another in time. Lockhart, Craik, and Jacoby (1976) have even argued that "episodic memory has *no* inherent structure" (p. 82). The organization of knowledge in the semantic system, on the other hand, is defined by many relations that could be classified as "conceptual." The temporal organization of the episodic system is relatively loose, whereas the conceptual organization of semantic memory is tight (e.g., Estes 1976).

The information in the episodic system refers to or represents events in the rememberer's personal past, and may thereby provide a basis for defining an individual's personal identity (e.g., Greenwald 1981; Grice 1941; Shoemaker 1959). The knowledge recorded in the semantic system is timeless: It has no necessary connection to the knower's personal identity and instead refers to the world. Finally, the rememberer's belief in the veridicality of the remembered event is an inherent feature of episodic remembering and independent of testimony of others, whereas the belief in the veridicality of semantic knowledge is supported by social consensus.

Differences in operations. The episodic system registers immediate experiences, the semantic system registers knowledge conveyed by referential events and language. Only the episodic system can keep track of temporal order of occurrence of personal events; the semantic system has no capability of *direct* recording and maintenance of such information, although it can solve problems of the temporal order of events by inferences. The episodic system is relatively limited in inferential capability, whereas the semantic system possesses a rich inferential capability. Affect probably plays a more important role in the recording and retrieval of information in episodic than in semantic memory.

It is generally thought that the operation of the episodic system is more context-dependent than the operation of the semantic system (e.g., Ehrlich 1979; Kintsch 1980). Yet the question of whether episodic and semantic systems can be differentiated in terms of context dependency is a complex one: It is quite possible that the acquisition and utilization of our knowledge of the world is as context-dependent as is our episodic knowledge.

Information in the episodic system is more vulnerable to interference than that in the semantic system. The actualization of episodic information tends to be deliberate, frequently requiring conscious effort; that of the semantic system tends to be automatic. The general form of the retrieval query directed at the episodic system is, "What did you do at time T in place P?" In the semantic system it is, "What is X?" where X refers to an object, a situation, a property or characteristic, a relation, and so on. Retrieval from the episodic system tends to change (recode) the stored information; retrieval of information from semantic memory usually leaves its contents unchanged. Retrieval from the episodic system takes the form of a synergistic combination of the information stored in the episodic system and the information provided by the cognitive environment of the rememberer, interpreted in terms of the person's semantic knowledge. In semantic memory, on the other hand, retrieval entails a process in which the dispositional knowledge is actualized, or in which it "unfolds," in a manner determined by the nature and organization of the stored knowledge and relatively independently of the nature of the instigating cue.

Recollective episodic experiences are interpreted by rememberers as being a part of their personal past, whereas actualized semantic knowledge represents the impersonal present. People use the word "remember" when referring to personal recollections, and the word "know" when talking about actualized semantic knowledge.

Although some writers have suggested that semantic memory develops "out of" episodic memory (e.g., Anglin 1977; Kintsch 1974), a more plausible argument is that, in the development of a child, semantic memory precedes episodic memory (e.g., Kinsbourne & Wood 1975; Schachtel 1947). A related speculation holds that childhood amnesia is a phenomenon of episodic (autobiographical) rather than semantic memory (Schachtel 1947).

Differences in applications. Formal education is aimed at the acquisition, retention, and utilization of skills and knowledge that have to do with the world; episodic memory is irrelevant to the accomplishment of these aims. The general utility of semantic knowledge for an individual is greater than is the remembering of personal events.

The prospects of endowing computers with episodic memories that faithfully mimic their human counterparts are decidedly less favourable than the prospects of making computers efficient language users, question answerers, inference makers, or problem solvers (cf. Schank & Kolodner 1979). In definitions and assessment of human intelligence, semantic memory occupies a central position, whereas episodic memory is unrelated to intelligence (e.g., Sternberg & Detterman 1979). The relevance of the distinction between episodic and semantic memory to legal testimony can be expressed by saying that for the testimony of eyewitnesses to be acceptable, it must be based on episodic memory, whereas for that of expert witnesses to be admissible, it must be based on semantic memory.

In the study of memory, the phenomenon of forgetting – discrepancy between input and output – defines the basic focus of interest in episodic memory; forgetting is of no interest to students of semantic memory. Most of the work on semantic memory has to do with people's knowledge of language (e.g., Anderson & Bower 1973; Lachman, Schaffer & Hennrikus 1974; Meyer 1973; Miller 1969; Rubenstein, Garfield & Millikan 1970; Smith 1978; Smith, Shoben & Rips 1974); episodic memory research need not involve language. In the laboratory, episodic-memory tasks *require* retention of information from a particular episode, whereas performance on semantic tasks is guided by general knowledge.

A number of writers (e.g., Kinsbourne & Wood 1975; Rozin 1976; Wood, Ebert & Kinsbourne 1982) have suggested that amnesia resulting from brain damage is a condition in which episodic memory is selectively impaired while semantic memory is less affected.

Finally, Jaynes's (1976) theory of the evolution of consciousness implies that although bicameral men had perfectly developed semantic-memory capacities they were deficient in episodic memory: They "could not reminisce because they were not fully conscious" (Jaynes 1976, p. 371).

Debate about memory

Agreements. All students of memory seem to be willing to accept the distinction between episodic and semantic memory as a purely heuristic device that helps us to classify and describe experiments and observations. The heuristic use of the terms "episodic" and "semantic" aids communication and serves as a first step to deeper questions. We can describe different memory tasks as either episodic or semantic, and we can interpret, categorize, and organize outcomes of certain older experiments in terms of the difference between episodic and semantic memory without accepting the idea that the two represent different systems (e.g., Drachman & Leavitt 1972; Penfield & Perot 1963; Slamecka 1966). More recently, many researchers have related their own findings and observations to the episodic/semantic distinction in at least the heuristic sense (e.g., Caine, Ebert & Weingartner 1977; Gilhooly & Gilhooly 1979; Herrmann & McLaughlin 1973; Moeser 1976; 1977; Ojemann 1978; Petrey 1977; Russell & Beekhuis 1976; Underwood, Boruch & Malmi 1978).

There is also good agreement among theorists that "episodic" and "semantic" refer to different kinds of information. Anderson and Ross (1980), for instance, who reject the functional distinction between episodic and semantic memories, have no objection to the corresponding "content distinction" (p. 463). Similarly, there should be no disagreement regarding the separation between remembered episodes and their "semantic contents," and the possibility of answering questions directed at episodic memory on the basis of our general knowledge of the world. Finally, virtually everyone agrees that episodic and semantic memories are not only similar in many ways but also interact closely almost all the time.

Open questions. There are several identifiable matters on which disagreement does seem to exist at the present time: Probably the most basic issue of this kind concerns the problem of whether episodic and semantic memories represent different functional systems.

The position advocated in the book is that episodic and semantic memory are functionally distinct. This statement does not mean that the systems are completely separate, that they have nothing to do with one another, that there are no similarities between them, or that they serve completely nonoverlapping functions. It does mean that one system *can* operate independently of the other, although not necessarily as efficiently as it could with the support of the other intact system. It also means that the operations of one system could be globally enhanced without a similar effect on the operations of the other, and that the activity of one system could be suppressed without a comparable effect on the activity of the other. The functional difference also implies that in important ways the systems operate differently, that is, that their function is governed at least partially by different principles.

Some theorists (e.g., Craik 1979b; Jacoby & Craik 1979; Kintsch 1980; Naus & Halasz 1979) have argued that episodic and semantic memories constitute a continuum of some kind. Craik (1979b), for instance, has suggested that "the implied break between two memory systems is unsatisfactory," and that a better solution is the idea of "a continuum of representation, running from highly context-specific episodes at one extreme to abstract generalized knowledge at the other" (p. 451). In light of the currently available evidence, the idea of a continuum is not appealing.

Another open question concerns the status of lexical memory. Although many writers think of it as a part of semantic memory, or at least as a form of propositional memory (e.g., Collins & Loftus 1975; Kintsch 1980; Lachman 1973; Miller 1969; 1972; Schank 1975), and although the idea makes intuitive sense, it is possible to contemplate the hypothesis that lexical memory is a form of procedural memory that serves the function of transmitting and expressing episodic and semantic knowledge.

Empirical evidence

On the basis of findings of transfer from an episodic to a semantic task with reaction time as the dependent variable, Anderson and Ross (1980) argued against a functional basis for the distinction between episodic and semantic memory. However, since other experiments (e.g., Jacoby & Witherspoon 1982; Tulving, Schacter & Stark 1982) have shown that what is transferred from episodic input to semantic retrieval can be uncorrelated with episodic information, Anderson and Ross's findings cannot be regarded as highly relevant to the episodic/semantic distinction.

Herrmann and Harwood (1980) obtained data that they thought supported the distinction between the two systems, and so did Shoben, Wescourt, and Smith (1978). The latter study was based on the logic of double experimental dissociation. In the semantic task, subjects verified the truth of sentences, whereas in the episodic task they made recognition judgments about the same sentences. Independent variables were semantic relatedness among the sentences and "fanning," defined in terms of the number of propositions learned about a concept. The results showed double dissociation: Semantic verification was influenced by semantic relatedness but not by fanning, whereas recognition was influenced by fanning but not by relatedness.

Single experimental dissociations between episodic and semantic tasks have been demonstrated by McKoon and Ratcliff (1979), Jacoby and Dallas (1981), and Kihlstrom (1980). In McKoon and Ratcliff's paper the relevant data were provided by Experiments 1 and 4 in which response latencies were measured in a (semantic) lexical-decision task and an (episodic) recognition task, as a

Table 2. *Response latencies (s) in McKoon and Ratcliff* (1979)

Task	Relation of priming word to target		
	Episodic and semantic	Episodic	Semantic
Semantic Lexical decision	0.53	0.54	0.53
Episodic Recognition	0.57	0.62	0.74

Source: Tulving 1983b, Table 5.4, p. 88.

function of the relation between the target word and its preceding word. Their results are summarized in Table 2. These data show that the manipulation of the relation between the target and the preceding word in the series had no effect on lexical decision, but a sizable effect on the episodic-recognition task.

Jacoby and Dallas (1981, Exp. 1) compared subjects' performance in a semantic task (tachistoscopic identification of words) with that in an episodic task (recognition of previously studied words) as a function of the encoding operations performed on the target items in the first phase of the experiment. The results, expressed in terms of the probability of correct responses, are summarized in Table 3. These results show a clear dissociation between episodic and semantic tasks.

Kihlstrom (1980, Exp. 1) measured episodic free recall and semantic free association in different groups of subjects varying in hypnotic ability, in a situation in which subjects had learned the words and then were given posthypnotic suggestions to forget them. His data, summarized in Table 4, show that the effectiveness of posthypnotic amnesia suggestions varied greatly with the hypnotic ability of subjects in the episodic task but not at all in the semantic task. These data thus demonstrate a dissociation between episodic and semantic tasks in an experimental situation in which the independent variable was defined in terms of differences in brain states induced by hypnotic suggestions.

Wood, Taylor, Penny, and Stump (1980), in a well-controlled experiment, observed differences in regional cerebral blood flow between two groups of subjects, one engaging in an episodic, the other in a semantic memory task. They interpreted these results as suggesting "an anatomical basis for the distinction between episodic and semantic memory" (p. 113).

Table 3. *Response probabilities in Jacoby and Dallas* (1981, Exp. 1)

Task	First-phase encoding condition		
	Appearance	Sound	Meaning
Semantic Identification	0.80	0.81	0.82
Episodic Recognition	0.50	0.63	0.86

Source: Tulving 1983b, Table 5.5, p. 89.

Table 4. *Response probabilities in Kihlstrom* (1980)

Task	Hypnotizability of subjects		
	Very high	High	Medium and low
Semantic Free association	0.61	0.50	0.53
Episodic Free recall	0.01	0.47	0.86

Source: Tulving 1983b, Table 5.6, p. 90.

Pathological dissociations. Pathological dissociations supporting the episodic/semantic distinction have been discussed more fully elsewhere (Schacter & Tulving 1982). A few examples are mentioned here.

Warrington and Weiskrantz (1974) compared (episodic) Yes/No recognition performance with (semantic) word-fragment completion performance in four amnesic patients and four control subjects. They found that the control subjects' recognition-memory performance was much better than that of amnesics, whereas the two groups did not differ in the word-fragment completion performance. More recent evidence reported by Warrington and Weiskrantz (1982, Exp. 1) also points to a dissociation between episodic and semantic tasks when amnesic patients are compared with control patients.

Dissociations of episodic and semantic memory are found in many clinical descriptions of the amnesic syndrome (e.g., Claparède 1911; Williams & Smith 1954). A dissociation between episodic and semantic memory tasks has also been described in an experiment with a single patient who was suffering from a temporary functional amnesia (Schacter, Wang, Tulving & Freedman 1982): During the amnesic episode, the patient had great difficulty remembering events from his life, but no difficulty in identifying well-known people from their photographs (Albert, Butters & Levine 1979).

Evaluation of the evidence. The evidence reviewed shows that dissociations between episodic and semantic tasks have been observed in both laboratory experiments and clinical settings, with data provided by normal subjects, hypnotized subjects, and brain-damaged patients, as well as by functional amnesia patients. Semantic memory in these observations was tapped by a number of different tasks: sentence verification, lexical decision, tachistoscopic identification, word-fragment completion, free association, naming of category instances, production of opposites; both recall and recognition served as tasks of episodic memory.

The hypothesis of a functional distinction between episodic- and semantic-memory systems provides an economical explanation of the finding of the same pattern of performance – dissociation of tasks – in the face of a great deal of situational diversity: The manipulated variables, or different subject groups, produce differences in performance in episodic and semantic tasks, because the tasks tap different memory systems. In the absence of such an overall explanation, a large number of different, unique explanations would have to be provided for the results of different experiments.

Extensions and contrasts

If we accept the hypothesis that the two types of memory represent functionally distinct systems, we can proceed with the study of similarities and differences between the systems, possible extensions of the taxonomy of memory systems, and contrasts with other systems.

Priming effects. A persistent finding in experiments demonstrating experimental dissociations between episodic and semantic tasks, experiments we have just reviewed, was that of priming in semantic tasks. In experiments by Jacoby and Dallas (1981), McKoon and Ratcliff (1979), Kihlstrom (1980), and Tulving et al. (1982) – as well as in other related experiments (Morton 1979; Williamsen, Johnson & Eriksen 1965; Winnick & Daniel 1970) – performance in the semantic task, although not influenced by manipulated variables, was enhanced by virtue of subjects' prior experimental exposure to the target words. Thus, the complete results of these experiments can be schematically depicted as in Figure 1: A manipulated variable has an effect on the episodic task, no effect on the semantic task, and there is a priming effect, independent of the manipulated variable, in the semantic task.

No good explanations of priming are available as yet. Jacoby and Witherspoon (1982) and Tulving et al. (1982) have shown that priming in semantic tasks is uncorrelated with performance on episodic recognition tasks: Tachistoscopic identification and word-fragment completion were found to be indistinguishable for words that subjects thought they had seen before and words they

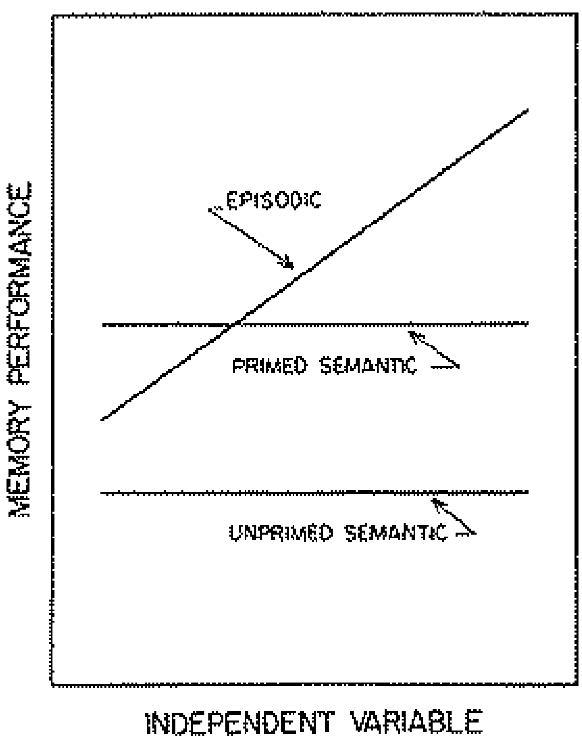

Figure 1 (Tulving 1983b, fig. 6.2, p. 106). A schematic pattern of data showing the effect of a manipulated variable in an episodic task, no effect of the same variable in a semantic task, and the priming effect, independent of the manipulated variable, in the semantic task.

thought they had not. These and other related findings (e.g., Kihlstrom 1980; Williamsen, Johnson & Eriksen 1965) seem to imply that priming effects are mediated by, and reflect the operations of, a system other than episodic memory.

Do priming effects reflect changes in the semantic-memory system? Since priming effects are defined in terms of changes in performance on semantic tasks, it would seem natural to answer the question in the affirmative. Certain facts, however, suggest that the answer may be more complicated. First, priming effects in semantic memory can be long-lived: Tulving et al. (1982), for instance, observed virtually no reduction in priming effects over an interval of seven days. The second fact has to do with the absence, or at least severe attenuation, of cross-modality priming effects (Jacoby & Dallas 1981; Morton 1979; Winnick & Daniel 1970): For priming to be optimal or to occur at all, the initial presentation of the target item has to be in the same sensory modality in which the item appears in a subsequent task. These two facts rule out *temporary* activation of modality-free semantic structures as responsible for the priming effects.

A third hypothesis is that priming reflects an improvement in the facility with which cognitive operations can be carried out, that is, that priming is a phenomenon of procedural memory. We know that many cognitive skills can be improved with practice (e.g., Cohen & Squire 1980; Kolers 1976b; Neisser, Novick & Lazar 1963; Peterson 1969), and priming effects may reflect nothing more than such improvement. The major difficulty with this hypothesis lies in the specificity of priming effects: Improvement in facilitation defined as priming occurs at the level of individual words or other small cognitive units. It has been customary to think of acquired skills in terms of their applicability to a wide variety of situations.

Priming effects that cannot be readily interpreted as reflecting changes in episodic, semantic, or procedural memory suggest the need for a modification of existing distinctions, or perhaps for an extension of memory taxonomy. Such a need is also hinted at by the existence of "free radicals" in memory.

Free radicals. Free fragments, discussed by Schacter and Tulving (1982), or free radicals, are bits of symbolic knowledge originally constructed as a part of the trace of an experienced episode that have become detached from episodic memory but have not, or not yet, been attached to any structure in semantic memory. Clinical descriptions of amnesia contain frequent references to patients' fragmentary knowledge of their recent experiences, in the absence of any awareness of the source of such knowledge (e.g., Claparède 1911; Luria 1976; Williams & Smith 1954). Evans and Thorn (1966) have described similar "source amnesia" for information acquired under hypnosis. The Warrington–Weiskrantz (1974; 1978) effect, discussed earlier as an example of pathological dissociation between episodic and semantic memory, could also be interpreted with the aid of the concept of free radicals.

Working memory and reference memory. The distinction made by Olton and his associates (e.g., Olton, Becker & Handelmann 1979; 1980; Olton & Papas 1979) between working memory and reference memory, based on work with animals, represents an interesting parallel to the episodic/semantic distinction. Working memory reflects an animal's knowledge of particular events in its recent past, whereas reference memory has to do with the animal's knowledge of relatively more permanent components of its world.

Olton and his associates have shown that bilateral destruction of the external connections of the hippocampus produces a permanent impairment in the working-memory component but not in the reference-memory component, reminiscent of similar dissociations between episodic and semantic memory performances in amnesic patients.

Part II: General abstract processing system

The second part of the book describes a conceptual framework for the study and understanding of episodic memory. The framework is referred to as a General Abstract Processing System (GAPS) of episodic memory. It is *general* in that it is meant to apply to remembering of events of all sorts; it is *abstract* in that the specific nature of its components is not specified; it is a *processing* system since its major components have to do with the activity and the functioning of the system rather than its structure; and it is a *system* in the sense of an ordered and reasonably comprehensive collection of interacting components whose assemblage constitutes an integrated whole.

Elements of episodic memory

The basic unit of the conceptual analysis of episodic memory is an act of remembering that begins with an event, perceived by the rememberer, and ends with recollective experience, the rememberer's private awareness of the event on a subsequent occasion, or with memory performance, the overt expression of the recollective experience.

GAPS can be described in terms of the componential structure of an act of remembering, summarized in Figure 2. It consists of 13 conceptual elements, organized in three groups: observables, hypothetical processes, and hypothetical states. Each element is tied to one or two other elements, indicated in Figure 2 by arrows, through relations such as "influences," "has an effect," or "brings about." (The broken arrows in the scheme represent relations that do not affect the ongoing act of remembering but may influence the outcome of a subsequent one.)

The processes of encoding, recoding, ecphory (actualization of a latent engram), and conversion in GAPS are to be thought of as "momentary" processes, or events, in the sense of Miller and Johnson-Laird (1976, pp. 443ff.). The states of the system can be thought of as processes held in abeyance, or as indicants that some processes have been completed and others have not yet begun.

The elements of GAPS can be classified into two categories, elements of encoding and elements of retrieval. The encoding part of an act of remembering begins with the perception of an event and ends with an original or a recoded engram; retrieval begins with the perception of a retrieval cue and ends with the recollective experience of the event, conversion of ecphoric information, or both.

ELEMENTS OF EPISODIC MEMORY

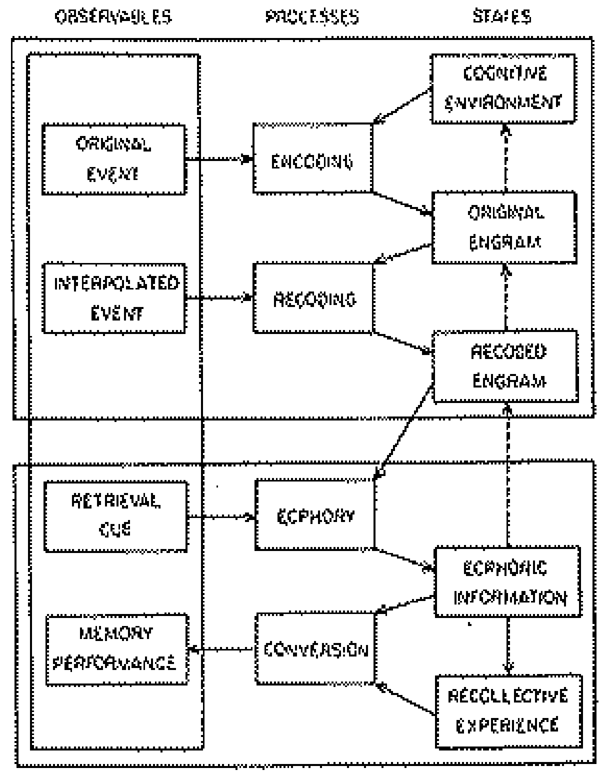

Figure 2 (Tulving 1983b, fig. 7.1, p. 135). Elements of episodic memory and their relations.

Elements of encoding

Original events. The basic units of perceived time are events. An event is something that occurs in a particular place at a particular time. The closely related term "episode" refers to an event that is a part of an ongoing series of events.

It is useful to distinguish between the setting and the focal element (or elements) of an event (Hollingworth 1913, pp. 532–33). Setting refers to the time and place in which the event occurs, whereas focal element is a salient happening within the setting. Events are always unique, they are never repeated. But events may resemble one another, by virtue of the similarity of their settings, focal elements, or both. Similarity relations among events play an important role in recoding and in ecphory.

In experiments that can be thought of as having to do with episodic memory, the settings have usually been held constant and the subjects' recollection of them has been taken for granted. Only the focal elements, or the factual or semantic contents (Schacter & Tulving 1982) have been varied, in the form of discrete units of verbal or some other symbolic material. As the remembering of settings has not yet been studied, it can be argued that full-fledged research on episodic memory has not yet begun.

What is stored about an event in memory depends not only on the event as such and its own characteristics, but also on a large number of both temporary and permanent characteristics of the memory system. These characteristics have been known throughout history under a large variety of names. For example, McGeoch (1942, p. 501) referred to them as the "context of the individual's symbolic or ideational events," Bower (1972, p. 85) named them the "organism's cognitive state," and Donald Thomson and I (Tulving & Thomson 1971, p. 123) used the term "cognitive environment" as a label for the factors other than the event that influence the processing of the event.

Encoding. Encoding is the process that converts an event into an engram. Encoding processes are manipulated experimentally through encoding operations that subjects are instructed or induced to perform on perceived events.

The effects of encoding operations on remembering are revealed by experiments conducted according to the "encoding paradigm." A large variety of methods and techniques have been used to vary encoding processes (e.g., Craik 1973; Craik & Watkins 1973; de Schonen 1968; Geiselman & Glenny 1977; Hyde & Jenkins 1969; Johnson-Laird, Gibbs & de Mowbray 1978; McClelland, Rawles & Sinclair 1981; Tresselt & Mayzner 1960; Woodward, Bjork & Jongeward 1973). In these experiments, subjects have performed many different kinds of orienting tasks while inspecting the material to be remembered.

The effects of encoding operations on subsequent recall or recognition of the material can be considerable. For instance, in an experiment done by Mathews (1977, Exp. 3) subjects made different *semantic* judgments about word triplets, under either incidental or intentional learning instructions. Probability of recall was the same for incidental and intentional learning instructions, but varied greatly with the nature of the encoding operations performed, from .10 for a relatively ineffective operation to .68 for a relatively effective one.

A number of explanations of the differential effectiveness of encoding operations have been offered, beginning with the seminal paper by Craik and Lockhart (1972) in which differences in retention were attributed to differences in depth of encoding. None of the explanations and theories advanced (e.g., Anderson 1976; Anderson & Reder 1979; Craik & Tulving 1975; Eysenck 1979; Jenkins 1974; Lockhart et al. 1976; Nelson 1979; Postman 1975b; Postman, Thompkins & Gray 1978) has as yet gained general approval. In GAPS, explanations of encoding processes are tied to explanations of retrieval processes.

Engram. The product of encoding is an engram, or memory trace. Within GAPS, an engram (the word was coined by Semon 1904), like other hypothetical concepts, is defined in terms of its position in the overall scheme of things and its relations to other elements of the system. Engrams are specified in terms of both their antecedent conditions – particular events particularly encoded in particular cognitive environments – and their consequent conditions, including the circumstances surrounding their subsequent ecphory and retrieval. Different conceptualizations of engrams – whether as information stored about past events, records of operations, attunements, dispositions, images, copies, propositions, analog representations, or as particularly marked parts of associative networks – are compatible with GAPS.

A particularly useful idea is that the engram of an event is a bundle of features, or a collection of some other kind of more primitive elements. This idea, advocated by many contemporary theorists (e.g., Bower 1967; 1972; Estes 1959; Underwood 1969; Wickens 1970) helps us to talk about, and in some sense understand, phenomena of memory that could not have been equally gracefully handled by other languages. One of the advantages of the feature language lies in the fact that it allows us to think about engrams of different events as *qualitatively* different. Two engrams are similar to, or different from, each other to the extent that they possess shared and distinctive features, in keeping with the theoretical analysis of Tversky (1977).

Recoding, interpolated events, and recoded traces. One of the most distinctive characteristics of engrams of events is their mutability: Functional properties of engrams change over time. *Recoding* is the generic name of related operations and processes that take place after the encoding of the original event and thereby bring about changes in the engram. Research relevant to the concept of recoding has appeared in the literature under headings such as: repetition effects (e.g., Glanzer & Duarte 1971; Peterson, Saltzman, Hillner & Land 1962); rehearsal (Rundus 1971; Woodward et al. 1973); retroactive effects (e.g., Barnes & Underwood 1959; Postman, Keppel & Stark 1965); retrieval-induced recoding (e.g., Allen, Mahler & Estes 1969; Bartlett & Tulving 1974; Bjork 1975; Darley & Murdock 1971); "mental contiguity" (Glanzer 1969; Jacoby 1974; Wallace 1970); diffusion of trace elements (e.g., Shephard 1961; Shephard & Chang 1963); cue overload (e.g., Earhard 1977; Watkins & Watkins 1975); information integration (e.g., Bransford & Franks 1971; Loftus 1975; Loftus, Miller & Burns 1978; Pezdek 1977); "incrementing" (Raaijmakers & Shiffrin 1981); as well as others. The recoding of an original engram in all these cases is governed by the similarity of interpolated events to the original event, similarity of encoding operations performed on interpolated events, or both.

The most systematic study of recoding of events has been conducted by Loftus (e.g., Loftus 1975; 1977; Loftus et al. 1978; Loftus & Palmer 1974). In these experiments, it has been shown that references to an original event after it has occurred can change what the person reports about the original event. Loftus has interpreted these data to suggest that the interpolated reference to the event modifies the information stored about the event. In the language of GAPS, we would say that the original engram has been recoded. After recoding, utilization of some of the information contained in the original engram is no longer possible.

Elements of retrieval

Engrams have no effect on ongoing mental activity unless they are retrieved. For retrieval to occur, two necessary conditions must be met: The system must be in the "retrieval mode," and an appropriate retrieval cue must be present.

We know little about the retrieval mode, since it has not been systematically studied. Although experiments have been done to compare intentional and incidental learning, "tests" of memory in experiments have always taken place under "intentional retrieval" conditions.

All retrieval is cued: Retrieval does not occur in situations in which appropriate retrieval cues are absent (Jones 1979; Tulving 1976; Tulving & Madigan 1970; Watkins 1979). An important research problem lies in the identification of the nature of "invisible" cues (Eich 1980; Tulving & Watkins 1975) in situations in which no cues appear to be present.

Ecphory. Ecphory is the process that combines the information in the retrieval cue and the engram into ecphoric information. The term "ecphory," too, was invented by Richard Semon (1904). Ecphory is one of the two central elements in the process of retrieval; the other is conversion. The distinction between ecphory and conversion is necessary not only because one precedes and the other follows the state of ecphoric information, but also because it helps us to conceptualize the relation between recall and recognition, as we will see presently.

The distinction between ecphory as a subordinate process of retrieval and the superordinate process of retrieval as a whole helps us to distinguish between the concept of retrieval as envisaged in GAPS and the same term as used by other writers (e.g., Anderson & Bower 1973; Indow 1980; Kintsch 1968; Mandler 1980; Mandler, Pearlstone & Koopmans 1969). Most contemporary theories of retrieval conceptualize retrieval as a kind of activation of latent information, or associations (e.g., Anderson & Bower 1972; 1974; Atkinson, Herrmann & Wescourt 1974; Jones 1976; 1980; LeCocq & Tiberghien 1981; Murdock & Anderson 1975; Norman & Bobrow 1979; Raaijmakers & Shiffrin 1981; Ratcliff 1978; Ratcliff & Murdock 1976; Shiffrin 1970). These theories assume, either explicitly or implicitly, that the contents of what a person recalls or recognizes reflect only the information that has been stored. Recall cues contribute to the experience of remembering only by virtue of determining what part of the stored information is activated. Thus, these theories attribute to episodic memory a retrieval process that characterizes semantic memory.

In episodic memory, according to GAPS, the process of ecphory is a constructive activity – a synergistic process – that *combines* the (episodic) information from the engram and the (semantic-memory) information from the cue. Similar ideas about the nature of remembering have been advocated by Bartlett (1932), Neisser (1967), Bransford and Franks (1971), Kintsch (1974), among others.

The complementary function of retrieval information in ecphory is somewhat conjectural, in that evidence for it is mostly indirect. The experimental work that comes closest to providing direct evidence for complementarity of engrams and cues is a series of experiments by Loftus and her colleagues mentioned earlier (Loftus 1975; Loftus & Loftus 1980; Loftus et al. 1978; Loftus & Palmer 1974).

Ecphoric information and recollective experience. Ecphoric information is the product of the process of ecphory. It determines the particulars of recollective experience and provides the input into the conversion process: What a person remembers of an event depends directly on the quantity and quality of relevant ecphoric information.

Recollective experience refers to the rememberer's subjective awareness of ecphoric information. The terms that have been most frequently used in descriptions of the mental experience of remembering are "memory image" and "consciousness": When a person remembers a past event, he has a mental image of it and is consciously aware of its being a mental replay of what happened once before.

The feeling that the present recollective experience refers to a past event and the feeling that the experience is veridical are determined by the intrinsic properties of ecphoric information. A reasonable assumption is that the intensity of the feeling of pastness is directly correlated with the relative contribution that the information from the (episodic) engram makes to the ecphoric information.

Ecphoric information can also serve as retrieval information. The product of ecphory involving ecphoric information as one source of input into the process is a new and different assembly of retrieval information. The recursive operation may be repeated until some stop rule is invoked (Kintsch 1974; Lockhart et al. 1976; Raaijmakers & Shiffrin 1981; Semon 1904).

GAPS describes a "snapshot view" of episodic memory: It focuses on conditions that bring about a slice of experience frozen in time which we identify as "remembering." The recursive operation of the process of ecphory, feeding upon the (changing) ecphoric information and combining it with the "fixed" stored episodic information, produces many snapshots whose orderly succession can create the mnemonic illusion of the flow of past time.

Conversion and memory performance. The act of remembering a particular episode may end with the recollective experience. The rememberer "just thinks about" the experience and does not express it in any overt fashion. At other times, recollective experience, or ecphoric information of which a rememberer is not directly aware, is converted into behavior. The form of conversion (e.g., recall, recognition, some "memory judgment") can be more precisely stipulated in laboratory experiments than in real life, but the general principles governing conversions are assumed to be very much the same.

The elements of the retrieval process in GAPS that have been labelled "ecphoric information" and "conversion" are related to the distinction between "memory" and "decision" in signal-detection analyses of memory (e.g., Lockhart & Murdock 1970; Murdock 1974). In recognition-memory tasks, "decision processes intervene between memory and response" (Murdock 1974, p. 8); in GAPS, conversion processes intervene between ecphoric information and overtly observable memory performance. The main difference between signal-detection analyses of recognition memory and GAPS lies in the process of ecphory.

Part III: Synergistic ecphory

The third main part of the book discusses findings from experiments that have helped to shape the overall structure of the General Abstract Processing System as summarized in the second part. The section begins with a brief review of the history of the work that led to the idea of encoding specificity.

From organization to encoding/retrieval interactions

With some imagination it is possible to see how the ideas discussed in the book grew out of my early work on subjective organization in multi-trial free recall (Tulving 1962) and intratrial and intertrial forgetting (Tulving 1964).

In 1966, Zena Pearlstone and I (Tulving & Pearlstone 1966) did a large experiment in which we compared cued and noncued recall. The finding that cued recall was better than noncued implied that recall depends on conditions of both storage and retrieval, that the failure of noncued recall of an item does not signify absence of stored information about the item, and that, with storage conditions held constant, successful recall varies as a function of the number and appropriateness of retrieval cues.

We used the term "availability" to refer to the hypothetical presence of information in the memory store, and the term "accessibility" to designate that part of the available information that could be retrieved. Given the idea that recall depends on both availability and accessibility, it was easy to imagine that sometimes some cues might be effective where others would fail. What determines the effectiveness of cues?

We did a number of small experiments, under relatively casual conditions, to try out a number of ideas relevant to this question. We quickly found out that the presence of preexperimental associations between cues and to-be-recalled list words did not always suffice for accessibility, and that effectiveness of cues seemed to depend on processes occurring at the time of study. For instance, a descriptive phrase such as *double letter in the middle* is an effective retrieval cue for a studied target word such as SUMMER, but *only if* the subject, while studying the list, had noted the fact that SUMMER was a word that had a double letter in the middle.

We formalized the operations of these "quick and dirty" experiments in a single but extensive experiment designed to examine the effectiveness of retrieval cues as a function of encoding conditions (Tulving & Osler 1968). The results of the experiment showed that "Specific retrieval cues facilitate recall if and only if the information about them and their relation to the [to-be-remembered] words is stored at the same time as the information about the membership of the [to-be-remembered] words in a given list" (Tulving & Osler 1968, p. 599). We thought that the same relation between effectiveness of cues and encoding conditions would hold generally, including situations where the learners were left free to encode the to-be-remembered words any way they wanted.

To test the generality of the conclusion, Donald Thomson and I did three experiments in which we varied both the encoding conditions and the preexperimental strength between the cue and target words (Thomson & Tulving 1970). We found that the cueing effectiveness of even a very strong preexperimental associate of the target word depends on what happens at the time of study. We contrasted these findings with the predictions made by the generation/recognition models of recall (e.g., Bahrick 1969; 1970; Kintsch 1970), according to which strongly associated words should have been effective cues regard-

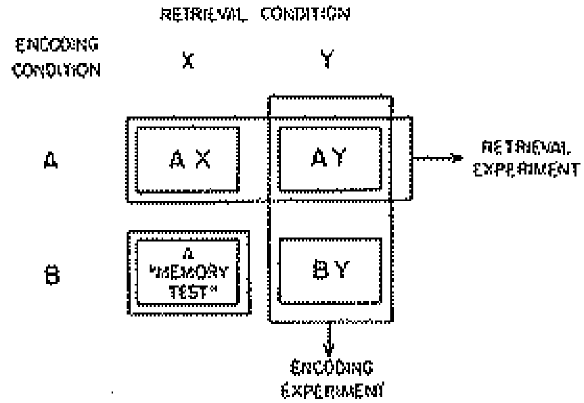

Figure 3 (Tulving 1983b, fig. 10.1, p. 220). A schematic diagram of the encoding/retrieval paradigm of episodic memory research.

less of how the target words had been encoded. We concluded that these models were wrong and that the two critical assumptions on which they were based – assumptions that we referred to as associative continuity and transsituational identity of words – were in need of revision.

Tulving and Osler's (1968) and Thomson and Tulving's (1970) experiments conformed to what we now refer to as the "encoding/retrieval paradigm." In this paradigm, both encoding and retrieval conditions are experimentally manipulated. Subject and material variables are usually held constant, although they could be varied as additional dimensions.

A schematic representation of a minimal encoding/retrieval experiment is shown in Figure 3. In it, two encoding conditions, A and B, are crossed with two retrieval conditions, X and Y. We can think of the total design as entailing two retrieval experiments and two encoding experiments, all conducted simultaneously. In a retrieval experiment, encoding conditions are held constant and retrieval conditions varied: Each of the two rows in Figure 3 represents a retrieval experiment. In an encoding experiment, retrieval conditions are held constant and encoding conditions are manipulated: Each of the two columns in Figure 3 represents an encoding experiment. An experimental situation in which both encoding and retrieval conditions are held constant represents a memory test of the kind used in psychometric measurement of abilities.

Outcomes of single retrieval experiments and single encoding experiments are theoretically uninteresting, because they seldom permit discrimination among alternative explanations. Thus, the finding that, say, recall is better after one way of studying the material than after another affords many possible interpretations; so does the finding that one set of cues leads to better recall than another. On the other hand, strong interactions between encoding and retrieval conditions that can be observed in an encoding/retrieval experiment do rule out explanations that fit individual encoding and retrieval experiments. The critical findings in the Tulving and Osler (1968) and Thomson and Tulving (1970) experiments had to do with such interactions between encoding and retrieval conditions. It was these interactions that led to the idea of encoding specificity.

Encoding specificity

Although the concept of encoding specificity had its beginning in the pursuit of the problem of the effectiveness of retrieval cues, it has changed over the intervening years. The essence of the concept now lies in the emphasis on the *relation* between the specifically encoded (and perhaps recoded) memory trace and the particular retrieval information as the determinant of recollective experience: The engram and the retrieval cue must match and complement each other for remembering to occur.

The concept of encoding specificity denies the validity of a number of ideas that not too long ago were widely held: The idea that items (events) of a particular class are easier to remember than items (events) of another class, that a particular encoding operation is more effective than another encoding operation, that a particular type of retrieval cue is more effective than another type of cue, that copy cues provide automatic access to the stored information, that recognition-memory performance provides a measure of trace strength, and that memory traces have strength. These ideas are no longer tenable. According to encoding specificity, no absolute statements about the memorability of items and the effectiveness of particular kinds of encoding operations or particular kinds of retrieval cues are justified. Further, the effectiveness of all cues, including copy cues, depends on the conditions under which the target event was encoded. Finally, recognition-memory tests provide no better basis for estimating the strength of memory traces than do any other memory tests, or the application of any other type of cue. Indeed, traces have no strength independently of conditions in which they are actualized: Any given trace has many different "strengths," depending upon retrieval conditions.

Empirical evidence in support of encoding specificity is provided by findings from a large number of encoding/retrieval experiments. A representative list of such experiments includes those by Baker and Santa (1977, Exp. 2), Dong (1972, Exp. 2), Eich, Weingartner, Stillman, and Gillin (1975), Fisher and Craik (1977, Exp. 3), Geiselman and Glenny (1977), Godden and Baddeley (1975), Jacoby (1973), Masson (1979, Exp. 3), Morris (1978, Exp. 2), Ozier (1978), Roediger and Adelson (1980, Exp. 3), Stein (1978, Exp. 1), Thomson (1972, Exp. 4), and Till and Walsh (1980, Exp. 3). In all these experiments, crossover interactions (G. Loftus 1978) between encoding and retrieval were observed under a wide variety of conditions. The to-be-remembered materials included unrelated words, homographs, word pairs, categorized words, words embedded in sentences, and whole sentences. Encoding conditions were manipulated in terms of distributions of target words in study lists, test expectations, verbal context of target words, intentional learning, interactive imagery, pleasantness ratings, comprehension judgments, "thinking about" initial letters or categories of to-be-remembered words, judgments of meaningfulness of relations between comparison and to-be-remembered sentences, single or pairwise presentation, typicality of actions depicted by sentences, imagined voices speaking visually presented words, changed physical environments, and changed drug states of the rememberer. In most experiments, subjects engaged in intentional learning; in some, learning was incidental. Retrieval conditions were manipulated by asking the subjects to engage in free recall or in different kinds of recognition, or by presenting various types of retrieval cues: intralist and extralist cues, associatively related words, category names, initial letters, parts of studied sentences, words describing plausible inferences drawn from the studied material.

Thus, the relativity of "goodness" of encoding operations and the relativity of the power of retrieval cues, or the critical requirement of compatibility between specifically encoded engrams and retrieval cues, seem to hold, if not universally, then at least over a very wide set of conditions.

The initial conflict between encoding specificity ideas and generation/recognition theories of recall has been largely resolved. Generation/recognition models have been revised and brought in line with the concept of encoding specificity (e.g., Kintsch 1974). Moreover, the concept of encoding specificity is perfectly compatible with generation/recognition as an effective *strategy* of retrieval under certain conditions (Rabinowitz, Mandler & Barsalou 1979). And there is substantial agreement that the system can generate potentially effective retrieval information.

But some problems more fully discussed elsewhere (Rabinowitz et al. 1979; Tulving 1976; Watkins & Gardiner 1979) remain. These have to do with the question of whether the process of generation is guided by episodic or semantic information, and the question of whether the product of the generation process has to be some sort of a "copy" of the to-be-remembered item, rather than just (any kind of) useful retrieval information. Still another problem concerns the assumptions that are made about what it is that determines whether the generated information is accepted by the subject as "desired" or rejected as "not desired."

It is sometimes useful to distinguish between the encoding specificity hypothesis and the encoding specificity principle. The hypothesis is a tentative statement about the relation between the properties of the memory trace of an event and the effectiveness of retrieval cues; its tenability can be evaluated empirically. The principle is the assumption that the hypothesis is true; its usefulness depends on the truth of the hypothesis.

Three entities are involved in the testing of the encoding specificity hypothesis: the engram of the event, the retrieval cue, and the relation between them. When we test the hypothesis, we must know, or must be in a position to make reasonable assumptions about, the encoded features of engrams, as well as the features useful for retrieval. The results of the observations then tell us something about the third entity, the relation between the trace and the cue.

When we adopt encoding specificity as a principle, we can make inferences about the informational contents of memory traces on the basis of the observed effectiveness of retrieval cues. In so doing, we rely on the same logic that has been used in other situations to describe objects that are not directly observable.

Criticisms of encoding specificity

Encoding specificity ideas have been criticized by a number of people on a variety of grounds. Some have declared the ideas to be untestable, others have said they are false; some have questioned the underlying logic, others have produced data that they have interpreted as contrary to encoding specificity; some have provided alternative explanations of findings supportive of encoding specificity, others have failed to replicate the findings. We will consider a sample of criticisms next, together with rebuttals of some of them.

The critical evidence for the concept of encoding specificity, as we have seen, has been provided by encoding/retrieval interactions. But what are we to make of experiments that yield no evidence of such interactions? One such experiment has been described by Postman (1975a, Exp. 1). It was designed to compare the effectiveness of different types of intralist and extralist cues following the study of target words in the presence or absence of associated list cues. The experiment was complex, comprising 16 experimental conditions, with a number of them nominally corresponding to the conditions in Thomson and Tulving's (1970) experiment.

Postman's data did not replicate the critical interaction found in the Thomson and Tulving experiment. Instead, they showed that a "strong" extralist cue (e.g., *bloom*) was as effective in eliciting the corresponding target word (FLOWER) following the target word's encoding in the presence of a "weak" cue as it was following the target's encoding as a single item in the study list, and that in both of these conditions strong-cue recall was considerably higher than noncued recall. Postman concluded that the kinds of results reported by Thomson and Tulving were obtained only under "narrowly circumscribed conditions" (Postman 1975a, p. 64).

This conclusion is defensible, as long as one compares only the two experiments in question. But when we consider encoding/retrieval interactions that, as we saw earlier, have now been observed under a wide variety of experimental conditions, Postman's criticism loses its force. Instead, a more reasonable interpretation is that, for reasons unknown, the intended manipulation of encoding conditions did not work in Postman's experiment.

Several writers have produced data and arguments claiming that, contrary to the encoding specificity hypothesis, unencoded cues are effective (e.g., Anderson & Pichert 1978; Baker & Santa 1977; Kochevar & Fox 1980; Light 1972; Marcel & Steel 1973). The relevant data are those showing that cues not present at the time of study and unlikely to have been encoded by the subjects at that time were nevertheless effective when given at retrieval.

There are two problems with the conclusions drawn by these critics. First, some of the relevant data have been drawn from simple retrieval experiments, rather than encoding/retrieval experiments, and such data are not compelling. Second, the criticisms would hold only if the experimenters' assumptions as to how their subjects encoded the material were true: Whether they are is not known. Baker and Santa (1977), for instance, chose to reject the encoding specificity hypothesis on the basis of comparisons of free and cued recall in retrieval components of their two experiments which together conformed to the pattern of the encoding/retrieval paradigm. The results of the two experiments considered together provide strong support for encoding specificity.

A different type of critical experiment is one that purports to show that encoded cues are ineffective. A representative experiment in this category is one reported by Humphreys and Galbraith (1975, Exp. 2). Using asymmetrical free-association pairs of words (e.g., *tobacco* and *smoke*) as a source of materials, they did an experiment whose results were very much what one would have expected on the basis of the generation/recognition theory, and contrary to those expected on the basis of the encoding specificity hypothesis. But this interpretation, as Humphreys and Galbraith acknowledged, depended on a critical assumption concerning the encoding of study-list words. When this assumption was put to experimental test by Ley (1977), it turned out to be wrong, thereby invalidating Humphreys and Galbraith's criticism.

Another type of objection to encoding specificity ideas concerned their circularity. Solso (1974, p. 28) put it as follows: "If a cue was effective in memory retrieval, then one could infer it was encoded; if a cue was not effective, then it was not encoded. The logic of this theorization is 'heads I win, tails you lose' and is of dubious worth in the history of psychology. We might ask how long scientists will puzzle over questions with no answers."

This criticism would be a serious one if it were indeed true that one always explained cue effectiveness by assuming certain properties of memory traces, and, at the same time, justified one's assumptions about the trace properties in terms of observed effectiveness of retrieval cues. But we can get out of the circle by doing what already has been done in many experiments: manipulate properties of memory traces by varying antecedent conditions, such as the events to be remembered or the way in which they are encoded. Although we never know exactly what information has been encoded into the trace, we can frequently influence it and make informed guesses about it. Experiments by Fisher and Craik (1977), Morris (1978), Geiselman and Glenny (1977), and Godden and Baddeley (1975), as well as many others, provide relevant evidence. Thus, in many cases, the observed effectiveness, or lack of effectiveness, of retrieval cues makes good sense in terms of what we know about the to-be-remembered events, their encoding, and their compatibility with cues at the time of retrieval. In other situations, however, we make inferences about how an event was encoded on the basis of observed effectiveness of retrieval cues (e.g., Ogilvie, Tulving, Paskowitz & Jones 1980; Tulving & Watkins 1975) and by so doing we extrapolate from what is known to what is unknown. The logic here is the same as the one used in estimating the strength of habits, associations, and memory traces on the basis of observed behavior, or characteristics of encoded features on the basis of observations of release from proactive inhibition (Wickens 1970), or false recognition (Anisfeld & Knapp 1968; Underwood 1965).

Until such time as the encoding specificity hypothesis is shown to be contrary to facts, it seems reasonable to claim that at some as yet undetermined level of abstraction, it probably holds for all phenomena of episodic memory, in the sense that there are no exceptions to it.

Recognition failure

Recognition failure is the name of the finding that previously studied items cannot be identified as "old" although their names can be reproduced to other cues. Like the encoding/retrieval interactions we discussed, the phenomenon of recognition failure represents one of the many applications of GAPS to the understanding of remembering of word-events.

Scattered examples of experimental findings showing recall to be better than recognition – thereby implying recognition failure – had been reported in the literature before recognition failure became a full object of theoretical interest (e.g., Bahrick & Bahrick 1964; Bruce & Cofer 1967; Lachman & Field 1965; Postman, Jenkins & Postman 1948; Tulving 1968b). These initial observations failed to generate much interest, presumably because the magnitude of the effect was not striking, because they were made under rather special conditions, or because the effects could be explained in terms of uninteresting assumptions, such as "chance fluctuation" of attention.

In 1973 Donald Thomson and I reported findings that seemed to have more serious implications for theory (Tulving & Thomson 1973). The method we used in our initial experiments was rather cumbersome and at the present time it is of historical interest only. Subsequent research has shown that the essential ingredients for the production of recognition failure are three: (a) presentation of an A–B pair of words for study, (b) presentation of B alone as a test item in the recognition test, and (c) presentation of A as a cue for recall of B. In our very first experiment (Tulving & Thomson 1973, Exp. 1) we found that subjects could identify only 24% of the previously seen B items as "old," although they recalled 63% of these targets when A items were presented as cues. These data ruled out the generation/recognition theory of recall, since it was impossible, according to theory, for words to be recalled that could not be recognized. The finding was also contrary to common sense: How could perfectly normal and intelligent people claim that they had not seen a familiar word in the study list but produce it to its pair-mate? A general interpretation of the finding, however, seemed possible with the aid of the encoding specificity principle: It looked as if the encoded trace of the A–B event was such that cue A was more compatible with it than cue B.

Probably because of the counterintuitive nature of the phenomenon, the demonstration of recognition failure created a certain amount of interest, scepticism, and criticism (e.g., Light, Kimble & Pellegrino 1975; Martin 1975; Postman 1975a; Rabinowitz, Mandler & Patterson 1977; Reder, Anderson & Bjork 1974; Santa & Lamwers 1974; 1976). The criticisms, documented by John Gardiner in an in-house report, ranged from the denial of the existence of the phenomenon to the claim that it was trivial and theoretically irrelevant. Since a great deal of research has been done to demonstrate the occurrence of the phenomenon under a wide variety of conditions, and since the magnitude of the effect has been brought under experimental control, most of the criticisms have by now fallen by the wayside.

Occurrence and magnitude. The magnitude of recognition failure can be indexed by the conditional probability of failure to recognize a previously seen item given that it was recalled (Watkins & Tulving 1975). This measure is intuitively meaningful as it directly expresses the proportion of recallable words that cannot be recognized. For certain purposes, however, it is more convenient to work with the complement of the recognition failure measure. This "recognition success" measure is given by the recognition hit rate conditionalized on recall.

In individual experiments, recognition success is highly correlated with overall recognition hit rate across a large number of individual experiments and experimental conditions (Flexser & Tulving 1978; Tulving & Wiseman 1975). A representative set of data is shown in Figure 4. Each data point in the graph represents a large number of observations, pooled over many subjects and many words, in a particular condition in a particular experiment. The systematic relation between recognition failure and overall recognition hit rate shown in Figure 4 is invariant with large variations in levels of recall and recognition in different experiments, with little correlation between them, as shown in Figure 5.

The data in Figure 4 were fitted with a quadratic function of the form:

$$P(Rn|Rc) = P(Rn) + c[P(Rn) - P(Rn)^2] \text{ (Equation 1)}$$

This equation expresses recognition success as a function of recognition hit rate with a single constant. The equation is shown as the solid line in Figure 4, with the value of the constant c set equal to 0.5 on the basis of the least-squares solution of Equation 1.

Recognition failure occurs, and its magnitude is governed by the overall level of recognition according to the function defined by Equation 1, in experiments in which

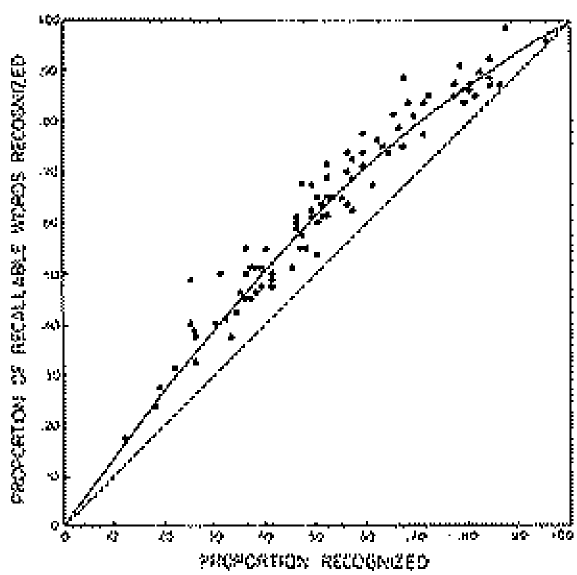

Figure 4 (Tulving 1983b, fig. 13.1, p. 282). Probability of recognition of recallable target words as a function of the probability of recognition of all target words. Each data point represents a separate experiment or a condition in an experiment.

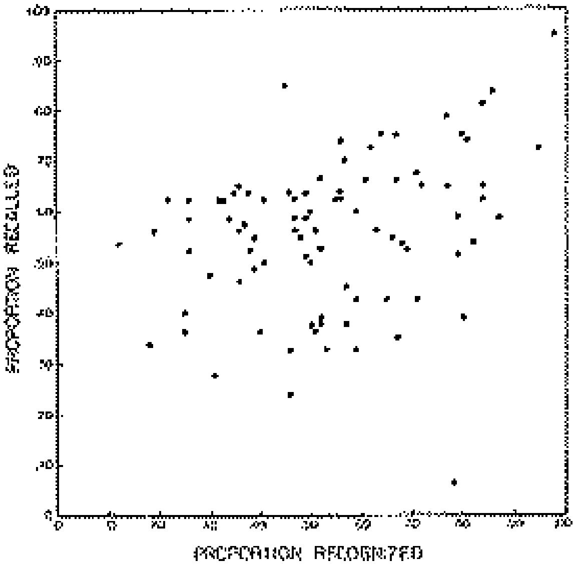

Figure 5 (Tulving 1983b, fig. 13.2, p. 283). Probability of recall of target words as a function of the probability of recognition of the same words. Each data point represents a separate experiment or a condition in an experiment.

recognition is better than recall as well as in experiments in which recall is better than recognition (Flexser & Tulving 1978; Wiseman & Tulving 1976); with naive as well as highly sophisticated subjects (e.g., Begg 1979; Bowyer & Humphreys 1979; Rabinowitz, Mandler & Barsalou 1977; Wiseman & Tulving 1975); in immediate tests as well as tests given a week after the learning (Tulving & O. C. Watkins 1977); with semantically related as well as unrelated study pairs (e.g., Begg 1979; Rabinowitz et al. 1977; Vining & Nelson 1979); with semantically related as well as unrelated recognition-test distractors (e.g., Begg 1979; Bowyer & Humphreys 1979; Postman 1975a; Rabinowitz et al. 1977); in situations in which no distractor items are presented at all in recognition tests (e.g., Begg 1979; Wallace 1978); with low-frequency words that have few semantic senses (Reder et al. 1974), as well as words that have only a single meaning in the dictionary (Tulving & O. C. Watkins 1977).

Recognition failure also occurs in experiments in which the study material consists of simple three-word sentences, such as "Shelter protected ESKIMO," and in which the cue presented in the recall test was not an explicit part of the input material, representing only an inference from the studied sentence (e.g., *igloo*). This experiment, using materials inspired by a study by R. C. Anderson, Pichert, Goetz, Schallert, Stevens, and Trollip (1976) was done at Toronto in collaboration with Norman Park. "Spectacularly large" recognition-failure effects have been demonstrated by Watkins (1974) in an experiment in which target items were meaningless letter bigrams. And Muter (1978) has demonstrated recognition failure, with the data adhering to the recognition-failure function, in a semantic-memory experiment.

Although sizable deviations of data from the recognition-failure function have been reported by Begg (1979), Nilsson and Shaps (1981), and Gardiner and Tulving (1980), the robustness of the phenomenon is no longer in doubt. A large number of explanations has been offered to account for it. One of the more popular ideas has been that recognition failure occurs because of associative asymmetry between the A and B members of a studied pair of items (e.g., Bartling & Thompson 1977; Rabinowitz et al. 1977; Salzberg 1976). There are two main problems with such theories. First, it is not clear whether associative asymmetry is a cause or a consequence of recognition failure (Tulving & Thomson 1973). Second, it is not clear that the relation between forward and backward retrieval, on the one hand, and recognition failure on the other, represents anything other than the general systematic relation between overall recognition and recognition failure, as depicted in Figure 4, a relation that requires explanation in itself.

Theories of recognition failure based on the idea of associative asymmetry, as well as some other theoretical accounts (e.g., Kintsch 1978; Reder et al. 1974) have been concerned with explanations of the occurrence of recognition failure. They have had little to say about the highly systematic nature of the relation between recognition and recognition failure. Such theories have been offered by Begg (1979), Humphreys and Bowyer (1980), and Flexser and Tulving (1978), with a refinement of the latter offered by Jones (1978). Of these, only the Flexser and Tulving (1978) model accounts for the quantitative relation between recognition failure and overall recognition, that is, for the observed value of c in the equation of the recognition-failure function (Equation 1). It does so by working out the logical consequences of retrieval as matching of trace features, under three simple assumptions: (a) In the recognition-failure paradigm, the recognition cue and the recall cue are directed at the same episodic trace; (b) individual traces vary in terms of how well they have been encoded; and (c) information extracted from the recognition cue is uncorrelated with that extracted from the recall cue.

This model of retrieval independence has been tested by conducting a large number of simulated experiments, in which the model's six parameters are *randomly* varied and which, as a consequence, show large, uncorrelated variations in proportions of target items recognized and recalled (as shown in Figure 5), but which also produce recognition-failure data points closely adhering to the function (as shown in Figure 4). Thus, the model achieves a good qualitative and quantitative agreement between experimental and theoretical data; it does so *without* fixing the values of any model parameters. The value of parameter c in Equation 1 (approximately 0.5) falls out of the assumptions and the structure of the model, independently of particular numerical inputs into the model.

The phenomenon of recognition failure represents a subclass of a class of phenomena showing that target items not retrievable in one retrieval situation may become retrievable in another (e.g., Arbuckle & Katz 1976; Buschke 1974; Erdelyi & Becker 1974; Estes 1960; Hoppe & Dahl 1978; Madigan 1976; Wallace 1978). But its counterintuitive nature and the fact that its magnitude can be very large have theoretical implications that other demonstrations of dependence of retrieval on retrieval conditions do not necessarily share. For instance, in what sense can one talk about "strength" of traces of target items in episodic-memory experiments, given that recallability and recognizability of these items are largely uncorrelated?

Recognition and recall

Recall and recognition *tasks* differ in two basic ways. One of these has to do with the type of retrieval information: In recognition tasks, a copy of the to-be-remembered item is given, whereas in recall tasks it is not. The second has to do with conversion: In recall, ecphoric information has to be converted into a description (e.g., production of the name) of the target event, whereas in recognition the subject has to make a simple "familiarity" judgment on the basis of the comparison of ecphoric information with the test item. Thus, in the recognition task, copy cues are presented and familiarity judgments are to be made, whereas in recall no copy cues are given and the name of the target item is to be identified and produced.

With the help of Judith Sutcliffe, we compared subjects' memory performance in a "direct comparison" experiment in which study lists, encoding of target words, and retrieval cues were all held constant, and only conversion requirements varied. Thus, for instance, we compared subjects' performance on familiarity conversion (Yes/No recognition judgments) with the "name identification" conversion (e.g., recall of list words), using either copy cues or, in a parallel comparison, associatively related extralist cues.

The experiment yielded two interesting observations. First, with copy cues performance was better when subjects had to make familiarity judgments about the cues than when they had to produce the (identical) names of target items. Second, the correlation between false recognition and the valence (effectiveness) of associative cues was *negative:* Subjects were more likely to make false positive recognition responses to those associative cues that were least effective in eliciting target items under the "name identification" conversion (recall) conditions. These data are clearly contrary to some earlier speculations about the relation between recall and recognition (Tulving 1976) and necessitated rethinking of the issue of the relation between recall and recognition.

Synergistic ecphory model. The data from the "direct comparison" experiment can be accommodated by the "synergistic ecphory" model schematically depicted in Figure 6. The horizontal axis of the graph space represents trace information, the vertical axis represents retrieval information, and the two-dimensional space defined by the two axes corresponds to ecphoric information. The two curved lines in the diagram represent two conversion thresholds. One determines naming (recall) of target events while the other determines their episodic familiarity. Each bivariate point in the ecphoric space in Figure 6 corresponds to a particular bundle of ecphoric information whose qualitative characteristics define the nature of the rememberer's recollective experience.

The two conversion thresholds divide the total space of ecphoric information into three mutually exclusive subspaces. The region above the naming threshold contains ensembles of ecphoric information that are sufficient for both awareness of episodic familiarity and the naming of the target event; the region between the familiarity and naming thresholds represents ecphoric information that is sufficient for making positive familiarity judgments but insufficient for naming the target event; the region below the familiarity threshold consists of ensembles of ecphoric information that fail to give rise to any feeling of episodic familiarity. The placement of the naming threshold above the familiarity threshold means that it takes *more*, or higher quality, ecphoric information for the naming of the target event than for a familiarity judgment.

The model assumes that retrieval information is independent of trace information. A trade-off between trace information and retrieval information is possible within limits: Poor quality of trace information can be compensated for by high-quality retrieval information, and vice versa. Conversion thresholds are asymptotic with the coordinate axes: No recollective experience or conversion can occur in the absence of either the trace or the appropriate retrieval information. Quantitative measures of memory performance are reflected in the distance of bundles of ecphoric information from relevant conversion thresholds.

The model accommodates a number of basic observations about recall and recognition. For instance, the negative correlation between the valence of associative cues and their (false) recognition occurs because more effective retrieval cues produce ecphoric information that is sufficient for naming, and hence sufficient for the judgment that the target item is different from the cue (e.g., point *b,z* in Figure 6), whereas less effective cues may produce ecphoric information (e.g., point *b,y*) that creates a feeling of familiarity but does not permit the retrieval of the target item, thus leading the subject to (falsely) accept it as "old." The phenomenon of recognition failure fits into the model, despite the fact that name (recall) threshold is higher than familiarity (recognition) threshold, because the thresholds are defined with respect to ecphoric rather than trace information, as used to be the case in classical strength-threshold models of recall and recognition, models that have now been thoroughly discredited (Anderson & Bower 1972; McCormack 1972; Tulving 1976). Other basic facts concerning the relation between recall and recognition can also be accommodated within the model.

The model can be used to illustrate graphically trace-dependent and cue-dependent forgetting, as well as "reversal" of forgetting when retrieval cues are changed (e.g., Tulving 1974). It shows how qualitative properties of memory traces and retrieval cues determine the qualitative properties of ecphoric information, and how the latter are related to quantitatively measured memory performance: "Proportion correct" in an experimental condition is determined by the proportion of bundles of ecphoric information that lie above the relevant conversion threshold. And it suggests that the feeling of pastness in recollection may be determined by the contribution of trace information to the ecphoric information: For instance, in Figure 6, ecphoric information represented by point *a,z* contains little trace information, whereas *c,y* contains more trace information but less retrieval information. We might expect, therefore, that the recollective experience corresponding to *a,z* is tinged with a fainter flavor of pastness, and may seem subjectively less veridical, than the recollective experience based on *c,y*.

The synergistic ecphory model shares many ideas with other contemporary theories of recognition and recall. It is in good agreement with the theory proposed by Lockhart et al. (1976), and has a number of important features

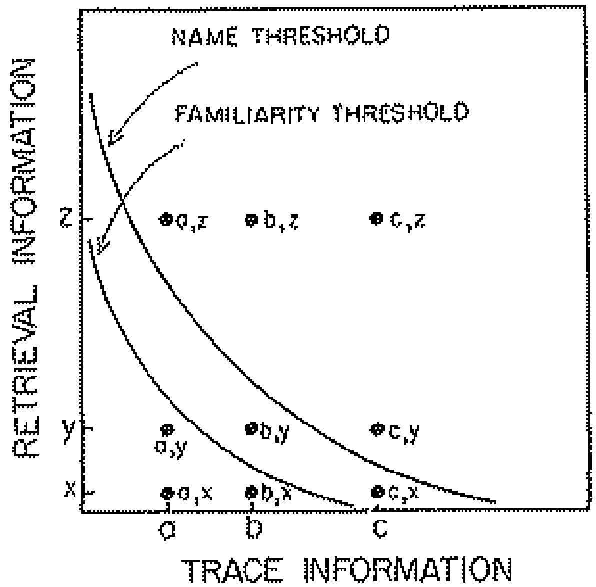

Figure 6 (Tulving 1983b, fig. 14.3, p. 312). A schematic diagram of the Synergistic Ecphory Model of Retrieval. The graph space represents ecphoric information, defined as a product of trace and retrieval information.

in common with theories proposed by Moeser (1977), Kintsch (1974), and Ratcliff (1978). Different recognition and recall thresholds postulated in the synergistic ecphory model are related to the idea, advocated by Mandler as well as others (e.g., Atkinson & Juola 1974; Humphreys 1978; Mandler 1980; Mandler et al. 1969; Tiberghien 1976), that recognition can occur either as a detection of familiarity or as a consequence of particular retrieval operations.

The unique feature of the synergistic ecphory model is the concept of ecphoric information as a conjunction of trace and retrieval information. Although there is as yet little direct evidence to support the concept, a number of facts about remembering and recollective experience, including the phenomenonal reality of variability of subjective feelings of pastness and veridicality, seem to necessitate the postulation of ecphoric information as a synergistic product of two memory systems, episodic and semantic.

ACKNOWLEDGMENTS
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NOTE

1. The book is written at two levels, "serious" main text interspersed with in-text footnotes that provide personal, sometimes tongue-in-cheek, commentary on the main text. These more personal sections of the book are studiously ignored in the present précis.

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Neuropsychological evidence and the semantic/episodic distinction

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While there is general agreement that the semantic/episodic distinction is heuristically useful, the claim that separate functional systems are involved is much less plausible. At first sight, the mass of evidence summarised in Table 1 of Tulving's accompanying Précis seems overwhelming. It does not, however, speak directly to the issue of whether separate systems are involved. An analogy might help explain why.

I first read this section of Tulving's book (1983b) while in a plane flying over wooded countryside. Out of the window, the forest beneath looked like a grey-green carpet, totally different from what its appearance would have been had I been standing in the forest. I could easily produce a long list of perceptual differences in terms of sight, sound, and even smell between the forest as experienced from the plane and as experienced from within. Would I therefore be entitled to conclude that they were quite separate forests? Clearly not. By analogy, one can reasonably argue that semantic and episodic memory emphasize different aspects of the same system.

With this in mind, the neuropsychological evidence for a distinction becomes particularly important. If it can be shown that one part of the brain is necessary and sufficient for episodic memory but unnecessary for semantic memory, while another part of the brain is necessary for semantic but not episodic memory, then the argument for two separate systems becomes vastly stronger. Tulving recognises this and appeals to neuropsychological evidence. How convincing is this appeal?

Consider first the blood flow study by Wood, Taylor, Penny & Stump (1980) cited in Tulving's Précis. This shows that the pattern of blood flow within the brain is somewhat different depending on how a subject is required to process a given word. More specifically, recognising whether a word has been presented previously gives rise to a somewhat different blood flow pattern from judging whether that word represents an object that could be contained in one's living room. This result is cited by Tulving as evidence for the separate location of semantic and episodic memory within the brain. Such an interpretation is possible but far from compelling. First, as Wood et al. (1980) point out, there are a number of differences between the two tasks other than the possibility that one relies on semantic and the other on episodic memory; they are, for example, different in difficulty. Second, even if such extraneous factors are ignored, the evidence merely suggests that the two processing tasks are different, and that this difference is reflected in cerebral blood flow. Given a fine enough measure of blood flow, it is conceivable that any two tasks that differ cognitively may be detectably different in blood flow pattern. Would one therefore wish to assume a physically separate system for each task? Clearly not.

Rather more compelling evidence for separate systems comes from the study of amnesia. A number of workers, including, alas, myself, have suggested that this implies a functional separation between semantic and episodic memory (Baddeley 1982a; Kinsbourne & Wood 1975). It is important in discussing this evidence not to confound the question of the distinction between semantic and episodic memory with the procedural/declarative distinction. This is a distinction which Tulving himself accepts, and one which most current theorists in amnesia would probably support. More specifically, there is abundant evidence to suggest that procedural learning may be intact in amnesic patients. Hence they are able to learn both cognitive and perceptual-motor skills, involving tasks ranging from conditioning and pursuit rotor learning through to the rapid solution of jigsaw puzzles and reasoning tasks such as the Tower of Hanoi (Baddeley 1982a).

Unfortunately, having accepted the procedural/declarative distinction, Tulving neglects to use it in interpreting the amnesia literature. He simply labels tasks which amnesics can perform as semantic and then concludes that their semantic memory performance is intact. He refers most extensively to the cueing techniques used by Warrington and Weiskrantz (1968). This typically involves presenting the subject with a word and subsequently testing for retention by presenting either the first few letters of the word or fragments of the original visual word pattern. Patients who show appallingly bad recognition memory nevertheless show comparatively normal learning when cued in this way. The most common interpretation of this is in terms of procedural learning or priming within the subject's verbal lexicon. I can see no convincing reason for referring to it as a semantic memory paradigm.

There is evidence, however, that amnesics may be able to perform conventional semantic-memory tasks just as efficiently as controls. Baddeley and Wilson (in press) investigated this recently in the case of two dense but pure amnesic patients. We found that they showed excellent performance on vocabulary tests, on generating items from semantic categories, and on categorisation and sentence-verification tasks. Surely, then, this argued for intact semantic memory, and hence for separate systems?

Once again the logic is less than compelling. Our semantic-memory tasks probed the retention of material that had been overlearned many years before, while the evidence for impaired episodic-memory rested primarily on the poor acquisition of new material. If our results did indeed separate semantic and episodic systems, then one might reasonably expect that the input of new material into semantic memory would be normal, while the recall of personal episodes from many years ago would be impaired.

The evidence for the input of new material into semantic memory is relatively clear. Amnesic patients show a conspicuous failure to update their semantic memories, frequently being quite unaware of who is the current prime minister or president, where they themselves are, or what is going on in the world about them. They have great difficulty in learning the names of new people, and in finding their way about using anything other than previously learned routes. Cermak and O'Connor (1983) report the case of a densely amnesic patient who had previously been an expert in lasers. They had him read a newspaper article on recent developments in laser technology. He was able to explain the new developments to them, but having read the article was totally unable to recall its contents or answer questions on it. It appears then that amnesics do not have a normal capacity to update semantic memory.

One could, however, still defend the concept of separate semantic and episodic systems by arguing that semantic memory requires episodic memory for its updating. The crucial case then becomes that of whether amnesic patients can recall individual episodes from the distant past. If they can, then the simplest interpretation of the data is to assume that old learning is intact but new learning is impaired.

Cermak and O'Connor (1983) report that their patient does have some difficulties in recalling autobiographical incidents. However, Baddeley and Wilson (in press) observed apparently normal autobiographical memory, with the patient able to recall details of incidents such as the weather or the colour of hair of the person involved. Zola-Morgan, Cohen, and Squire (1983) have extensively investigated the autobiographical memory of their amnesic subjects and find their retention of personal events from the distant past to be unimpaired. In short, the neuropsychological evidence indicates that amnesic patients are impaired in new learning but may have excellent recall of old memories, whether personal and episodic or generic and semantic.

In conclusion, then, the neuropsychological evidence supports the distinction between procedural and declarative learning, but does not at present provide any convincing evidence that semantic and episodic memory are based on separate neurological systems.

There is more going on in the human mind

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We used to have a hard time reading Tulving's papers in the sixties when we followed his publications closely. His style was dense and his empirical work difficult to understand. Moreover, the research issues and his conclusions were often, at least on first reading, quite surprising. The style of *Elements* differs considerably from his well-established journal style. First, he allows himself to recount anecdotes and to include autobiographical elements, which make reading the book an enjoyable experience and help us to understand the concatenation of his research ideas from the early sixties up to the present. Second, by using inserts in smaller print, he frees himself to go beyond what the rigor of scientific thought would allow. Third, by giving the convictions, beliefs, and values behind his scientific enterprise, his already published material, which is summarized in the third part of the book, becomes more coherent and understandable, and the Daedalian research work of his Toronto psychology enterprise is put in perspective. This unusual style is new, and it may even be a new method of scientific publication.

Tulving sharply distinguishes propositional and procedural knowledge and immediately adds that the propositional nature of both episodic and semantic memory is a fundamental given. Calling both types of knowledge "propositional" is at the same time entering into the debate about the nature of mental representation of our knowledge. Of course, the current trend in cognitive psychology is toward a propositional representation, but the bulk of the research findings favoring a propositional representation comes from work on semantic memory. The propositional approach to episodic memory has not received as much emphasis. Moreover, Tulving's own work does not touch this issue directly. We do not see why Tulving feels that this propositional assumption is essential to his theoretical constructs. Every event or episode has, to some extent, a semantic content, and this content may have a propositional nature. However, if episodic memory is concerned with unique, concrete, personal experiences from the rememberer's past, there must be a lot of imagery, pictorial recollections, and mental analogs to reality that escape a rigid propositional format. We acknowledge that, by a speculative "tour de force," this kind of information could be considered as perhaps belonging to procedural knowledge, saving Tulving's "fundamental" assumption. However, such speculation is not necessary, because his own work need not have pushed him to make this assumption. By not referring to the propositional nature of episodic memory, he could have circumvented the heated and, in our opinion, often sterile discussion on, for example, the propositional nature of imagery (Anderson 1978; 1979; Hayes-Roth 1979; Pylyshyn 1979). The longest-standing proponent of the position that the only type of knowledge representation is propositional quite recently converted to the acceptance of nonpropositional knowledge (Anderson 1983). Episodic memory, Tulving states, has almost no organization apart from a loose, temporal one. This position contradicts the propositional nature of the engram since propositions are generally conceived to be interrelated either by an hierarchical network or by strong associations.

The dynamic nature of the mental processes intervening during encoding and retrieval is not sufficiently highlighted. This lack of interest in encoding operations is especially surprising to us, considering Tulving's involvement (Craik & Tulving 1975; see especially p. 292: "Subjects remember not what was 'out there' but what they *did* during encoding") in the first major empirical contribution to the Levels of Processing Framework (Craik & Lockhart 1972). It must be acknowledged that there are numerous examples in the *Précis* and in the book that refer to mental operations. Tulving lists a number of operations that differ in episodic and semantic memory (see Table 1 of the Précis): "The process of ecphory is a *constructive* process." Still, one cannot avoid the impression that the human mind is conceived very statically as one unravels the details of the mechanisms in the General Abstract Processing System (GAPS). Tulving was quick to point out that there were no differences in the probability of recall between incidental and intentional learning conditions in Mathews (1977, Experiment 3). There are, however, many other findings with clear differences (see, for example, Craik & Tulving 1975, Experiments 3 and 4). Procedural knowledge *is* intrinsically dynamic but is never discussed thoroughly by Tulving. In the GAPS, explanations of encoding processes are tied to explanations of the retrieval processes. Even at the level of retrieval processes, the active nature of the subject facing the retrieval task is minimized. For example, in *Elements*, Tulving (1983b, pp. 140–41) seems to reject any kind of search processes during retrieval that are time-dependent. We wonder how he would deal with the large body of empirical work on retrieval reaction-time. There are some tables and figures reporting reaction-time data, but those data are inserted mainly to document, without elaboration, the dissociation of the two functional systems, the episodic and semantic memories. In the subject index the term "search" does not occur, either as a first-order or as a second-order entry, which is rather revealing. The process of "search" (and generation) is briefly discussed in Tulving (1983b, p. 194), but finally rejected. Some of Kolers's work (e.g., Kolers 1976a) within the "Ebbinghaus Empire" of Toronto could be interpreted as showing that retrieval of information is basically a retrieval of mental operations of the original event. Pushing the idea further, we would emphasize that encoding and retrieval imply a considerable variety of mental processes. The act of recalling is perhaps a reenactement of the mental processes during learning.

Tulving likes to use a peculiar terminology (e.g., ecphory, free radicals, conversion, and synergistic) and to refer to obscure historical events. Most people accept that scientific psychology was born in 1879, although some would set it at an earlier date with some early work of William James (see Hearst 1979). Tulving places the start of psychology in 1875 at a small meeting in London. In general, he seems to like strong statements. "Retrieval does not occur in situations in which appropriate retrieval cues are absent" (Précis), but we do not feel quite comfortable with such a position for dealing fully with free-recall data. "If some information already exists in the system, the same information is not entered again" (p. 37). Of course, episodic information is by definition always unique. Tulving (p. 70) surprisingly entertains the hypothesis that "lexical memory" is not a part of the informational content of semantic memory; nevertheless, he is willing to accept findings from lexical decision tasks as reflecting the basic operations of semantic memory (p. 68).

If Tulving does not sufficiently stress and discuss the large variety of processes and mental activities involved in memory tasks, he surely succeeds in stimulating the readers' mental activities by his provocative thoughts, unusual insights, and references to minor historical events of psychology that contradict widely accepted and oft-cited landmarks in the history of psychology.

Episodic versus semantic memory: A distinction whose time has come – and gone?

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There is much that I agree with in Tulving's book, and more with which I disagree. What I agree with most is the encoding specificity hypothesis. If one assumes that memory stores traces of events or episodes and that the retrieval of a trace requires the occurrence of information similar to that which the trace represents, it seems that the encoding specificity hypothesis – at least in its mild form – must be correct.

I also find Tulving's Synergistic Ecphory Model attractive. There is an element of recall in recognition, which sometimes allows one to base a confident "new" response on the realization that even though the retrieval cue has triggered a feeling of familiarity, it does not match the information that has been retrieved. This important insight is captured nicely by the model. I suspect that the Synergistic Ecphory Model could be mapped onto the old Hull-Spence generalization model (e.g., Hull 1943) by adding a couple of thresholds and changing some of the labels, but that does not detract from its appeal. My only misgiving about this model is that Tulving's two thresholds divide the ecphory space into just three regions (no familiarity or name information, familiarity but no name information, and both familiarity and name information), and I wonder if three cells of the 2 × 2 table are enough. In particular, there is the phenomenon called cryptomnesia, in which one recalls something but does not know one is doing so – presumably because the feeling of familiarity is missing. Access to name information without familiarity, which is what cryptomnesia suggests, seems to be contrary to the model. (Of course, one can always invoke semantic memory to handle such recalcitrant phenomena; but, as I shall argue, the episodic/semantic distinction may be one we can do without.)

If I had to point out one place where Tulving's General Abstract Processing System (GAPS) goes astray, I would say that it is focused too narrowly on the "individual act of remembering," which begins with the encoding of a memory trace and ends with its retrieval. Taking this as the starting point begs the important question of how the retrieval cue singles out the appropriate trace from among its rivals. Can the cue find its target as unerringly as Tulving's treatment suggests? Consider, for example, a case where two traces are equally compatible with the cue. Does the cue ecphorize one of the traces chosen at random? That seems too arbitrary. Or does it ecphorize both traces at once? If the latter is the case, what is the result?

The man from whom Tulving borrowed the terms "engram" and "ecphorize" also had a name for that result. According to Semon (1923), two or more similar traces that are simultaneously ecphorized are in "homophony – a kind of resonant state in which the rememberer's chief experience is of the features shared by the resonating engrams. (Actually, Semon distinguished between two kinds of homophony. In "differentiating homophony" the rememberer is able to suppress the common features and concentrate instead on the features that distinguish one resonating engram from another. In "nondifferentiating homophony," which is of greater interest here, similarities among engrams are emphasized by mutual reinforcement of their common properties and mutual interference of their distinguishing ones. Semon's writings suggest that nondifferentiating homophony is the more stable and more usual state.)

A theory incorporating homophony might be more parsimonious than the GAPS framework. It would not only address the question of how the target trace and its rivals vie for the attention of the retrieval cue; it might have two other positive effects, as well.

First, it would allow the concept of recoding to be dropped. Retroactive interference, including the esoteric variety induced by misleading questions in eyewitness-testimony experiments, may not be caused by recoding at all; it may simply reflect the simultaneous activation of the target and interpolated traces. If the information sought in the target trace conflicts with information in the interpolated trace, homophony-induced interference is likely to occur. Thus, the notion that existing traces are recoded when new, similar events are encoded may be entirely superfluous.

With regard to recoding, Tulving says this: "The concepts of ecphory and recoding are closely related, and for some purposes indistinguishable. This point was one of the major theoretical contributions of Richard Semon (Schacter [Eich & Tulving] 1978; Schacter 1982). Recoding implies ecphory, and ecphory implies recoding" (p. 152). I am skeptical not only about the claim that ecphory and recoding are so intimately related, but also about the attribution. Semon was adamant that ecphory influenced the new engram that was laid down, but I find no discussion of the recoding of old traces in Semon (1923). Nor do I find it in Schacter et al. (1978) or in Schacter (1982). Indeed, the authors of the former article make special note of Semon's silence on the causes of forgetting – and in GAPS that is the only obvious thing that recoding does.

Second, homophony might allow us to avoid the awkward episodic/semantic distinction. Although Tulving argues for as many as 28 differences between the systems, all but one seem secondary. The primary difference is that episodic memory represents temporally and spatially localized events, while semantic memory represents the abstract or generic information commonly called concepts. Now, suppose that a large number of episodic traces are ecphorized by a retrieval cue and are thereby put in a state of nondifferentiating homophony. Since temporal and spatial location attributes are unlikely to be among those the traces share, these are the features that are most likely to cancel out. What the rememberer will experience is an abstract concept, stripped of the specific details that are represented in the individual traces from which the abstract experience is derived (Semon 1923, chap. 16). In this way, a "semantic" memory can be retrieved from the episodic store.

While Semon's discussion of homophony is somewhat vague, the idea should not be lightly dismissed. I have been working with a computer simulation model of a theory that is similar to Semon's in many respects, including a retrieval process which is an information-processing analog of nondifferentiating homophony. The model has been applied not only to episodic-memory tasks such as frequency judgments and recognition, but also to the learning and representation of concepts. The behavior of the model under a variety of manipulations parallels that of human subjects to a remarkable degree (Hintzman 1983).

But can we get by without assuming different episodic- and semantic-memory systems? I liked the distinction when I first encountered it in a little book on the philosophy of memory by Don Locke (1971), which proposed three kinds of memory: personal, factual, and practical. These are similar if not identical to the episodic, semantic, and procedural memory that Tulving is proposing now. I even predicted in a textbook that the distinction would grow in importance (Hintzman 1978). But I am not so sanguine now, for two reasons. First, it appears that the compelling subjective evidence for the distinction – the remembering of temporally dated experiences versus abstract facts – might be explained more simply, as was indicated above. Second, the objective evidence we have accumulated in the eleven years since the publication of Tulving's (1972) influential paper is discouragingly weak. Nowhere is this more evident than in the present book.

A good part of the problem may stem from the lack of anything that could be called a theory, specifying what the two memory systems are like, and how they interact in different tasks. Tulving claims (p. 75) that the "logic of dissociations" allows one, in absence of a theory, to draw conclusions about whether a particular outcome supports the distinction; and he goes on to discuss several such outcomes in chapter 5. In a typical study, there are two different tasks – one judged to be episodic (e.g., recognition memory) and the other to be semantic (e.g., lexical decision), and some independent variable is reported to affect one task but not the other, or to have opposite effects on the two tasks.

But all a dissociation can do is show that at least one process is different in the two tasks; and this is something we are already fairly sure of, or we would not refer to them as different tasks. Obviously, evidence for one difference in underlying processes does not justify the claim that two different "systems" are involved. Word frequency has opposite effects on recall and recognition, but few would conclude that the two tasks must therefore be carried out by entirely different systems.

My point is this: There are severe limits on what can be learned by slapping "episodic" and "semantic" labels on tasks and doing dissociation experiments – particularly if one believes the two systems may interact. If one wants to claim that a dissociation outcome supports the episodic/semantic distinction, one must show that the dissociation is predicted by a theory that embodies the distinction. Tulving has not done this. On the contrary, he repeatedly says things that throw doubt on that entire approach. He tells us that the episodic and semantic systems interact (while saying little about when and how); that remembering the semantic content of an episode reflects operation of the semantic rather than the episodic system (p. 31); and – apparently expanding on that theme – that "psychology has not yet begun" to study episodic memory (p. 129). Since these statements imply a role for the semantic system in recognition memory, there is no reason to assume that a theory consistent with the statements would predict the dissociation results described in chapter 5.

Factual memory?

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Memory is an all-pervasive phenomenon, encompassing almost every activity that people engage in; it is not surprising that students of memory inevitably find themselves making distinctions between memories. It is the old divide-and-conquer strategy. If one cannot begin to understand memory in general, then perhaps one can divide it into small components and study these.

Tulving begins his clear, concise, and often witty book, *Elements of episodic memory*, by explicitly advancing such a strategy. He divides memory into propositional and procedural memory, and propositional memory into episodic and semantic memory, and then decides to study episodic memory. He spends the first half of his book justifying this action; in particular, he offers both experimental and logical arguments for an episodic-memory system that is functionally distinct from a semantic-memory system.

In reading Tulving's attack on episodic memory, I kept wondering what kind of territory Tulving had mapped out. Although he spends much energy on reviewing the evidence for and against a functional distinction, I was never certain that I fully understood what the terrain was like.

Tulving tries to map out the territory by listing several ways in which episodic and semantic memory are distinct. Episodic memories are more than just memories wrapped in the context in which they were acquired, and semantic memories are more than just memories for which this spatiotemporal context is absent. Tulving is not interested in just describing the content of these memories, but in exploring memory systems. Memory is usually discussed in terms of encoding, storage, and retrieval, and presumably, if episodic and semantic memory are to be functionally distinct, this distinction must be reflected in not just one but all three phases of the system. Consequently, when discussing retrieval, Tulving notes that episodic memories are accessed deliberately, are quite vulnerable to decay, and are remembered, whereas semantic memories are automatically accessed, show little vulnerability, and are known.

The territory of Tulving's battle becomes fuzzy precisely because he wants to discuss functional distinctions in terms of a system and not merely in terms of memorizing, storing, or retrieval. To the extent that there is an episodic-memory system and a semantic-memory system, one would expect to be able to look at one component alone – let's say the content of memory – and to describe the properties of its acquisition and subsequent retrieval. But many memories do not fall neatly into one system or another. At one level, they would seem to be unequivocally an episodic memory; at another level, they share none of the appropriate properties.

Consider factual memories. The other day I was playing Trivial Pursuits, a game that tests participants' grasp of mostly unimportant and offbeat facts. On the basis of content, I would classify such factual memories as semantic memories. They are more like "facts, or ideas, or concepts, or rules, or propositions, or schemata, or scripts," Tulving's list of the units of semantic memory. But throughout the game, these semantic memories behaved more like episodic memories. To concentrate on retrieval again, they were by no means accessed automatically, and given the frequency with which people lamented that they "used to know this," they were certainly vulnerable.

What am I going to be guided by in classifying such factual memories – the processes by which they are retrieved or their content? Tulving does not really guide me; it is never clear whether he should do battle with factual memories in this book on the episodic memory, because factual memories exhibit some of the retrieval properties of episodic memory, or leave them for a discussion of semantic memory, because they have the content of a semantic memory.

It may be that in the end it does not matter if I cannot classify factual memories. One can do battle with uncharted territory. It is risky business, but it is possible. Indeed, I am almost persuaded that for episodic memory it is possible. Once Tulving leaves his struggles in dividing memory into components and begins to pursue what he confidently feels is in the domain of episodic memory, a series of clever, insightful, and penetrating experiments unfold. Here, Tulving is at his best, and everyone is urged to read of his battles and triumphs.

Analyzing recognition and recall

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Not all the pleasures of reading *Elements of episodic memory* are apparent from the Précis. Absent are the inexorableness of the author's analyses, the search for historical precedent (e.g., Chalmers, Clifford, von Feinaigle, Harris, Reid), and the asides on scientific realpolitik. The summary accurately conveys, however, Tulving's concern to obtain a theoretical account of memory that is as general as possible in its ambit. Is this an appropriate goal?

The quest for generality is certainly more attractive than its not uncommon converse, the construction of theories that hardly step beyond existing data. But attempting to formulate a very general account or principle has its own dangers. The flexibility of processing met with in psychology increases the likelihood of the existence of counterexamples to any such principle. Avoiding this Scylla of contradiction tends to propel one towards the Charybdis of unfalsifiability. The present book steers an adroit passage between these twin hazards. Nevertheless, progress may be best made at an intermediate level of abstraction such that, while the theory itself is sufficiently condensed to represent a useful cognitive economy and tool, its implications for observation are direct enough to be unambiguous. An example of such a theory is provided by the retrieval independence model of recognition failure which is outlined, and which was originally proposed by Flexser and Tulving (1978).

The retrieval independence model provides an explanation for the strikingly regular empirical relation between recognition and recall that was first noted by Tulving and Wiseman (1975). In the Précis this is given as Equation 1 and illustrated by the solid curved line of Figure 4—for ease of reference it may be termed the Tulving–Wiseman law. The retrieval independence model posits that both recognition and recall involve a retrieval process that operates via the occurrence of feature overlap between probe and trace, and makes the basic assumption that the features extracted from the recognition and the recall cues are uncorrelated with each other. However, this assumption needs to be supplemented by some further ones in order to account for the law. The model possesses a number of parameters such as theoretical probabilities of extracting features at presentation, at recognition, and at recall. In the simulation referred to by Tulving, the values of each of these probabilities were indeed sampled at random, but only over the central three-fifths of their ranges. Values outside these limits may yield points far from those prescribed by the law. Given that there seems no a priori reason to limit sampling in this way, it appears that the constraints could themselves be viewed as estimated parameters, and thus the model viewed as less outstandingly parsimonious overall.

An alternative approach (Jones 1978) has shown that the Tulving–Wiseman law can be derived algebraically. The derivation is drawn from a more general account of recall (Jones 1979; 1983) which envisages two different types of retrieval, direct and indirect. This theory has the advantage of asserting that the small deviations of individual observations from the law (i.e., the scatter of the points around the solid line in Figure 4 of the Précis) are systematic rather than random in nature; the theory has enjoyed some success in predicting their disposition.

In the book's final chapter, Tulving proposes a new framework within which to consider recognition and recall, termed the synergistic ecphory model of retrieval. The framework is relatively abstract, but it is possible that its scope could usefully be extended further. Evidence adduced in its favour centres upon an ingenious study carried out with Sutcliffe. One interesting finding that this yielded, which is taken by Tulving to be crucial, concerned the relation between the efficacy of a retrieval cue and the extent to which it is falsely recognized as a target. A subject is presented with a list of single target words, such as BABY, and in the two conditions of interest is subsequently shown additional, strongly related words (e.g., *infant*) either as cues (in this case, for the recall of *baby*) or else as distractor items in a recognition test. Across these additional words, efficacy as a cue is negatively related to likelihood of false recognition. This result suggests that if subjects are able to make the covert mnemonic observation that *infant* evokes *baby*, they may thereby deduce that *infant* is not itself a target word in the recognition test. Tulving himself advances an explanation that includes a similar account. A problem in the current context, however, is that it is not clear how this account provides evidence for, or even fits within, the wider framework proposed. The account hinges upon the role of inference-making in remembering: How does this facility arise in the synergistic ecphory model?

Questions of this kind may perhaps be considered as addenda to one of the other principal attractions of the book under discussion. This is the liberal scattering of ideas for future lines of research, and indeed for whole new areas of research. An example is the topic of retrieval mode: the hypothesised state of preparedness into which a person must enter if any potential retrieval cue is to be effective. Regardless of such future developments, however, it is clear that this book already represents a substantial advance in our understanding of the organization of memory.

A fact is a fact is a fact

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In a career spanning more than a quarter century of published work, Endel Tulving has been at the cutting edge of the field of memory. He has a special knack for producing counterintuitive findings and careful arguments that, when fully appreciated, lead to major advances in theoretical development. The list of such products is long: part-to-whole negative transfer, the A + 2B effect, subjective organization, availability versus accessibility, cue-dependency, episodic ecphory, and especially the recognition failure of recallable words. Each of these findings or theoretical principles has dramatically altered the way in which we view the structure and function of the memory system. One of the results of his efforts has been the abolition of distinctions that have had broad appeal for other theorists. Early on, for example, he argued against a qualitative difference between primary (short-term) and secondary (long-term) memory (Tulving 1968a; 1970). Somewhat later, and more to the point of the book under review, he denied that there was a qualitative difference between recall and recognition (Tulving 1974; 1976). In my view his arguments have been extremely compelling, and have promoted the development of a unitary conception of the memory system. So when in *Elements* Tulving argues for a *difference* within the memory system, we are well advised to sit up and take notice.

The general case for a distinction between episodic and semantic memory is intuitively appealing. This is true even of the original argument (Tulving 1972), now described as "inchoate." The empirical evidence mustered in its favor is also extremely compelling. This applies especially to the demonstrations of single dissociations (double dissociations would be even better), where an independent variable is observed to affect performance on one type of task but not the other. In particular, the literature on clinical and experimental amnesias seems to demand a distinction between episodic and semantic memory. Nevertheless, it is unclear exactly what kind of distinction is to be drawn. Tulving wants to go beyond a mere heuristic distinction, or one that postulates different types of knowledge stored in memory. He also rejects a quantitative distinction, which would hold that episodic and semantic memories differ in terms of the number or strength of self-referent and contextual features associated with them. He appears to favor a distinction rooted in biological structure, as if episodic and semantic memories resided in separate locations in the brain, or consisted of separate, parallel, networks of neurons. In this regard, it is worth remembering that the amnesic syndromes, now used by Tulving to suggest a structural distinction between episodic and semantic memory, were used not too long ago to support a structural distinction between primary and secondary memory. Many theorists now favor a unitary model of memory, in which primary memory comprises those memory structures which are activated at any given moment.

Why not opt for a similar solution with respect to the episodic/semantic distinction? Assume that a declarative memory can be characterized as a bundle of features describing an object or event. Such a memory can be portrayed graphically as a set of nodes representing concepts interconnected by associative pathways representing the relations between them to form propositions. Some of these propositions represent semantic knowledge about similarities (e.g., *Grenada is like Afghanistan*), category membership (e.g., *A robin is a bird*), characteristic attributes (e.g., *Birds have feathers*), or other facts (e.g., *A hippie touched a debutante in the park*). Others represent episodic knowledge about personal experiences in which propositions describing some event are linked with others representing the self as actor and experiencer, and the spatiotemporal context in which the event occurred – e.g., *I saw a bird in the park on Thursday afternoon* (Kihlstrom 1984; Kihlstrom & Cantor 1984). According to this argument, the concepts out of which episodic memories are formed are the same as those that comprise semantic memories, but the propositional links are different. Thus, a single memory system can represent both episodic and semantic forms of knowledge, and one is not led to search for anatomical or physiological correlates of the difference between them. Such a proposal does not seem to rely on a hypothesis of associative continuity, in that the associative links involved in episodic and semantic memories are different. But it does assume the transsituational identity of the underlying conceptual nodes.

Perhaps the most compelling experimental evidence in favor of a unitary theory of memory comes from the very experiments Tulving cites as revealing the operation of two separate systems. Typically, there is a dissociation observed between episodic and semantic tasks, which is the primary evidence for two separate systems. But this is also accompanied by a priming effect on the semantic task stemming from the (episodic) study phase. A similar difficulty is presented by free radicals in memory, bits of semantic knowledge, or beliefs, which have their origin in some particular experience but which have lost the self-reference and contextual features that would give the memory episodic nature. Tulving recognizes the problems created by these findings, as they seem to imply that an episode of experience has affected the contents of semantic memory. His appeal to procedural memory as the mediator of the priming effect, and his suggestion that free radicals comprise yet a third form of declarative memory, both have an ad-hoc quality. It would seem much simpler to suggest that episodic memories are formed from semantic memories representing the features of the event, the self, and the situational context. A failure to encode, store, or retrieve the self-referent or contextual features, whether through normal forgetting or some amnesic process, would result in a performance deficit on an episodic-memory task; but the residual activation of the underlying conceptual knowledge would result in temporary facilitation on a semantic-memory task. Similarly, a novel experience would lead both to the formation of a proposition describing the new fact and a linkage between this fact and the personal context in which it was acquired. A failure to encode or preserve these episodic features would have no effect on the status of the fact itself as a new entry into semantic memory, which could then be accessed in the same way that any other semantic memory is retrieved. I admit to difficulty accounting for long-term, modality-specific priming effects. Perhaps these are procedural in nature, though procedures shouldn't be modality specific.

In arguing for at least a functional distinction between episodic and semantic memory, Tulving asserts that the two systems can operate independently, although it is more efficient for them to coordinate their activities. But it is difficult to understand how an episodic memory could *ever* be encoded without contracting the concepts in semantic memory that correspond to the features of the event. Such an encoding must involve linking self-referent and contextual information either to the semantic memory node itself or to a copy of that node stored separately from the original. Despite Hilgard's (1965, p. 460) dictum, parsimony would seem to favor the former alternative. The desire for parsimony must be frustrated by the distinction between declarative and procedural memory (Anderson 1983; Winograd 1975a) – because, as Tulving notes, the former has a propositional representation and accessibility to consciousness while the latter does not. The episodic/semantic distinction within declarative memory undoubtedly has heuristic value, providing a useful means of categorizing the kinds of information stored in memory and supplied by queries to the memory system, and the kinds of retrieval tasks to which the rememberer can be put (Cantor & Kihlstrom 1982; Hastie & Carlston 1980). But there doesn't seem to be any need to argue for two *separate* propositional systems when one will do. Semantic memories are facts about the world. Episodic memories are facts too, about the self. Facts are facts, and they all ought to be representable within a common pool of declarative memories.

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Armchair theorists have more fun

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Imagine a person who perceives the world in black and white and suddenly discovers that the rest of the world sees colors. The response may not be very different from that of psychologists studying human memory with traditional list-learning procedures when they read Tulving's 1972 article on semantic and episodic memory. It now seems obvious that subjects who repeat back a list of words are remembering not the words, but their occurrence in an autobiographical episode. But what seems obvious now was not then, and to some readers at least, the episodic/semantic distinction suggested that half of human memory remained unexplored despite decades of contemporary investigation.

In making this point clear, Tulving's depiction of semantic and episodic memory has had obvious heuristic value. But in *Elements* he argues that it has more: that it represents a distinction between two systems of memory with the potential for independent function. Unfortunately, the evidence for the dual-systems approach to semantic and episodic memory is far from unequivocal.

The still unresolved debate about imagery and propositional knowledge should have taught cognitive scientists something about the perils of duality assumptions. [See Pylyshyn: "Computation and Cognition," *BBS* 3(1) 1980 and Kosslyn et al: "On the Demystification of Mental Imagery," *BBS* 2(4) 1979.] The present case is particularly problematical because it is unclear what is meant by dual memory "systems," especially when the proposal comes from someone who has long criticized the tendency of theorists to divide memory into boxes. The experimental evidence Tulving reviews seems to consider two potential bases for separating the memory systems: It should be possible for one to operate without affecting the other (a lack of transfer), and there should be some variables that influence the systems in different ways. But as Tulving concedes, virtually any experimental evidence along these lines can be interpreted in terms of a unitary theory of memory, in which distinctions are made between semantic and episodic *knowledge*, semantic and episodic *tasks*, and/or semantic and episodic *decision rules*.

On the basis of the experimental work described, it seems doubtful that anything more is needed than a content distinction between semantic and episodic memory. With the straightforward assumption that "Episodic information is picked up by the learner on a particular occasion, at a particular time in a particular place, and . . . semantic information has no such association with a particular occasion of acquisition" (p. 63), other distinctions follow without the need for postulating dual systems. For example, episodic tasks probe for information about the acquisition context whereas semantic tasks do not, providing ample potential for differential effects of experimental variables.

At first glance, at least, some of Tulving's "armchair arguments" for separate systems seem more persuasive. One in particular concerns the nature of the conscious experience of remembering. To remember semantic knowledge is to have a feeling of knowing, but to remember episodic knowledge is to reexperience. One is cold cognition, the other hot. Somehow this is not captured by models of memory in which episodic and semantic knowledge are distinguished solely by the presence/absence of associations to contextual information. Why should the mere presence of context change the phenomenological experience of retrieving information from memory?

A better account of phenomenological differences between remembering facts and remembering events may lie in considering the nature of the retrieved information. For example, Johnson and Raye (1981) have suggested that certain elements in the traces of past events are particularly useful in evaluating whether those events were real or imagined. These include not only information about spatial and temporal context, but also the sensory quality of the memory trace, its semantic elaboration, and records of how it was encoded. Although these data appear to be represented to different degrees in the traces of real and imagined events, the critical point here is that to some degree they are properties of episodic representations in general. If the activation of such information in episodic traces (the ecphoric component of memory retrieval, as Tulving terms it) were to simulate the perceptual, semantic, and affective reactions of the initial experience, remembering would have the "warmth and intimacy" that William James attributed to it. *Episodic* remembering would, that is. The retrieval of semantic information, lacking the record of a particular encoding circumstance, would be a considerably more barren experience.

Note that differences in the experience of remembering episodic and semantic information do not require the assumption of separate systems. The above hypothetical account, which attributes phenomenological differences to the content of what is retrieved, does require considerable speculation about the nature of the information in memory episodes and the effects of activating that information. Nonetheless, pushing the semantic/episodic distinction along these lines seems more promising than trying to justify a new taxonomy of memory.

The episodic/semantic continuum in an evolved machine

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Tulving's *Elements* is many things. It is a superb, if disguised, treatise on the philosophy of science. It is a uniquely informed scientific history of the field of memory, including a clear and concise synopsis of the author's considerable scientific accomplishments and a capsule view of the most influential empirical findings in memory research during the last two decades. Finally, and perhaps most important, it presents a general pretheoretical system – General Abstract Processing System (GAPS) – for the study of human long-term memory.

As a philosophy of science, the book illuminates the processes of "consensual validation" in contemporary psychological inquiry. It provides cogent suggestions for substituting procedures of "canonical validation" for certain habitual pradigmatic predilections. Put in a somewhat different way, Tulving provides guidelines for converting many of the irrational practices of the field into rational ones.

As a history of memory research, Tulving makes a major contribution by integrating the data and theories from cognitive psychology's investigation of memory with those from developmental psychology and brain-damaged patients. As we have previously argued (Naus & Halasz 1979), most recent models of memory can be seriously criticized for conceptualizing memory as a static system rather than as a dynamic, evolutionary system as is suggested by changes that occur during childhood and old age. Naus and Halasz have further argued that a developmental perspective helps to clarify a number of issues, such as the relationship between structure and process in short-term memory and the distinction between automatic and controlled memory processing, which may prove difficult to resolve through an analysis of the asymptotic performance of the adult memory system. Along these same lines, Tulving argues that both the developmental and brain-damaged literatures help to establish and define the episodic/semantic distinction. While we might quibble with some of the details of Tulving's review of these literatures, and even the conclusions that he derives from them, we are very encouraged by his integration of these fields into the domain of the memory psychologist's investigations.

As a pretheoretical system, Tulving's introduction of GAPS represents the next logical extension of his work toward the development of an overall theory of the nature of long-term remembering. GAPS elaborates and strengthens Tulving's encoding specificity hypothesis in light of recent empirical findings by reviving the concept of "ecphory" from an earlier era of the discipline and extensively elaborating upon the basic notion of the interaction between the memory trace and the eliciting cue. In this context, the notion of ecphory seems to have both an intuitive appeal and the capacity to integrate a number of disparate ideas and empirical findings. While we expect that memory researchers will spend a generation testing and refining this conceptualization, we wish that it would have been possible for Tulving to formalize his concepts and the interrelationships among them.

Tulving's book's major contributions in the above three areas can only be appreciated by a thorough reading of the text; we are in disagreement with the author, however, regarding his insistence upon the necessity of postulating two distinct long-term memory stores. He accords ontological status to the distinction between episodic and semantic memory. We are puzzled by his insistence on maintaining this strong claim, especially when it seems unnecessary to a presentation and defense of the concepts in the GAPS system. Although we find the semantic/episodic distinction heuristically useful, we are unable to accept it as having structural correlates. We believe that all memory starts as experience which immediately commences to shed the contextual accompaniments of the input experience. Any memory can be located somewhere on a trajectory from highly episodic (in the sense that autobiographical markers are intimately intertwined in the memory experience) to highly semantic (in the sense that the memory experience does not incorporate spatial and temporal information).

Why should all memories be in the process of becoming semantic? Consider a general-purpose machine, one that has achieved its domination of the earth by virtue of its generality of purpose. It is relatively small, usually under six feet in height. It is comparatively weak; it is slow-moving. Its sensory acuity ranges from mediocre to poor among its animal colleagues. It has no unique adaptation such as tusks, or quills, or camouflage. What does it have that has given it such survivability? It can solve a potentially infinite range of problems in its efforts to survive and reproduce its kind. That is the meaning and the significance of "generality of purpose." Our conviction is that an evolved general-purpose machine needs a memory system that strips the memory representation of episodic information at the optimal time.

Why should a general-purpose problem-solver divest itself of the episodic particularities of its experiences? The answer is obvious. Generality of purpose requires abstraction, and episodic information is necessarily concrete. It is not helpful for our general-purpose problem-solver to remember that, at the last full moon, over by the tree with the broken branch, near the small waterfall, a large animal having black and yellow stripes killed its brother. It does not afford a great adaptive advantage for it to recall, as an independent and unrelated memory, that a large animal having black and yellow stripes caused serious screaming on the part of its father on a particularly hot day in the large meadow while the blue flowers were in bloom. On the other hand, it is extremely useful for this kind of problem-solver to retain the knowledge that, wherever and whenever encountered, large animals having black and yellow stripes are to be avoided. In other words, an evolved general-purpose machine excels at extracting abstract principles of broad and enduring utility from its limited set of concrete experiences. That is what it does best; that is what its adaptive history has equipped it to do. For such a system, the most characteristic use of its intellect is to convert its episodic experience into semantic memories. Its natural proclivity is to strip its world knowledge of all arbitrary contextual information that has no relevance beyond the circumstances of the stimulus input. It is very hard to prevent this kind of system from following its natural proclivities.

Not that it hasn't been tried. Verbal-learning psychologists have made a high art of developing situations designed to make success contingent on the subject's ability to overcome his natural tendencies to abstract. The rote learning experiment is an exercise in counteradaptation. What works to keep the wolf from the door, so to speak, will not lead to successful performance in traditional verbal-learning studies. One is hard pressed to think of naturally occurring situations where an organism is required to group and retain a set of perceptions having no adaptive utility whatsoever, the only salient properties of which are the fact that they occurred at a particular time and place (in the experimental laboratory at the appointed day and hour). The rote learning experiment may be unique in this regard.

Nevertheless, memory of episodes is an absolute requisite of the process of abstraction, because it is generally not adaptive to abstract from single incidents. The well-adapted general-purpose problem-solver should not divest itself of arbitrary and irrelevant episodic data until it has accumulated enough episodes to know what is arbitrary and irrelevant. Remember the animal who observes serious screaming by its father in the presence of a large, yellow-and-black-striped animal on a hot day in the large meadow while the blue flowers were in bloom? Later experience may identify the salient features of this experience as the animal (a treacherous beast) or the meadow (a dangerous place) or the season (a hazardous time). Accordingly, a system that is designed to convert episodes into semantic principles as soon as possible should nevertheless have the capacity to retain unanalyzed, unabstracted, and ungeneralized information for a while, just to see if it fits into a useful pattern somewhere along the line. Indeed, the countless college sophomores who have served faithfully in verbal-learning experiments prove beyond peradventure that the human intellect has this capacity, even if it isn't altogether fun. The astute student of human mentation, observing this undoubted capability, might naturally perceive in it a memory of a qualitatively different kind from generalized semantic understandings. However, the observer with a different starting point might see in this capacity merely semantic memories aborning – putative abstractions awaiting enough company to metamorphose into the generalizations nature intended them to become.

We recommend that interested readers not stop with the *Précis*, which promises much less than the book delivers. Despite the fact that our view of semantic and episodic memory is very different from Tulving's, few other recent monographs have so stimulated and entertained us. Tulving has always been a brilliant and articulate representative of his discipline, and this book is true to form.

Recoding processes in memory

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As with so many of Tulving's contributions, the broad observations and provocative thoughts contained in *Elements* are certain to influence the future direction of memory research. In brief, Tulving's new book offers an expansion and defense of his 1972 distinction between semantic and episodic memory. In addition, he presents a framework for studying episodic memory, called GAPS for General Abstract Processing System, that outlines a general set of episodic-memory principles. We describe a few kernels of GAPS wisdom that are particularly meaningful to our own research program.

As Tulving correctly notes, one of the pervasive facts about episodic memory has to do with changes over time in recollection of an event. Some changes are due to retrieval conditions, while others are a result of a mutable engram (p. 164). Changes in the engram occur as a result of a variety of processes, and the term "recoding" describes these processes. Recoding operations are central to the functioning of GAPS.

We are sympathetic to the notion of recoding. Over the last several years we have conducted experiments in which subjects view a complex event and are subsequently exposed to new, often misleading, information about the event. For example, they may see a car pass a yield sign but are subsequently "told" it was a stop sign. Under certain conditions, a substantial number of subjects will recall having seen a stop rather than the yield sign. Even strong incentives have failed to produce the original information from memory (Loftus 1983). We have interpreted these results to mean that information to which a witness is exposed after an event is integrated into the witness's memory. In the terminology of GAPS, the original event has been recoded as a consequence of the postevent input.

A question arises as to the fate of the original engrams. Some have suggested that, once formed, they are never changed. The new inputs simply provide additional memory traces which, under proper conditions, can be discriminated from the original ones. However, our view and a strong implication of the recoding process as envisaged in GAPS suggest that after certain modifications of the original engrams, it should not be possible to utilize the information that was originally contained in memory. To show the "recoding hypothesis" to be incorrect can be done quite easily: All one needs is to demonstrate the existence of original information after recoding has allegedly occurred.

Tulving correctly anticipated that attempts to falsify the recoding hypothesis would be forthcoming: Two such efforts have now been published (Bekerian & Bowers 1983; Christiaansen & Ochalek 1983). In these studies, subjects were able to dissociate misleading postevent information from original information even when the postevent information had presumably already been recoded. In one of these studies the critical manipulation involved context reinstatement just prior to the final set of recall; in the other, the critical manipulation involved a strong warning about the presence of erroneous information. It too was given just *prior* to final recall.

Are we, and GAPS, wrong then, about the fate of recoded information? We think not. With the aid of two ideas – the "free radical" and the "conscious act of retrieval" – we may be able to distinguish between those situations where it is possible to recover original information and those situations where it may not be possible. What appears crucial is whether the critical manipulation occurs prior to conscious recollection or afterward.

Why might conscious recollection be important? Suppose the postevent information leaves a "free radical" or "free fragment" in memory (p. 112), that is, a bit of episodic information detached from the rest of the memory for the episode. Tulving believes that, like free radicals of the chemical world, these bits of memory are highly reactive and unstable. It may be that at the time of the final test, these fragments become laminated, via the act of conscious retrieval, to the memory for the episode. Accordingly, prior to conscious retrieval it is still possible to separate these bits from the original engram whereas afterward it may be exceedingly difficult if not impossible to do so. Recent experiments in our laboratory (Schooler & Loftus 1983) provide support for our proposition that once subjects consciously retrieve a piece of misinformation, they are not readily induced to recover the original engram. Prior to conscious retrieval, they are. In short, we propose that bits of misleading postevent information may exist as free fragments until a subsequent act of retrieval completes the recoding process.

Tulving is sympathetic to the important role of conscious recollection. For him, the act of retrieval is an "event-like mental activity" with many "empirically identifiable consequences" (p. 140). For example, it increases the probability that the event will be recalled on a subsequent occasion.

How "conscious" must the act of retrieval be? We agree with Tulving's emphasis on the distinction between conscious and nonconscious processes. Conscious processing certainly is needed to recall episodic information; that is, by definition, retrieving an episodic memory must include a conscious awareness of the temporal and spatial details associated with its encoding. Other recent research tells us that one's reactions to information can be different depending upon whether it is processed consciously or subliminally (Marcel 1983). Yet questions like (1) how consciously does one need to process postevent information in order for a free fragment to be laid down in the episodic system? or (2) how conscious must the act of retrieval be in order for the recoding process to be maximally completed? remind us that the notion of conscious mental activity is really a matter of degree.

Our hope is that investigators of episodic memory will take Tulving's distinctions as a starting place and run with them. One direction is to distinguish further among the various types of episodic memory. Hertel (1982) has already suggested that thematic episodic memory may differ from memories for specific episodic details. More specifically, she observed that misleading postevent information regarding a specific episodic fact has its greatest effect when it is presented some time after an original event (and just prior to the conscious recall). On the other hand, misleading postevent information regarding the theme of an episodic memory has its greatest effect when it is presented immediately after an original event (and some time prior to conscious recall). This result provides support for a way of further differentiating episodic memory and extending the fruitful work that Tulving has begun.

Inference and temporal coding in episodic memory

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In *Elements*, Tulving repeatedly describes episodic and semantic memory as "functionally different yet closely interacting" (p. v). They are neither "completely separate" nor "sharply different" (p. 32) and, in fact, have many similarities. The crucial points are: (1) that they can operate independently of one another (albeit less efficiently); and (2) that the episodic system more directly *registers* information about events (and about their temporal relations in particular), the remembrance of which eventually becomes peculiarly central to self-understanding in that it informs an individual's sense of personal history. Tulving insists that all events are unique (presumably in virtue of their spatial and temporal coordinates) and that the "organization of knowledge in the episodic system is temporal" (p. 38). It stores information about the temporal relations of events in "the rememberer's personally experienced time" (p. 39). This more direct registration, apparently, minimizes the role of inference in their retrieval. Tulving's remarks about these autobiographical events and their mnemonic manipulation, though, seem problematic on certain counts.

The directness of episodic encoding is unclear in the absence of any criterion. Tulving's account is at least tacitly inferential. The encoding of a particular event includes references to its temporal relations with "other similar or related events" (p. 42) previously encoded in episodic memory. Inference, though, must surely underlie the recognition of most interesting varieties of event relatedness.

That episodic memory is a part of propositional memory would seem naturally to engender expectations about the role of inference in its operations. Tulving asserts, however, that the inferential capability of the episodic system is "relatively limited," as our knowledge of events' "contents and temporal dates to other events need not be deducible from other knowledge" (p. 43). Yet he also holds that gaining access to the contents of episodic memory is a deliberate process (p. 46). In fact, we often ascertain the time of past episodes in our lives on the basis of conscious inferences – even when we are otherwise clear about their crucial autobiographical contents. Also, these inferences typically concern only the autobiographical contents of other related events. (This coherence among the autobiographical contents of events in the episodic store is, presumably, the source of their *personal* significance.) Either of at least two situations demands such inferences. Quite often, we do not encode temporal (and even spatial) dimensions of episodes, because they are irrelevant to the significance those episodes hold for us personally. But even more frequently, we do not (and cannot) anticipate at the time of an event's encoding the significance that will accrue to it in virtue of relationships it has with various *past* epiodes – relationships which become salient only as a function of events we have yet to experience. Consequently, the episodic system will fail to encode the relevant temporal relationships with these other episodes that will eventually prove to be significantly related. Rather than being minimally inferential, episodic processing seems, at least some of the time, to be essentially inferential.

Since Tulving does not indicate that the encoding of events in the episodic system is exhaustive with respect to their temporal relations to (all) other events previously encoded in that system, he must argue that "whenever episodic information is retrieved through inferences, it turns out that inferences are made on the basis of knowledge of the world" (p. 44), that is, on the basis of the contents of semantic memory. This out, however, substantially obscures the sense in which the episodic, but not the semantic, system is peculiarly autobiographical, and it minimizes the role of episodic memory generally, if such inferences as these, based exclusively on the autobiographical contents of other related events, fall into the domain of semantic memory.

Since Tulving accounts for ecphoric information in terms of the compatibility of feature bundles stored in the engram and extracted from the retrieval cue, his insistence on the uniqueness of (the encoding of) events, the directness of that encoding, the essentially temporal organization of the episodic store, and the limited role of inference in the functioning of the episodic system seems either irrelevant (in the cases of the first three) or unimportant (in the case of the last) with respect to his synergistic threshold model of retrieval. This is especially true if he adopts the extremely restricted account of the contents of episodic memory alluded to at the end of the previous paragraph. In addition, it is unclear (given these considerations) why that model does not apply equally well to retrieval in semantic contexts.

To summarize then, Tulving reviews plenty of experimental evidence for distinguishing the episodic- and semantic-memory systems for lots of purposes. On that basis he drives a sharp theoretical wedge between the factual content and the autobiographical dimensions of events, between the focal element and the setting of events (pp. 148–49), and between the semantic contents of events and the events themselves. He proposes a basically noninferential account of processing in a temporally ordered episodic system. Yet often inferences are necessary to discover even the temporal relations of episodes. So it would seem that Tulving must either construe the episodic domain so narrowly (in order to minimize the role of inference) as to seriously limit its interest or he must further restrict both his theoretical account of the episodic system and his specific model of its operations to plausibly confine their application to episodic phenomena alone.

My conclusion, though, is not negative. Tulving has provided an extremely suggestive explanatory sketch for two subsystems of human propositional memory – subsystems, however, that apparently share a number of principles and processes. This is not inconsistent with either his claims about their independent functioning or (if he takes a liberal view of the episodic domain) about any sense of direct registration and temporal organization in the episodic system – at least until he further clarifies those latter claims.

The episodic/semantic distinction: Something worth arguing about

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Tulving (1983) has produced a serious extension of his original idea. He faces the fact that the memory we use from day to day has to bear *some* relation to the memory used in the laboratory, even in the old verbal-learning experiments, and he creates a framework in which this range of phenomena can be encompassed. It is still a modest scheme, with only nine elements relating to internal processes and states, and it is a sad commentary on his (probably accurate) assessment of his audience that he fears lest "anyone despairs of the complexity inherent in [this] scheme . . ." (p. 138). Why should we expect psychological models to be simpler to understand than, say, biochemical cycles?

Tulving's achievements include a very clear differentiation between two kinds of knowledge. Our first challenge is whether or not such a differentiation justifies a separation into two *systems*. If the two kinds of knowledge were in a single system their very nature would strongly influence their properties, the conditions under which they are stored, the ways in which they can (or must) be retrieved or used. The advantages of having a single memory system instead would include, first, that the properties of different kinds of knowledge would not have to be ascribed to the systems in the rather apologetic way the data require. Thus, many of the initially distinctive attributes of the two systems become qualified with "more" and "less." We discuss this below in relation to affect. The second advantage of a single system is that the distinctions are allowed to be blurred. Thus, to take one of Tulving's examples (p. 42), suppose a student is told that Freud was born in 1856 and a week later learns that Pavlov was born in 1849, examples of "propositions entailing temporal relations in semantic memory." We might discover that the memory for this information behaves either like episodic or like semantic memory. This is alright because episodes can contain "semantic content," which can be treated, for instance, for inferential purposes," on the same basis that applies to semantic memory" (p. 43). It doesn't worry us that one wouldn't, by a single task, be able to determine which system some information was in, but with a single system one might be able to do without equivalent qualifications.

However, although we support a single memory system, with one retrieval method (using different kinds of retrieval cues for different kinds of information), we suspect that we are more in agreement with the spirit of Tulving's proposals than most single-memory theorists, particularly when they treat all knowledge as equivalent. More serious disagreement occurs when it comes to the specified properties of the memory systems, and it is to some examples of these that we now turn.

The modification of memories. "Mutability is one of the distinctive characteristics of engrams of events" (p. 164). Tulving is careful here; he is talking about "functional properties." The recoding process he envisages "that bring[s] about changes in the engram" (p. 164) implies that "utilisation of certain information originally contained in [the engram] should not be possible after encoding has taken place" (p. 168). He conjectures that attempts to test the hypothesis "will undoubtedly be forthcoming." In fact, they have forthcome.

Tulving cites as experimental support for his thesis the work by Loftus and her colleagues (e.g., Loftus, Miller & Burns 1978). Loftus's major finding is that subjects who are given inconsistent postevent information about details of a previously seen slide sequence or film will be misled and will erroneously remember the inconsistent details as having been in the original sequence.

However, the fact that subjects can be misled by subsequently presented inconsistent information does not require the assumption that memories are modified, or "recoded," as Tulving puts it. Our view, based on the assumptions of a recently formulated model (Morton, Hammersley & Bekerian 1980), would be that memories cannot be modified. Difficulties in recalling the original information are due to difficulties at retrieval. We claim that when subjects are given postevent information, a new memory record is formed which contains the inconsistent information. This record coexists with the memory record for the original event. At the time of retrieval, subjects will form a "description" (see Norman & Bobrow 1979) that searches memory for the information to be retrieved. Under most circumstances, as in Loftus's studies, we assume that the description will be biased to retrieve the most recent, relevant Headed-Record. However, it should be possible to override this tendency. What will be retrieved, then, will ultimately depend on the conditions existing at the time of recall (in line with Tulving's own position on many other issues).

A study by Bekerian and Bowers (1983) shows that we are right. Bekerian and Bowers noted that in all of Loftus's studies, subjects were presented with test items in a random order with respect to the original sequence. They argued that randomizing test items might prevent the formation of a description that would match the record containing the original information. In order to test this possibility, Bekerian and Bowers manipulated the order of test items: One group received the test items in a random order; the other group received the test items in a fixed sequence (i.e., an order that matched that seen during the original presentation). When subjects were given items in a random order, the misleading effects of inconsistent postevent information were found, as in the Loftus studies. However, when given test items in a fixed sequence, a vast majority of subjects responded with the accurate information and did not show the misleading effect of postevent information. These findings, as well as others (Bowers & Bekerian 1984), are predicted by our Headed-Records model and support the notion that, once encoded, memories cannot be modified.

The absence of affect in semantic memory. In his discussion of affect, Tulving states that "it makes sense to assume that only episodic memory has affective components, or at least that affect plays a more important role in the episodic than in the semantic system" (p. 42). The heavy qualification in the second clause of the strong claim in the first is symptomatic of the attempt at strict separation of the systems when faced with contrary data. Reviewing some of the evidence will help to clarify the issue. For example, Teasdale and Russell (1983) have shown that mood can affect the recall of words varying in their affective connotations. Subjects learned a list of positive and negative words while in a "normal" state. If subjects were then induced to be in a happy mood at recall, they retrieved more pleasant items from the stimulus list; if subjects were induced to be in a sad mood at recall, they remembered more unpleasant items. A similar point is raised by the findings of Bower and Gilligan (1979). Positive trait adjectives were remembered better if they were judged in reference to the self rather than for meaning or for sound. The "self-referent" condition and its influence on the memory for affectively loaded traits clearly cannot be viewed as operations solely within the episodic system. Tulving could argue that such findings are the result of an interaction between semantic and episodic memory systems and leave the semantic system (or, more precisely, semantic *knowledge*) totally free from affect. In this way, his account would approach our own single system account.

Memory in young children. "The absence of episodic memory in young children" (p. 50) involves a curious assertion. It seems to depend on a strict requirement for including in episodic memory the maintenance of temporal organisation in memories. Yet memories, in the sense of stored, usable information concerning events, can be found in young children. Thus, Barrett (1983) has found that the use of words in a child of twenty months is tied closely to complex, repeated events. Thus "duck" was initially restricted to when the child was knocking a yellow toy duck off the side of the bath at bath time. To use some of the criteria in Tulving's Table 1, the source of this seems to be sensation rather than comprehension; it concerns an event, it has self-reference, it is context-dependent and vulnerable (a month later the use of the word had generalised). Maybe we need a third, early, system to deal with such phenomena.

Coda. The points we have raised are not crucial. They merely chip away at some peripheral aspects of Tulving's framework. His book sets standards for the serious critic who will have to provide an alternative view with the same scope and more utility. We hope one is forthcoming.

Bridging gaps between concepts through GAPS

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There is no doubt, that *Elements of episodic memory* will be an important conceptual source book. In my opinion it is a fundamental contribution to memory research; it is rich in fact and theory, and provocative in speculations. It provides a most enjoyable reading experience in many respects.

For many years Tulving's most important contribution to the science of memory has been as a founder of basic conceptual tools. This tradition is continued in the book and the ambitions are extended to incorporate the main concepts into a general framework – General Abstract Processing System (GAPS).

At a general level GAPS can account for many empirical phenomena, but I count it as a disappointment that no unique predictions can be made on the basis of the framework. Tulving could, of course, reply to this by saying that this was never the purpose: Frameworks should not make predictions, they should be heuristic. In my view such answers are not satisfactory. The heuristic value per se is far too often seen as the important asset in current theoretical formulations. This is unfortunate because it constitutes such a poor instrument for evaluating theoretical formulations. I am less than sanguine that we will ever be able to determine the scientific value of broad frameworks such as GAPS.

For some reason those theoretical views that are said to be high in heuristic value are often closely related to common sense. To some extent this is true for Tulving's conceptualizations as well. There is a great danger in this. The history of science has shown that almost all intellectual achievements of lasting value are those that are not immediately transparent. Tulving is also aware of this fact and states explicitly, that "given the choice between two otherwise equivalent ideas, the one that fits less readily into what we already know may be preferable" (Tulving 1979a, p. 31).

In contrast to the common-sense nature of the general formulations of GAPS and also the episodic/semantic memory distinction, there are indeed theoretical principles in the book that do not concur with common sense but go along very well with a massive amount of empirical data. Probably the best example of this is the theoretical principle emanating from the recognition-failure phenomenon. This phenomenon and the recognition-failure function expressed by Flexser and Tulving (1978) are not at all immediately transparent, but it is an intriguing phenomenon and the function summarizes an impressive amount of data that tell us about something we certainly did not know before.

With respect to GAPS in particular I am disappointed by Tulving's reasoning in a few basic regards. It is clear that GAPS describes a relatively passive psychological system. In view of this I would have expected, for example, a more neurobiological orientation than is at present exhibited. This is not to say that in general a neurobiological approach would have been the only appropriate one. However, once one has decided to talk about psychological processes it seems inadequate to deal with these as primarily passive in nature, with homunculuslike agents to govern various aspects of the act of remembering. We certainly know from much current research that active subprocesses are involved in the act of remembering. One cannot help wondering whether the 13 elements of GAPS, accompanied by the encoding specificity principle, are enough to account for the various forms of retrieval in which reconstructive processes are prominently involved. The postulate of a homunculuslike "memory system" is not a convincing way to avoid the passivity of GAPS.

I fully agree with Tulving that it is more appealing to emphasize the interaction between encoding and retrieval than it is to view these separately. However, I do not think that the specification of all the elements of GAPS is the best and most parsimonious way to argue for this. Really, the "encoding box" and the "retrieval box" are describing the same basic observables, processes, and states, and there is no need for a separation of these two main boxes. What we need to specify is the observable event, the particular demands of that event, and the cognitive environment. The rememberers register the observable event in light of these demands and their previous knowledge and experience. As an example of the advantage of such a view in comparison to GAPS, let us consider what happens when there is no recollective experience of a certain encoded event. In such a case the subject may still manage to report the correct event on the basis of discarding available response alternatives. Since there is no ecphoric information, GAPS cannot account for the correct response made. This is possible when emphasizing the particular task demands in relation to the event per se and the cognitive environment of the rememberer.

Although the recognition-failure phenomenon has been mentioned in positive terms, it should be added that in my opinion Tulving deals too easily with the data showing deviations from the recognition-failure function (e.g., Begg 1979; Gardiner & Tulving 1980; Nilsson & Shaps 1980; 1981). One can only wonder how much of a deviation or how many studies showing such deviations one would need in order to say that the data obtained have invalidated some basic aspects of the recognition-failure function.

Tulving's book will be read and discussed by many, from quite different theoretical perspectives. I have discussed here only a few of its aspects. For obvious reasons commentaries of this sort are usually dominated by critical remarks. However, the few negative notes I have sounded are indeed slight in view of the great accomplishment of the book as a whole. It is certainly easier to criticize an effort of this magnitude than to produce it. The book is a great achievement and Tulving deserves all the credit he will get.

The source of the long-term retention of priming effects

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Chapter 6 of Tulving's *Elements* concerns priming effects. I am very interested in the long-term retention of priming effects. This has impelled me to do some experiments with it.

Tulving, Schachter & Stark (1982) ran an experiment concerning priming effects using word-fragment completion tasks. The results were as follows: (a) Although recognition accuracy was greatly diminished over the intervals from 1 hour to 1 week, priming effects were unchanged; (b) at the level of individual test words, primed word-fragment completion performance was uncorrelated with episodic recognition. These investigators concluded that priming effects in word-fragment completion were independent of recognition memory. They then discussed the interpretation of the long-term retention of priming effects in terms of memory systems such as episodic memory, semantic memory, etc.

According to Tulving et al. (1982), it is certain that the long-term retention of priming effects cannot be regarded as phenomena of episodic memory. Tulving et al. doubted that priming effects were mediated by the semantic system. Finally, they considered procedural memory, not propositional memory, to be the source of priming effects; however, they were still uncertain about this.

If the long-term retention of priming effects does not come from episodic, semantic, or procedural memory, what brings it about? Tulving has suggested a very new idea: "free radicals."

The long-term retention of priming effects not only in word-fragment completion tasks but also in other verbal, motor, and perceptual tasks has already been shown in the past. For the present, my colleagues and I have made a Japanese version of the word-fragment completion tasks. We have completed several experiments in order to extend Tulving's results and specify their source.

We have confirmed that priming effects can be attributed to episodic memory for only a few minutes after presentation of primers, but they are independent of episodic memory after that. We found clear priming effects 5 weeks later, although Tulving dealt with priming effects 1 week later. We therefore generalize the long-term retention of priming effects for a longer period than Tulving does. In order to clarify the cause of the phenomenon, we ran several experiments, one of which was designed with different graphic symbols as primers on the priming effects. The results indicated that with different symbols between study and tests, the priming effect decreased in five minutes after the study, compared to identical symbols between study and tests. This implied that perceptual information was one of the important factors in the retention of priming effects.

How does information processing after the perception phase affect the retention of priming effects? We investigated priming effects in word-fragment completion in terms of levels of processing. We found that different levels of processing in the orienting tasks did not affect the retention of priming effects. Moreover, for the purposes of identifying the role of semantic processing in priming effects, we compared direct priming with indirect priming in which primers had strong associations with primed words. The results showed that although direct priming effects were exhibited in both the immediate test condition and the delayed test condition, indirect priming effects were only exhibited in the immediate condition, not in the delayed condition. Retention of indirect priming effects has not been demonstrated in other papers (e.g., Dannenbaum & Briand 1980). We did not observe it either.

The results of these last two experiments suggest that an unknown cognitive system other than semantic and episodic memory brought about the long-term retention of priming effects. The system does not involve the framework of levels of processing, the spreading activation theory, the logogen model, and so on.

In my opinion, a part of the system concerns perceptual information processing without awareness, or, in other words, an unconscious perceptual scheme. It is different from sensory memory such as iconic and echoic memory. We have a great deal of perceptual information in adapting ourselves to ordinary circumstances. We usually see, hear, and behave unconsciously in everyday life. For example, you unconsciously see books, desks, pictures, and so on in your office every day. You can drive a car while having a talk with others. When you notice that something has changed in these situations, you pay attention to them. When you notice a crooked picture, you look at it and other things in your office. It seems to me that the presentation of primers in the laboratory is essentially the same as changing a routine.

In the case of verbal tasks, the long-term retention of priming effects can be partially supported by lexical memory which is different from semantic memory. Lexical memory, which Tulving discusses in chapter 4, can be conceived as a complex skill or memory for procedures.

In discussing the long-term retention of priming effects more generally through a variety of tasks, we can say by way of summary that perceptual information-processing upon presentation of primers automatically generates peculiar traces that unconsciously affect subsequent tasks. These traces are presumably different from ordinary encoded traces in terms of the function. They might be similar to free radicals, or they might not. We must identify them. We hypothesize that they have some relation with unconscious underlying factors such as culture, motivation, and so on. We are currently conducting further experiments.

Comparative analysis of episodic memory

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> Remembering past events is a universally familiar experience. It is also a uniquely human one. As far as we know, members of no other species possess quite the same ability to experience again now, in a different situation and perhaps in a different form, happenings from the past, and know that the experience refers to an event that occurred in another time and in another place. Other members of the animal kingdom can learn, benefit from experience, acquire the ability to adjust and adapt, to solve problems and make decisions, but they cannot travel back into the past in their own minds. (Tulving 1983b, p. 1)

Introduction. In the opening paragraph of *Elements*, Tulving argues that animals do not have episodic memory. If this argument is true, it has profound implications for every comparative analysis. Of most importance for this particular commentary is the implication that animal models cannot be used to study either the brain mechanisms involved in normal memory, or the pathological changes that produce amnesia. Fortunately, for the sake of comparative behavioral neuroscience, Tulving is wrong. Three lines of evidence support this conclusion. First, laboratory tasks solved by animals do require them to "travel back in time." Second, animals foraging in their natural habitats face similar problems, and solve them successfully. Third, the characteristics of both the laboratory tasks and the foraging situations meet the criteria for episodic memory as laid out in the table on page 35 of Tulving's book. Fourth, amnesic syndromes in animals are very similar to those in humans, with a disproportionate impairment in tasks that require episodic memory as compared to those that require only semantic memory. I'll address each of these issues in turn.

Laboratory tasks requiring episodic memory. Tulving very elegantly distinguishes between processes that involve the accumulation of experience and those that require the individual to travel back in time. Thus, for a task to be relevant to the issue of episodic memory, it must require an animal to travel back in time in order to perform correctly. The class of tasks known as a "delayed condition discrimination" falls into this category.

In a delayed conditional discrimination, the individual is first presented with a sample stimulus. The sample is then removed, and a delay interval occurs. Following the delay, the individual is given a choice between two or more responses. The response that is correct is conditional upon the stimulus presented as the sample at the beginning of the trial. Thus, the only way the individual can determine which of the responses is correct at the end of the trial is to travel back in time to the beginning of the trial and remember which stimulus was presented then.

A very familiar example of a delayed conditional discrimination is a delayed match-to-sample. At the beginning of the trial, a sample stimulus is presented. It is removed for the delay. Then the sample stimulus is presented along with other stimuli. Choosing the stimulus that was presented as the sample at the beginning of the trial is correct, choosing any other stimulus is incorrect.

More complicated delayed-condition discriminations are also possible. The stimuli presented at the end of the trial need not include the sample stimulus. For example, the sample stimuli might be either red or green. The comparison stimuli at the end of the delay might be a square and a triangle. The conditional discrimination in this case might be the following: If the sample stimulus was red, choose the square; if the sample stimulus was green, choose the triangle.

In short, a single trial of a delayed conditional discrimination is an excellent example of an episode. Careful controls are necessary to show that animals really do use memory to perform correctly. These controls eliminate a number of alternative strategies. For example, animals do not have to use mediating response strategies, such as orienting toward the correct alternative during the delay. Likewise, animals do not have to use rehearsal; requiring them to perform an interfering task during the delay does not eliminate correct performance. In short, the experimental procedures demonstrate that the information about the correct response resides in the animal's head for relatively long periods of time (over 8 hours) and does not require rehearsal. Thus, the animal must travel back in time to the beginning of the trial or episode to determine which stimulus occurred (Roitblat 1982).

Foraging. The delayed conditional discrimination described above in the laboratory setting is not just a convenient laboratory procedure. Predators searching for prey are often faced with these types of discriminations. For example, consider birds and insects that feed on nectar from flowers. In this case, the discrimination can be best described as a delayed non-match-to-sample. The animal goes to a flower at the beginning of foraging and obtains some nectar there. After leaving that flower, the animal must decide where to go next. Because the flower takes a considerable amount of time to replenish its nectar, the optimal strategy is to go to some other flower during that interval. If the original visit to the flower is thought of as the sample, then in the subsequent choice, the correct response is to choose a flower that does not match the sample.

Animals solve these types of foraging problems very well. As in the laboratory tasks, controlled experiments demonstrate that the animals do not use response strategies or stimulus marking strategies to perform the task. Rather, the information about the correct response lies in the animal's head.

Many other examples of delayed conditional discriminations can be found in natural habitats. Analyses of foraging patterns show that predators using relatively efficient strategies have a selective advantage over those using relatively inefficient strategies. Thus, the processes of natural selection should have put substantial pressure on animals to develop an effective episodic memory (Kamil & Sargent 1981).

Diagnostic features of episodic memory. In the table on page 35 of *Elements*, Tulving clearly and elegantly lays out the differences between episodic and semantic memory. Performance in the delayed conditional discriminations described above meets many of the criteria for episodic memory as outlined in this table, and violates none of them. Some of the criteria are easily applied to delayed conditional discriminations, and these I will discuss. Other criteria cannot be applied so readily because the relevant data simply are not available.

The source of information is sensation. The sample stimulus to be remembered is presented to the animal via standard sensory modalities. Visual, auditory, and somatosensory stimuli have all been used for the sample stimulus.

The units of information are events or episodes. Indeed, the unit of analysis in a delayed conditional discrimination is a single trial or an episode. Because the stimulus that is the sample varies from trial to trial, the animal must remember the sample stimulus in the context of a given episode in order to perform correctly.

The organization of the information is temporal. Animals have a very well developed ability to measure the passage of time (Church 1978). Experiments varying the intertrial interval and the delay interval show that choice accuracy is highly dependent upon the temporal aspects of the task.

The temporal coding is present and direct. Indeed, if the stimulus presented as the sample is already familiar to the animal, successful choice accuracy cannot be attained without temporal coding of the most recent experience with that stimulus.

The retrieval query must be based in terms of time. Tulving gives as an example, "What objects did you see on the table?" (p. 40). "What stimulus did you see at the beginning of the trial?" is the query that is addressed to the animal in a delayed conditional discrimination.

Laboratory tasks emphasize particular episodes. As Tulving himself remarks: "By this rule, conventional . . . recognition tasks, in which the rememberer must . . . identify as 'old', a copy of an item encountered on an earlier occasion in a particular situation, are classified as episodic" (p. 55). A delayed conditional discrimination task is clearly an episodic task by this definition.

Amnesic syndromes produced by brain damage in rats (Meck, Church & Olton, in press; Olton, in press) and monkeys (Mishkin, Spiegler, Saunders, & Malamut 1982) clearly involve episodic memory. In various animal models of amnesic syndromes, the brain damage produces severe impairments in episodic memory but little impairment in semantic memory.

Comparative behavioral neuroscience. Brain damage in humans can produce amnesic syndromes. These amnesic syndromes are never global, but involve some aspects of memory to a greater extent than others. The distinction between episodic and semantic memory summarizes many of the dissociations that are seen in amnesia. As outlined by Tulving and by other reviewers (Meudell & Mayes 1982; Rozin 1976; Schacter & Tulving 1982; Squire 1982), choice accuracy in tasks requiring episodic memory is impaired to a much greater extent than choice accuracy in tasks that require only semantic memory. Damage to temporal lobe structures in humans produces an amnesic syndrome with severe impairments in delayed conditional discriminations, but relatively minor impairments in tasks that do not involve episodic recall (Sidman, Stoddard & Mohr 1968).

The similarity of the amnesic syndrome in humans and animals, and the fact that similar pathology produces the syndrome, provide two forms of very strong evidence that animals do have episodic memory. First, the memory processes dissociate in a similar manner with episodic impaired and semantic spared. Second, this dissociation is seen after similar types of brain damage, suggesting a homologous functional organization of the brain.

Summary. All four lines of evidence described above suggest that animals have an episodic memory. Certainly, many of the examples that Tulving gives in his book require performance that animals cannot easily provide, namely verbal recall. However, people remember in many different nonverbal ways, and if the idea of episodic memory is to be useful, it must apply to more than just verbal tasks. When this extension of episodic memory is made to nonverbal tasks, then the implication is clearly that animals have episodic memory.

Such a conclusion is absolutely critical if the comparative aspects of behavioral neuroscience are to proceed unhindered. Although people provide a wealth of detail about cognitive function, therapeutic considerations markedly limit the information that can be obtained about basic neural processes. Precise manipulations and accurate measurements of the brain are critical if we are to understand the brain mechanisms involved in normal memory and the pathological bases of amnesia. Only through work with animals can this information be obtained. If animals do not have episodic memory, and if episodic memory is involved in amnesic syndromes, then animal models of human amnesias are not possible and an alternative approach to these issues must be developed. Fortunately, for reasons outlined above, I think that animals do have episodic memory, and, as indicated by past experimentation, these models can make valuable contributions.

On falsifying the synergistic ecphory model

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Problems of testability and falsifiability are evident in the discussion of the encoding specificity principle and the synergistic ecphory model. Although I certainly do not want to argue that the encoding specificity principle is in some sense wrong [Shiffrin and I (Raaijmakers & Shiffrin 1980; 1981) have used similar ideas in our model for memory retrieval], it should in my opinion be considered as a theoretical principle that may be useful as a rule of thumb, but one that is not itself a real explanation or theory. The reason for this is, of course, that it is not falsifiable. As far as I can tell, there is no conceivable experiment that could necessitate rejection of this principle (fortunately, I might add, since I would not know how a content-addressable memory could function in any other way). This principle does not by itself explain anything; it only gives a direction in which to look for a proper explanation. If this is true, then of course the claim that "no experiments have yet been done whose results are inconsistent with the principle" (*Elements*, p. 266) becomes vacuous. By the way, I must say that I do not think it wise to try to make a distinction between this principle and the principle that retrieval depends on a reinstatement of the original encoding conditions. This reinstatement principle surely does not have to be interpreted as applying only to physical similarity, but includes similarity in mental set, the activities carried out during encoding, and so forth.

Consider next the synergistic ecphory model. This model is proposed as a general framework for recall and recognition. Note, however, that there are a large number of phenomena that cannot easily be incorporated into this model (e.g., list-length effects, interference phenomena, etc.). Perhaps it is not Tulving's objective to present a truly general theory. What is a major purpose of the model is to provide an explanation for the relation between recognition and recall. How does it fare in this respect? The model does not seem to generate a priori predictions (judging from the way Tulving uses it – but see below). Encoding/retrieval interactions are "explained" by assuming that the retrieval and trace information in the various conditions is such that an interaction results. However, in each case the model (used in this way) would also explain a result of no interaction or an interaction in the opposite direction.

However, I believe that Tulving's analysis of the synergistic ecphory model is incorrect. Let us take a closer look at encoding/retrieval interactions. If we take Figure 6 of the Précis seriously, then it must be the case that if in recognition encoding condition *a* leads to a higher recognition rate than condition *b*, condition *a* results in more trace information than *b*. Now, unless recall probability is 0 or 1, it must be predicted that *a* also leads to a higher recall probability than *b*. This prediction is based on the assumption that probability of success is a monotonic function of how far we are above the threshold. Note that this assumption must be true under almost any reasonable model that handles probabilistic measures. It will be evident that there are a number of situations where this particular prediction does not hold (e.g., maintenance rehearsal has no effect on recall, but a significant effect on recognition). Hence, perhaps the model is falsifiable after all. If it is, however, then it must unfortunately be concluded that it has in fact already been falsified. Tulving may, of course, object to my analysis by arguing that this is not the way the diagram should be interpreted. In that case, however, I wonder how much value should be attached to such a versatile and intrinsically nonfalsifiable theory.

A more specific model, compatible with the synergistic ecphory model, is the Flexser and Tulving (1978) model for recognition failure. According to Tulving, this model accounts for the constant in the Tulving and Wiseman (1975) function. This should not be accepted at face value, however. Flexser and Tulving account for the constant by keeping the parameters of the model within an experimental condition constant. If, however, the probability of encoding a feature were to be varied between subject–item combinations, more dependence would be predicted, and hence the constant would no longer be explained. The issue here is that one must conclude either that the model does not really predict the constant or that the assumption of within-experimental constancy of parameters is a fundamental and intrinsic aspect of the model (which should be testable). In opting for the latter alternative we may attach too much value to what seem to be arbitrary choices. It should also be noted that this model (which was specifically designed for this phenomenon and does not seem to explain any other results) does not really provide an explanation for the most intriguing aspect of the phenomenon, the one that made it interesting in the first place, that is, the large amount of independence between recognition and recall. It gives no rationale for predicting when retrieval cues will or will not be independent.

Does current evidence from dissociation experiments favor the episodic/semantic distinction?

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In Part I of *Elements*, Tulving presents the case for two separate types of memory, episodic and semantic, that are differentially engaged depending on the memory query directed at the cognitive system. Queries that require retrieval of the time and place in which the information was learned are said to involve episodic memory, whereas those that can be answered without recourse to retrieving specific events are said to depend on semantic memory.

What is the best evidence to be put forward for such a proposition? If we ignore the speculative remarks in chapter 3 and direct our attention to the empirical research, the logic of which is considered in chapter 4 and the results of which are presented in chapter 5, we see that the most convincing evidence involves experimental dissociations.

> Experiments following the logic of experimental dissociation involve the manipulation of a single variable and comparison of the effects of the manipulation in two different tasks, one episodic, the other semantic. Dissociation is said to have occurred if it is found that the manipulated variable affects subjects' performance in one of the two tasks, but not in the other, or affects the performance in different directions in the two tasks. (p. 73)

The finding of dissociation is taken as support for the two systems, whereas the finding of similar effects of independent variables in the two tasks fails to support the distinction.

Several findings, reviewed on pages 84–91, do show experimental dissociations between tasks reasonably classified as episodic and semantic by Tulving's criteria. To date almost all of these take the following form: An independent variable is shown to have some effect on an episodic-memory task, such as recognition, but no effect on some semantic-memory task, such as perceptual identification (e.g., Jacoby & Dallas 1981). Evidence showing that some variable could affect a semantic-memory task and leave an episodic-memory task unaffected, or even have opposing effects on the two tasks, would be a convincing complement to the dissociations already reported. (Jacoby, 1983b, does in fact report a case in which manipulation of a variable has opposing effects on the two types of task.)

Neely and Payne (1983) have criticized research comparing performance in episodic- and semantic-memory tasks as typically involving numerous confounded variables besides the critical one of interest, namely, the nature of the retrieval query directed at the system. Their criticism is well founded, but for purposes of this commentary I will assume that the data from the functional dissociation experiments can be taken at face value and will direct my remarks at the logic of the enterprise.

Several difficulties exist with the logic of functional dissociation that vitiate its plausibility as a rationale for separating memory systems. In fact, Tulving has himself previously argued against this logic in attacking other proposals for separate memory stores or processing modes (Tulving 1976; 1979b; Tulving & Bower 1974). For example, Fisher and Craik (1977) reported an experiment in which they found a strong interaction (dissociation) between the type of processing subjects engaged in when studying word pairs and the processing task required at retrieval of the target member of the pair. The dissociation took the form of a greater advantage in recall when semantic processing occurred on the occasions of both study and test than when phonemic processing occurred at both times. Since there was also a main effect of type of processing at the study stage, with semantic processing producing better performance than phonemic, Fisher and Craik (1977) concluded that both the notions of level of processing during study and congruity of processing between study and test (embodied in the encoding specificity hypothesis) were needed to explain the results. Tulving (1979b) criticized the conclusion that the data revealed evidence for different levels of processing, convincingly in my opinion, on the grounds that one could equally well describe the main effect in the data in terms of an effect of processing "level" of the cue at the test stage rather than the processing of the study episode (see Tulving 1979b, pp. 417–22 for the details of this reasoning). Tulving (1979b, p. 421) suggested that "Fisher and Craik's findings are logically consistent" with the notion that "probability of recall is always determined only by the compatibility between the trace information and the retrieval information. If one accepts this conclusion, any insistence on the importance of encoding or retrieval conditions outside the relation between the two makes little sense." Thus, despite the dissociation revealed by Fisher and Craik (1977), Tulving argued against their evidence as indicating separate processing levels. But if experimental dissociations can be accounted for in this way in the Fisher and Craik study, then why not in the other cases that Tulving uses as evidence for the episodic/semantic distinction? I will return to this point shortly.

A second difficulty with the logic of experimental dissociation as it has been applied in all studies to date is that only a single episodic and semantic task have been employed. At the least, the logic would seem to demand that experimenters should use two tasks allegedly relying on each system to ensure that an independent variable has different effects on tasks supposed to engage different systems, but similar effects within the same system. A natural question is how to interpret the finding of dissociations *within* the semantic- and episodic-memory systems. Would such findings implicate subsystems? Suppose, for example, that an investigator were to provide four groups of subjects with high-frequency and low-frequency words mixed within a list and then test them later with either episodic-memory tasks (recall and recognition) or semantic-memory tasks (completing word fragments and answering general-knowledge questions). Although no one to date has reported such an experiment (one is currently being conducted by T. A. Blaxton at Purdue University), we can predict on the basis of past results that the two episodic tasks will not show one common pattern of results with the two semantic-memory tasks showing a different pattern. The reason is that even in the comparison of the episodic-memory tasks a strong interaction will be evident: Recall of high-frequency words will be better than that for low-frequency words, but recognition of low-frequency words will be superior to that of high-frequency words (e.g., Balota & Neely 1980; Gregg 1976). Are we to interpret this finding as indicating different subsystems within episodic memory, perhaps a recall system and a separate recognition system?

Tulving (1976) considered such approaches to interactions between recognition and recall as reflecting basically different processes in the two tasks and rejected them in favor of interpretations based on an "episodic ecphory" view, a predecessor of GAPS (General Abstract Processing System) in the current volume. The general approach is to argue that interactions between recognition and recall can be explained in terms of information from two sources, that in the trace and that in the retrieval environment, as in the explanation of Fisher and Craik's (1977) results. The cues in recognition and recall are said to overlap differently with information in the memory traces, thus producing differing patterns of performance. Once again, strong dissociations are explained in some way besides postulating separate systems. Finding interactions within two tasks that are both supposed to engage the semantic-memory system would complicate matters further, but at the moment I know of no such data, because researchers have typically not included comparison of semantic-memory tasks in their experiments.

The functional dissociation logic has also been used to support the notion that separate short-term and long-term memory stores exist in human memory (e.g., Glanzer 1972), but once again Tulving has sharply criticized this logic. Tulving and Bower (1974, pp. 282–84) criticize the "two component analysis," which involves dissociations induced by independent variables in the serial position curve in single-trial free recall. For example, many variables affect the prerecency part of the serial position curve but leave the recency part unchanged, thus implicating (according to some) separate memory stores. Tulving and Bower (1974, p. 283) argued against the assumption that dissociations necessarily reflect differences in the way information is stored: "The data are equally consistent with the possibility that retrieval information is differentially effective for the two components of recall, whereas the traces are indistinguishable."

Another problem with the functional dissociation logic, one alluded to previously, is how to interpret dissociations that occur within the two systems, or (more generally) how to account for other embarrassing interactions. What has happened in other domains is that memory systems have proliferated in order to explain new results. For example, in reviewing evidence from variations in materials that is taken to indicate different memory systems, Tulving and Bower (1974, p. 273) remarked that "The question is whether we should postulate a distinct memory system for every discriminable stimulus variable and for every variation of events along values of that variable that produces differences in memory for those events. If we did, we would soon have more memory systems or stores than we could name." However, the same trend seems to occur in Tulving's own work using the dissociation logic. When Tulving, Schacter, and Stark (1982) reported a puzzling pattern of results that did not fit well with the episodic/ semantic distinction, they suggested that the results might "reflect the operation of some other, as yet little understood, memory system" (p. 341). To quote Tulving and Bower (1974, p. 273) again, "it has not yet been made clear by anyone how the task of explaining memory phenomena is materially aided by the hypothesized existence of different memory stores and systems," a remark which still rings true.

Here I have taken Tulving's frequent arguments against functional dissociation as a logic for separating memory stores, levels, or systems and turned them to examine the episodic/semantic distinction. Unfortunately, the logic here does not seem any more forceful than it has in other cases. In fact, there is probably a much stronger case to be made for separate short- and long-term stores, although Tulving and Patterson (1968, p. 247) argued that "In the long run, nothing much can be gained by postulating a homunculus searching through one or more types of memory store for desired mnemonic information." Perhaps the case is different for memory systems, and perhaps people really do have separate episodic and semantic systems as Tulving proposes, but certainly there is no compelling evidence for the case now.

In fact, the remainder of Tulving's book suggests a more parsimonious way of interpreting all these dissociations taken as evidence for the episodic/semantic distinction. The same general arguments for interpreting interactions between study and test conditions in "episodic"-memory experiments can apply. Information can be coded in many different ways, and we might consider the memory trace (with Flexser & Tulving 1978, and others) as a bundle of features encoded about an experience. Various types of test may cause people to encode features from retrieval cues that overlap to a greater or lesser extent with those in the memory trace for the event, thus effecting different patterns of remembering. Interactions between study and test situations are thus normal occurrences, rather than exceptions to be explained by postulating various memory systems. Such features as time and place of occurrence of a memory should be given no special status, but will be emphasized in some testing situations and not in others. Thus, interactions from experiments using very different testing procedures can be described in terms of encoding specificity (Tulving 1983b, chaps. 10 and 11) or transfer appropriate processing (where stored information is said to transfer to greater or lesser extents depending on the nature of the test; Morris, Bransford & Franks 1977). In accounting for results from his experiments Jacoby (1983a; 1983b) has taken the general tack that Tulving uses as evidence for the episodic/semantic distinction. Kolers and Roediger (in press) argue somewhat similarly, except that they suppose that performance is determined by a matching of mental operations or procedures at the occasions of study and test rather than a matching of informational contents from traces and cues.

In sum, I believe that Tulving (1976; 1979b; Tulving & Bower 1974) was on target in criticizing the logic of functional dissociation as typically used to establish separate memory stores or systems. Unfortunately, the criticisms apply with equal force to his own methods to separate episodic and semantic memory. Happily, however, his own ideas detailed in the remainder of the book provide a general framework for interpreting the results of the dissociation experiments reviewed in chapter 5. As a side benefit of accepting this other framework for discussing the results, other problems dissolve. For example, if we abandon the episodic/semantic distinction, the whole problem of "free radicals" falls away.

The ontogeny of episodic and semantic memory

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Several years ago, while writing an undergraduate text on human memory, I made use of Tulving's distinction between episodic and semantic memory. Episodic memory, I repeated, represented our memory for events, while semantic memory represented our knowledge of the world. Facts such as *two plus two equals four* and *most fire engines are red* are examples of information from semantic memory (Seamon 1980). At that time the distinction between these memories seemed clear and it remains so today. But I was concerned then and I remain concerned now over the lack of systematic information on the ontogeny of both types of memory. Put another way, one effective strategy in arguing for the functional separation of these memories would be to show how they become functionally separated. With this in mind, I opened Tulving's book to learn about the source and development of both types of memory.

Table 1 of the Précis (Tulving 1983b, p. 35) provides a summary of factors that are believed capable of differentiating episodic and semantic memory. In terms of the origin of information, Tulving states that *sensation* is the source of episodic memory, while *comprehension* is responsible for semantic memory. To store information in semantic memory, it is necessary to relate it to existing knowledge (Tulving 1983b, p. 37). Operationally, episodic memory is seen as direct and experiential, while semantic memory is viewed as symbolically coded knowledge that can be acquired secondhand (Tulving 1983b, p. 41).

Curiously, at the bottom of the table of differences is a reference to *bicameral men*. Based on Jaynes's hypothesis that ancient people lacked consciousness and lived without an awareness of past events (Jaynes 1976, p. 371), Tulving sees this idea as supportive of the episodic/semantic memory distinction. Accepting Jaynes's hypothesis, Tulving equates the conscious awareness of past events with episodic memory and argues that since ancient people had knowledge but could not reminisce, it must be that they possessed only semantic memory; episodic memory had not yet evolved. Phylogenetically and ontogenetically, states Tulving, semantic memory came first (Tulving 1983b, p. 57). Even though presented in an informal and speculative portion of the text, this is a bold assertion to make. Ontogeny may recapitulate phylogeny, but here both seem turned on their head.

Since the human brain is largely unchanged over the last 50,000 years and primitive people, whether conscious or not, would need to learn from their experiences in order to survive, the long-term presence of episodic memory is difficult to deny. It is more reasonable to assume that because human progress is dependent upon the accumulation of knowledge that is held in semantic memory, such progress has been relatively recent because such knowledge has only been recently acquired. Ontogeny probably does recapitulate phylogeny, but episodic memory must have come first.

Developmentally, we are just beginning to learn about memory for episodic experiences and the acquisition of knowledge (Kail 1979). On these topics my thoughts are similar to those of Furlong (1951) that Tulving cites in his text. Distinguishing between retrospective (episodic) and nonretrospective (semantic) memory, Furlong held that retrospective memory gradually becomes the nonretrospective type as its reference to temporal and spatial context is *subject* to gradual decay (Tulving 1983b, p. 17). I, too, believe that semantic memory is derived from experience; however instead of viewing semantic memory as the result of fading episodic contexts, I believe that semantic representations can be associated with so many experiential contexts (e.g., all the times and places that you experienced *two plus two equals four*) that they become functionally context-free. As a child matures and accumulates a body of knowledge, the direct reliance of semantic memory on episodic experience would appear to diminish. Thoughts can generate knowledge, provided that a structure for knowledge exists. To this extent I agree with Tulving when he argues for the differential source of episodic and semantic memory in terms of sensation and comprehension. He is dealing with adults and their memory structures are already largely developed. But rather than trying to differentiate episodic and semantic memory in terms of the presence or absence of contexts (which Tulving is reluctant to do) or particular sources of information at a particular point in time, greater understanding may be obtained by examining the ontogeny of both types of memory within the framework of developmental changes. Since our progress and well-being are tied to the development of semantic memory, we need to know more. This book is an impressive step in that direction.

Recognition and recall: The direct comparison experiment

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Tulving's book represents an important contribution to our understanding of comparison of recognition and recall. He has postulated the Synergistic Ecphory Model of Retrieval as a new framework which accounts for some of the findings of recognition and recall. In his earlier paper (Tulving 1976), recognition and recall are assumed to differ only with respect to the nature of retrieval information. However, the earlier hypothesis has been revised, based on the direct comparison experiment of recognition and recall. The Synergistic Ecphory Model of Retrieval is a framework which explains the findings of the direct comparison experiment. However, I will comment on two points: (1) the possibility of an alternative interpretation of the findings of the direct comparison experiment; and (2) the relation between the Synergistic Ecphory Model of Retrieval and the classical strength theory of memory.

(1) Effects of copy cues and of associative cues are found in the direct comparison experiment of recognition and recall. The former effects can be obtained from the fact that recognition performance is better than recall performance, and that a positive correlation between recognition and recall is shown. The latter is concerned with a negative correlation between false positive responses to associative cues in recognition and correct responses to those in recall.

To explain these results, Tulving proposes the Synergistic Ecphory Model of Retrieval, which integrates ecphoric information and conversion thresholds. It seems to me, however, that there are alternative explanations for the results of the direct comparison experiment.

For example, one of the possible explanations is that retrieval attributes in recognition and recall are different (Tajika 1980). Tajika conducted a factor analytic study to extract retrieval attributes in recognition and recall. Two factors emerged. One is a discriminative attribute which associates with performance on tests of the explicit targets', the other is an associative attribute which is involved in generating the targets implicitly. This means that recognition and recall differ not only with respect to the nature of the retrieval information, but also with respect to the retrieval attributes. The copy cues task Tulving has used in the direct comparison experiment can be associated with performance on tests of the explicit targets, regardless of recognition or recall. As a result, recognition and recall will show a similar pattern of responses in the copy cues task. Those results suggest a positive correlation between recognition and recall. Moreover, subjects process retrieval information better from copy cues in the recognition task than in the recall task, because the task may draw more on the discriminative factor. Therefore, recognition performance exceeds recall performance.

Let me turn to effects of associative cues. Tulving emphasizes that there is a negative correlation between the valence of associative cues and their false positive recognition rate. However, the recognition rate involves false positives. The correct recognition rate to associative cues is not shown in Table 14.2 of Tulving's book (1983b). Judging from the results of associative cues, the pattern of responses in the Recognition Group is similar to that in the Recall Group. If the results of the valence of associative cues and their correct recognition rate are analyzed, they will be positively correlated.

(2) Tulving insists that thresholds derived from the Synergistic Ecphory Model of Retrieval are different from those derived from the strength theory of memory with respect to retrieval information. I am impressed with the similarity of both kinds of threshold in Tulving's schematic diagram of the Synergistic Ecphory Model of Retrieval (Tulving 1983b, Fig. 14.3).

In the strength theory of memory, the threshold for recall is assumed to be higher than that for recognition. On the other hand, the name threshold is higher than that for the familiarity threshold under ecphoric information, as Tulving states. As a result, both the Synergistic Ecphory Model of Retrieval and the strength theory of memory view recognition and recall as a single-stage process. It seems to me that both kinds of threshold are similar, though Tulving points out that the thresholds he refers to are grasped regarding ecphoric information, whereas in the strength theory of memory they are grasped with respect to the strength of the memory trace.

Just how does ecphory work?

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It is now about twenty years since Tulving proposed distinguishing between accessibility and availability in memory. He has since systematically developed all the implications of this fruitful insight. The results of this theoretical work are impressive and his book, *Elements*, is certain to remain for a long time a reference not to be ignored by anyone who attempts to elucidate the mechanisms of memory. Nevertheless, although I agree with most of the author's basic tenets I cannot, for the moment at least, go along with the view that semantic and episodic memory are functionally *completely* independent one from the other. A priori, for the time being, although we cannot decide empirically, I do not understand why the syntactic–semantic system would not be rich and flexible enough to allow the encoding and the retrieval of spatial and temporal information. I cannot see any clear qualitative difference between the two following memories which "spontaneously" come to my mind: "I was born at the time of the Battle of Stalingrad" and "Victor Hugo was born a long time ago at the beginning of Napoleon's Empire." It is true that one of the two memories can be strictly biographical (episodic) and the other is factual knowledge (semantic), but in both cases the events are "temporally dated" and "spatially localizable"; in the two cases the memory indispensable to the production of language is involved, spatiotemporal relations are syntactically and semantically encoded and retrieved, and lastly, in both cases the discursive and pragmatic context is the essential determinant of the access to the memory.

A second problem touches on the access to memory information. Of course it is now hardly questionable that such access is the result of an interaction between the conditions of encoding and the conditions of retrieval. Just what is the exact nature of this interaction? Tulving discards the hypothesis that the individual, through simple association, activates a previously stored memory trace. According to him, this associative mechanism is characteristic only of semantic memory. Indeed, the associations that first attracted attention were the semantic associations (predominant, general ones) and only lately have we been concerned with episodic associations (not predominant, circumstancial, specific ones). But, for instance, the system of "horizontal" and "vertical" associations of Wickelgren (1979) conveys information of an episodic as well as a semantic nature. Moreover, we have been able to show that an association, normatively or semantically defined, between the context of recognition and the encoding context is an important determinant of recognition (Peris 1983; Peris & Tiberghien 1984; Tiberghien 1981). According to Tulving a memory does not exist prior to its retrieval but results from the "combination" between the retrieval cues and the momentarily available mnesic information. The hypothesis is of course a tempting one for it enables us to account for the extreme diversity of our subjective experiences of recollection with a remarkable economy of means. But now what metaphor can we choose to describe such a process: resonance, hologram, scanning? How can we define and operationalize the predictions derived from such a hypothesis? Personally I do not think that the associationist or neo-associationist solutions have been sufficiently thoroughly investigated to be definitively discarded (Donaldson 1981; Hunt & Einstein 1981; Jones 1982; Mandler 1980; Murdock 1982; Ratcliff 1978).

If we want to take the explanation further, several conditions have to be satisfied. The first point is to specify what is implied by the concept of context of encoding or retrieval. Modalities of contextualization can be very different, and it is not certain that identical psychological mechanisms are involved in effects of context linked to the psychophysiological state of the individual ("state dependent learning"), in effects of context linked to general environment, in effects of "list context," or in effects of specific context. Likewise, there are undoubtedly different degrees of integration of the context and focal information ranging from simple context juxtaposed to the target to context that, together with the target, constitute a highly integrated mnesic representation (Baddeley 1982b; Godden & Baddeley 1980). It is not certain that the dynamics of these different effects of context are entirely reducible. Our personal preference for the moment is a mechanism of access to memory led by semantic associations ("horizontal" or "vertical," "intrinsic" or "extrinsic," "interactive" or "independent") between the context of retrieval and the context of encoding. Perhaps we should postulate a double mechanism of retrieval of memory information: a very rapid, not very conscious, almost automatic process of combination between contextual retrieval cues and memory trace; a much slower, conscious, and intentional process of associative search or reconstruction of memory representation (Mandler & Boeck 1974; Peris & Tiberghien 1984).

The second point is to improve noticeably our understanding of the concept of familiarity and more precisely to ask ourselves whether there might not be two different origins to the feeling of familiarity. Does perceptual information repeatedly encoded in the same context give rise to a feeling of familiarity equivalent to the one resulting from the encoding of perceptual information in multiple contexts (Lamon 1982)?

The final point is to find out whether or not the psychological mechanisms of identification of new information and of old information are strictly identical. If not, we would be faced with an important theoretical problem for, in fact, the peculiarity of new perceptual information is that it cannot be characterized by its former context of encoding. Besides, Tulving is perfectly aware of the problem since he raises a question about the mechanism capable of determining the acceptance or rejection of mentally evoked information. The puzzle is far from being solved since, for example, some researchers note, in human-face memory, effects of context of the same magnitude on correct recognitions and on false recognitions (Davies & Milne 1983a,b; Donaldson 1981; Thomson, Robertson & Vogt 1982, Exps. 2–7; Winograd & Rivers-Bulkeley 1977, Exp. 1), others note an effect of context only on correct recognitions (Bruce 1982, Exp. 2; Brutsche, Cisse, Deleglise, Finet, Sonnet & Tiberghien 1981; Thomson & al. 1982, Exp. 1; Winograd & Rivers-Bulkeley 1977, Exp. 2); and finally Bower & Karlin (1974) do not observe any effect of context, either on correct recognitions or on false recognitions. This lack of coherence is puzzling and one can rightfully wonder whether the psychological processes leading an individual to accept old information can be unreservedly assimilated to those that lead him to accept new information as being old.

Finally, another problem arises from the confrontation between Tulving's synergistic model and the theory of signal detection applied to memory. Taking up again a very old theory, a practice which is often fruitful, Tulving suggests that there are two different thresholds determining the conversion of the response: a threshold of denomination and a threshold of familiarity (McDougall 1904). If I have not misunderstood Tulving's line of reasoning, contextual variations should not modify these two thresholds but only the ecphoric information resulting from the combination between contextual cues and mnesic traces. Now this is far from being an absolute law since in 86 experiments we have examined (Lecocq & Tiberghien 1981) context affects the index of discriminability in 91% of the cases but equally affects the criterion of decision in 74% of them. How can Tulving's theory explain this sensitivity of the criterion of decision to the effects of context?

Despite the importance of theoretical questions which remain to be answered, Tulving's work is a necessary and long-awaited incentive for all the researchers interested in the study of human memory. Moreover, we hope that the necessarily technical nature of the theoretical debates will not prevent the reader from appreciating the personal remarks developed by Tulving in the expansion of his basic text. The context in which a theory originated and was developed is often as instructive as the theory itself (the context again!).

Memory: Two systems or one system with many subsystems?

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There are many things in Tulving's *Elements* with which I agree, and some with which I tend to disagree. One of the points of disagreement concerns the interpretation of the episodic/semantic memory distinction. The distinction is undoubtedly an important one. Its heuristic value for distinguishing between tasks that involve differences in information, in operations, and in applications is unchallenged. The problem, however, is whether it is profitable to postulate two functionally different memory systems which can operate independently. I will argue that instead of a distinction between two memory systems a unitary memory system consisting of many interrelated subsystems may be preferable.

According to the currently prevailing view of information processing, the input resulting from a sensory stimulation undergoes a rapid automatic analysis at different stages or levels of abstraction (e.g., Craik & Lockhart 1972; Shiffrin 1976). Although much may be said about the order of activation of codes in these stages (e.g., Nelson 1979; Treisman 1979; Van der Heijden 1981) this need not concern us here. As a result of the analysis, a large number of memory codes, each connected in a systematic way to codes in previous or subsequent stages, is activated shortly after the onset of a sensory stimulation. Of these codes only those at the first encoding stage have a direct correspondence with the specific physical characteristics of the input. At all following stages codes are abstractions that represent the organized knowledge about the world.

It is this organized knowledge that makes up semantic memory. As Tulving (1983b, p. 69) notes, the common interpretation of the concept "semantic" as referring to word meanings is too restrictive. Semantic memory also includes knowledge about many other characteristics of verbal and nonverbal stimuli that are not necessarily verbalizable, such as natural sounds, voices, visual forms, textures of objects, melodies, mood states, tastes of food, and so on. With this extension of the concept "semantic" in mind, each of the encoding stages may be conceived of as one or more subsystems which are involved in processing different aspects of the same sensory stimulation.

The codes in the various subsystems remain activated for a short period of time and compete for the limited capacity for controlled processing (e.g., Posner & Warren 1972; Shiffrin & Schneider 1977). A subset of these activated codes is selected for controlled processing that consists of performing any of a number of elaborative operations. This set of selected codes, and probably some of the nonselected but simultaneously active codes as well, becomes related and forms the episodic representation of the event. According to this point of view, an episodic-memory representation consists of a unique complex of semantic codes at different levels of abstraction (see Craik 1979a).

I believe that such a conceptualization of the relationship between episodic and semantic memory is preferable to a two-system dichotomy. It is more parsimonious, because it does not need a specification of two independent types of encoding, storing, and retrieving information, nor does it necessitate a specification of the nature of the interaction between the two systems.

The conceptualization of episodic-memory representations as sets of codes or attributes (e.g., Bower 1972; Martin 1972) has proven to be very useful in explaining many kinds of episodic-memory phenomena. Such representations will show many semantic characteristics. It is, therefore, not surprising to find semantically related intrusions and semantic clustering in free-recall tasks, and high false-alarm rates to distractor items that are in some way similar to target items in recognition tasks (e.g., McCloskey & Santee 1981). Moreover, episodic representations will be very vulnerable to interference because the same codes may be part of different representations.

Also from a developmental point of view, semantic and episodic memory seem to be inseparable. People learn many things through their personal interaction with the world. Thus, semantic knowledge derives from episodic experiences (e.g., Kintsch 1974). Semantic representations are created ex novo as new generalizations and differentiations corresponding to various types of invariants in the world are formed. These invariants are encountered many times in different contexts and eventually become represented as concepts in a richly interconnected network of relations to other concepts. Such representations may be used without reference to any of the particular contexts in which they were embedded originally (e.g., Wickelgren 1977). A number of authors have argued against such a view and have suggested that semantic memory develops before episodic memory. However, Tulving already notes that the apparent absence of episodic memory in children may be related to their inability to keep track of the order of events. Given that young children have not yet learned ordering concepts, nor do they possess knowledge about clock times, days of the week, or calendar dates, it should not come as a surprise that they sometimes cannot tell whether an event took place before or after another, yesterday or a week ago. Development of episodic and semantic memory seems to go hand in hand: Semantic memory develops on the basis of episodic experiences, whereas the ability to accurately report the content and temporal order of episodic experiences increases with the growth of semantic knowledge.

In some respects the episodic/semantic distinction seems to be analogous to other postulated dichotomies of the memory system, such as the STM–LTM (short-term memory, long-term memory) distinction. For some time the latter distinction has also been interpreted as referring to two different systems each with particular characteristics. However, it is now commonly believed that STM and LTM may better be conceived of as different states (active and passive) of the same system. Differential results are caused by the specific demands posed by STM and LTM tasks (e.g., Bernbach 1975; Shulman 1972).

The episodic/semantic memory distinction is here to stay. Its value, however, is not in distinguishing two different memory systems but in allowing a convenient taxonomy of memory tasks. These tasks may sometimes show different results in transfer and dissociation paradigms. Such results, however, are not to be taken as evidence for the existence of two different memory systems, but as showing that the system can be used in different ways. Episodic and semantic tasks are different methods of tapping one system, not similar methods of tapping two systems.

Author's Response

Relations among components and processes of memory

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Most of the individual commentators have focused on different aspects of *Elements*, although the main division is between those who have addressed the distinction between episodic and semantic memories and those who have chosen to comment on the General Abstract Processing System (GAPS) and related experimental research and theoretical ideas. My response is organized along these two main divisions.

Episodic and semantic memories

I expected to be stimulated, educated, amused, annoyed, and entertained by the commentaries, and I was not disappointed. I was also surprised to discover that in the course of the exercise my ideas concerning the relation between episodic and semantic memory had changed. In this sense, and at least from my personal point of view, the unique BBS "treatment" has turned out to be invaluable.

Criticisms of the episodic/semantic distinction. The hypothesis that episodic and semantic memory represent functionally distinct systems was not seen to have much merit by the commentators. **Baddeley, Hintzman, Kihlstrom, Klatzky, Lachman & Naus, Morton & Bekerian, Roediger, Seamon, Tiberghien,** and **Wolters** all found the idea unacceptable, and **Hirst,** too, expressed doubts about its viability.

The rejection of the hypothesis is based on a variety of reasons. These include: (a) Evidence for the distinction is weak (Baddeley, Hintzman); (b) memories do not fall neatly into the two categories (Hirst); (c) no theory exists as to what the systems are like and how they interact (Hintzman); (d) we should have learned the lessons of history regarding the futility or perils inherent in duality assumptions (Kihlstrom, Klatzky); (e) the view of unitary memory is preferable (Kihlstrom, Lachman & Naus, Morton & Bekerian, Roediger, Wolters); (f) the logic of dissociations is faulty (Hintzman, Roediger); (g) the idea that episodic and semantic systems are completely independent is not easy to accept (Tiberghien); (h) the distinction is not necessary (Hintzman, Lachman & Naus).

I will not attempt to deal with all these criticisms in detail: It is unlikely that much light would be generated by the polemics. Moreover, there is not a great deal that I can do with assertions and expressions of belief that memory is memory, or that facts are facts, features are features, codes are codes, that the time of the episodic/semantic distinction has come and gone, or that there is neither need nor convincing evidence for it. I can only agree that the case for the distinction is not absolute and that it is possible to interpret any findings without invoking the distinction; and I understand why the venerable idea of unitary memory appeals to so many people. But I also think that there must be room in science for ideas that are not based on irrefutable evidence, ideas that depart from what is known and accepted, ideas that look towards the future rather than summarize the present. The self-correcting nature of the scientific enterprise takes care of these ideas if they turn out to be wrong.

From the point of view of someone who believes in the distinction, an optimistic interpretation of the loud chorus of dissenting voices might be that what is deficient is not the idea itself but rather the way it has been presented, or perhaps some of its details. Remembering the struggles I had in trying to state the relation between the two systems and my own uneasiness with what I put down on paper, I re-read what I had written on the topic. What I found was disconcerting: The episodic and semantic systems are not just "functionally distinct" yet "closely interacting" (*Elements*, p. iii), but also "separate" yet not "completely separate" (p. 32). Whatever we can say about the quality of evidence regarding the distinction, the description of the relation between episodic and semantic memory is pretty fuzzy. Given such a state of affairs, how can the critics be faulted for their reaction?

We shall return to the matter of the relation between episodic and semantic memory. For the moment let us simply note that it seems to be in need of help.

Ontogeny of episodic and semantic memory. Several unitary memory theorists among the commentators – Hintzman, Lachman & Naus, and Wolters – agree among themselves that semantic memories develop out of episodic memories. Hintzman suggests that the mechanism by which this is accomplished is the one that Semon (1923) described under the label of "non-differentiating homophony." Lachman & Naus argue for their claim on the basis of plausibility of a particular kind of evolutionary development. Wolters takes his cue from Kintsch (1974). Seamon, who accepts the general idea of two types of memory, also agrees with Hintzman, Lachman & Naus, and Wolters in expressing his belief that semantic knowledge is derived from experience. Since Seamon thinks that "episodic memory must have come first," to him the suggestion, made by Schachtel (1947) and by Kinsbourne and Wood (1975) and accepted in *Elements*, that semantic memory precedes episodic memory not only in the development of an individual but also in that of the species, seems to have turned both ontogeny and phylogeny on their heads.

No direct evidence is as yet available on the crucial issue of the developmental sequence of episodic and semantic memory. Thus, only speculation can tell us whether the inability of young children to keep track of the order of events is attributable to their not yet having learned the concept of calendar time and order of events, as suggested by Wolters, or whether it reflects inadequately developed episodic memory, as suggested in *Elements*. It is not even entirely clear yet how we could determine whether young children "have" or "do not have" episodic memory. Although Morton & Bekerian think that when a child knocks a toy off the bath at bath time and refers to the event as "duck," the child has provided evidence of having remembered a personal event, the basis for such a conclusion is not clear. The same problem will emerge when we turn to Olton's claim that animals have episodic memory.

Be that as it may, the debate has highlighted another problematic feature of the nature of the distinction: If the episodic system "grows out of" the semantic system, why should the two systems end up as "separate" or "functionally distinct," and how?

Episodic memory in animals? What I say in the opening paragraph of *Elements* is contested by Olton, who proceeds to establish the case for animals with episodic memories. I think he is right, to a point.

In writing that questionable paragraph, I was mindful of the kinds of concerns that Olton has now expressed and therefore carefully inserted the innocent-looking but important qualifier "quite": "Members of no other species possess *quite* the same ability to experience again now . . . happenings from the past . . ." (*Elements*, p. 1, emphasis added). The presence or absence of episodic memory is no more an all-or-none matter between species than it is within them, and there is indeed a good deal of evidence, as Olton points out, that animals, in their behavior, can rely on information from the past. The question is whether they can do it in the same way as humans, albeit without mediation by language and language-based thought. For instance, was Aristotle wrong when he said that, "Many animals have memory and are capable of instruction, but no other animal except man can recall the past at will" (Winograd 1971, p. 259)? Can animals mentally travel back in time to recollect and reminisce the way humans do?

I am sympathetic to Olton's cause, not only because it is inherently reasonable but also because it offers comfort to mine. If it could be established that animals have episodic memory and that their episodic memory has many features in common with that of human episodic memory, and if in animals the memory processes dissociate in a manner analogous to that in humans as a result of damage to homologous brain structures, then the case for the separate existence of an episodic/memory system in humans would be considerably strengthened. In *Elements*, I discussed Olton's (Olton, Becker, & Handelman 1979; 1980; Olton & Papas 1979) distinction between working memory and reference memory as "an interesting parallel" to the episodic/semantic distinction, because the two sets of concepts do have some obvious similarities. Is it more than a parallel?

Olton refers to matching to sample or nonsample tasks that animals can successfully perform as evidence for their possession of episodic memories. But making use of information stored in the past need not in and of itself imply the kind of time travel that is entailed in remembering personal events. It is quite possible that in matching to sample or nonsample situations the representation of information stored is "causal" rather than "informational," using Dretske's (1982) terms; that is, that the memory trace of the stored event only contains instructions for *future* behavior, without any information that would permit the reconstruction of the *past*.

The important point for our present discussion that emerges from Olton's commentary echoes others already made. It concerns the question about the nature of the relation between episodic and nonepisodic memories in animals: Are they "separate," "functionally distinct," or what?

Inferences in episodic memory. The perceptive commentary on inferences and temporal coding in episodic memory by McCauley raises further difficulties for the relation between episodic and semantic memory as discussed in *Elements*. McCauley argues, or at least implies, that my account of basically noninferential processing in an episodic system that is organized only temporally impales me on the horns of a dilemma: Either episodic memory is uninterestingly narrow or further restrictions in its theoretical description are necessary. Not a happy prospect, that.

When I proposed that the episodic system is not very good at making inferences I only had in mind the difficulty (frequently the impossibility) of reconstructing the temporal order of two or more experienced events. Other kinds of inferences about events from the personal past, in the scheme, were entrusted to the semantic system. McCauley's commentary suggests that the scheme is not just lacking in plausibility but also fraught with logical difficulties. It is difficult not to agree with him. Again, it appears that the nature of the relation between the two systems needs revision.

In his comments, McCauley has also suggested an interesting experiment to illuminate the nature of temporal coding and subsequent remembering of autobiographical events. Consider a situation in which a person experiences an event A, and then, some time later, another event B that is not encoded as being in any way related to A. Still later, a third event, event C, occurs that now, in terms of semantic knowledge the person possesses, suggests that A and B are in fact meaningfully related. Question: Can experimental situations be created in which the occurrence of a subsequent event C enhances the probability of a correct temporal-order judgment regarding A and B, in comparison with the probability of correct temporal-order judgment regarding A and B in the absence of C? McCauley seems to believe this possible but, on the basis of the encoding specificity principle, I must remain sceptical.

Episodic memory within semantic memory. At this juncture it may be useful to introduce the possibility that the relation between episodic and semantic as described in *Elements* is wrong and that a modification may be called for. The new idea is this: *Episodic memory may be best conceptualized as a functionally distinct system that grows out of but remains embedded in semantic memory.* It is not a system parallel to the semantic system, standing, as it were, side-by-side with it, but rather a subsystem, a system within a system.

The precise meaning of "embeddedness" and of "system within a system" will be clarified as the idea is elaborated in the course of reevaluation of some of the issues already referred to in this response and others yet to be discussed. For the moment, let us quickly note some of the advantages of conceptualizing the relation between episodic and semantic memory as one of class inclusion rather than as one of separate categories.

(1) It does away with the need to try to answer the difficult question concerning the functioning of the episodic system independently of the semantic system (Tiberghien). The answer would now be that it cannot so function.

(2) It provides a better fit with the fact that there are organisms and species that – in the course of development, because of disease, accident, or experimentally produced changes in brain function – may possess good knowledge of the world but no knowledge of the relation between specific events and their occurrence in the organism's personal space and time.

(3) It makes it easier to imagine how episodic memory evolves from semantic memory as a "higher form" of memory (cf. Hintzman, Lachman & Naus, Wolters). It is not difficult to think of the evolutionary advantages for organisms endowed with the capability of having available for present use descriptions of the past, *in addition to* the capability of utilizing only the stored prescriptions for the present.

(4) By doing away with the sharp boundaries between episodic and other memory systems it renders less controversial the proposition of episodic memory in animals (Olton). The idea that it may exist in a rudimentary form in other species, or in the very young of our own, seems to be more compatible with the hypothesis that episodic memory is a subsystem of semantic memory than with the idea that it exists side by side with the semantic system.

(5) It helps to solve the problems raised by McCauley regarding the inferential capabilities of the episodic system. As a subsystem of the semantic system, the episodic system would have at its disposal all the resources of the semantic system, even if the converse of the proposition is not true.

(4) It "predicts" that it would be difficult if not impossible for an organism to possess episodic knowledge without the corresponding (supporting) semantic knowledge.

(5) It helps us to resolve some other difficulties that have cropped up in the commentators' critique of the distinction between episodic and semantic memory as presented in *Elements*. Some of these will be considered next.

Neuropsychological evidence. In *Elements*, following suggestions made by Kinsbourne and Wood (1975) and Rozin (1976), I speculated that amnesia caused by brain damage affects primarily the episodic system, and that amnesic patients' knowledge of the world is relatively unimpaired.

Now Baddeley, upon reviewing more evidence, has arrived at the conclusion that although the neuropsychological evidence reflects the distinction between procedural and declarative learning, it does not support that between episodic and semantic memory. The same conclusion is also presented by Zola-Morgan, Cohen, and Squire (1983).

I concede the distinction between episodic and procedural memory. It has been known for a long time that amnesic patients can learn a variety of new skills without having any recollection of having done so. This matter surely should be beyond dispute now, as it was when I wrote *Elements*. (For an excellent characterization of the learning and memory tasks in which amnesic patients' performance is relatively unimpaired, the reader is referred to Moscovitch, in press.) The open question is whether some of the preserved learning and memory capabilities of amnesic patients entail semantic memory.

In *Elements* I said yes, Baddeley and Zola-Morgan et al. say no. Who is right?

Although Baddeley is right when he claims that the neuropsychological evidence for the episodic/semantic distinction is not strong, the fact remains that, in addition to clinical evidence – some of it mentioned in *Elements* – relevant evidence is not completely lacking (e.g., Cermak & O'Connor 1983; Kinsbourne & Wood 1975; Marslen-Wilson & Teuber 1975; Rozin 1976). For instance, in 1975, some 22 years after his operation, the much-studied patient H. M. showed no signs of remembering anything about his postoperative personal life, or daily events, while identifying, albeit with the help of cues, 80% of public figures who had become famous in the 1960s (Marslen-Wilson & Teuber 1975). Would Baddeley, and Zola-Morgan et al., want to label *all* the preserved learning functions in amnesics as "procedural" simply by virtue of amnesics' ability to perform on the tasks?

Part of the problem here stems from the tendency to talk about amnesia as if all amnesic syndromes were identical. Most students of amnesia today accept the fact that they are not. The claim that in all forms of amnesia episodic *and* semantic memories are impaired while procedural memories are not seems neither justifiable by facts nor reasonable by current consensus on the nature of amnesia. As long as *some* amnesic patients can be identified who show dissociations between episodic and semantic memories, the distinction is supported by neuropsychological evidence. There is no need for all reported cases of amnesia to do so.

According to the "embeddedness" hypothesis of the relation between episodic and semantic memories, we may expect to be able to identify at least two large classes of amnesic syndromes: (a) those involving impairment in episodic memory without comparable impairment in semantic and procedural memory; and (b) those involving impairment in both episodic and semantic memory without similar impairment in procedural memory. If the "embeddedness" hypothesis is correct, no amnesic patients should ever be found in whom semantic memory is impaired but semantically related episodic memory is not (cf. Warrington 1975).

Evidence for the distinction. The logic of experimental dissociations that I used in support of the distinction is questioned by Roediger. He wonders why dissociations sometimes are and sometimes are not interpreted in terms of memory systems.

Dissociations represent a necessary but not a sufficient condition for different memory systems. On the one hand, it would be difficult to argue for the existence of different systems if all variables had similar effects on performance in different memory tasks. On the other hand, it would be silly to account for all dissociations in terms of different memory stores or memory systems. There is more evidence to encourage the hypothesis of the distinction than just the experimental dissociations, as the following list indicates:

1. Armchair speculations summarized in Table 1 of the Précis (Table 3-1 in *Elements*), which Roediger wishes to ignore but which some others, for instance Klatzky, regard as useful.

2. Experimental dissociations described in chapter 5 in *Elements*, with some additional data mentioned by Ohta. Reasonably wide generality of these dissociations is demonstrated across different experiments.

3. Stochastic independence between word-fragment completion and judgments about previous occurrences of words (recognition memory) as described by Tulving, Schacter, and Stark (1982) for *unprimed* as well as primed words.

4. Pathological dissociations. Although some pathological dissociations clearly involve episodic versus procedural memory, as discussed earlier, there are others that are at the present time more naturally interpreted in terms of episodic and semantic memory (e.g., Cermak & O'Connor 1983; Kinsbourne & Wood 1975; Rozin 1976; Schacter & Tulving 1982; Wood, Ebert & Kinsbourne 1982).

5. Functional amnesia. A case of functional amnesia has been described by Schacter, Wang, Tulving, and Freedman (1982; *Elements*, p. 96) that showed a dissociation between performance on an episodic- and a semantic-memory task.

6. Brain activity. Wood, Taylor, Penny, and Stump (1980) have shown that the patterns of regional cerebral blood flow, an index of neural activity, differ for episodic- and semantic-memory tasks.

7. Cortical evoked potentials. Sanquist, Rohrbaugh, Syndulko, and Lindsley (1980) have reported that the late positive component of the wave-form of event-related potentials in a recognition-memory task was "much different" from that obtained in a task of semantic judgments (p. 575).

8. Effects of drugs. Differential effects of psychoactive drugs on the operations of episodic- and semantic-memory systems provide critical evidence for the distinction. Some early relevant experiments have been reported by Hashtroudi, Parker, DeLisi, and Wyatt (in press), and Parker, Schoenberg, Schwartz, and Tulving (1983).

9. Factor analysis. Underwood, Boruch, and Malmi (1978) included five measures of performance on semantic tasks in a factor-analytic study of a large number of memory tasks. Intercorrelations of scores on the tests showed that "our episodic-memory tasks and the semantic-memory tasks represent two different worlds" (p. 409).

10. Brain lesions in animals. A number of experiments with animal subjects have shown that experimentally produced brain lesions have differential effects on different kinds of tests of retention and memory (e.g., Gaffan 1974; Mishkin, Malamut & Bachevalier, in press; Olton & Papas 1979).

11. The impossibility of episodic memory in computers. This is a thought experiment. Imagine that a person and a computer both claim that they saw Jacqueline Kennedy Onassis in the Museum of Modern Art last Thursday. You reverse the flow of time for both the person and the computer and travel back with them. When you reach a particular moment you see, in one case, the person in the museum eyeing another visitor, and, in the other case, a programmer feeding ones and zeros into the computer. The same computer that today need not be ashamed of its semantic-memory capabilities is a failure when it comes to episodic memory.

12. Analogy with the visual system. The visual system, too, is subdivided into two, or perhaps three, subsystems, conceptualizable as a class-inclusion hierarchy.

Each subsystem serves special functions, although there is some capacity for substitution (Leibowitz & Dichgans 1977). Such a structure of the visual system encourages the belief that other major cognitive functions are similarly served by multiple subsystems.

Each item in the list, taken individually, can undoubtedly be understood or interpreted without any reference to different memory systems. The hypothesized distinction between episodic and semantic memory represents an attempt to detect some unity in the apparent diversity of these experiments, observations, and data. Experimental dissociations, whose logic troubles Roediger and Hintzman, represent a useful part of the total picture, but only a part.

Processes of episodic memory

De gustibus non est disputandum. I mentioned some matters of (scientific) taste earlier, while discussing the episodic/semantic distinction. Here are a few more, arising out of the other two section of *Elements*.

D'Ydewalle & Peeters find it "rather revealing" that the term "search" does not appear in the subject index of *Elements*. I rationalize my lack of appreciation of the idea of search by noting that for my taste it seems to be too closely tied to the warehouse metaphor of memory (*Elements*, p. 5).

Tiberghien asks whether resonance, hologram, scanning, or something else might be chosen as a metaphor to describe the process of combining the information in the engram and that in the retrieval cue. I ask, why do we need metaphors at all when we think and talk about remembering? What is the metaphor for metabolic processes, or for the workings of the immune system, or for spatial vision?

Raaijmakers notes that the encoding specificity principle is not a real explanation or theory because it is not falsifiable, whereas the synergistic ecphory model, for which my analysis is incorrect, has already been falsified. Why the preoccupation with falsifiability? Holton and Brush (1973), practicing representatives of a science somewhat more advanced than ours, in discussing the criteria for a good theory, specified six: the ability to correlate many facts, ability to stimulate directed research, deducibility of predictions, simplicity, plausibility of assumptions, and flexibility for modifications. They did not mention falsifiability as such, and probably for a good reason. There is more to theory than falsifiability. For instance, astrology is full of, indeed thrives on, predictions that are falsifiable, and frequently falsified, yet few people are willing to afford scientific status to the pronouncements of astrologists (Kuhn 1977).

Another difference of opinion emerges from Tiberghien's query as to how the synergistic ecphory model accounts for the fact that changes in context affect decision criteria in recognition-memory experiments. It does not. Tiberghien's decision criteria refer to measures derived from signal detection theory. Is there any need for one theory to account for the output of another? Models account for the data. And while we are on signal detection theory, it may be worth noting that although it has been very useful in psychophysics, its contribution to the understanding of recall and recognition has been less spectacular. I am not aware of any important discovery or insight concerning memory that was crucially dependent on signal detection theory.

There are other matters of the sort briefly mentioned here whose airing is unlikely to get us anywhere. The best way to solve these disagreements is to agree that *de gustibus non est disputandum*.

Recoding. Recoding is the label for the fact that functional properties of an originally established engram may subsequently change: The recoded engram, when ecphorized by a cue, does not yield the same recollective experience as did the original engram, and some of the information ecphorizable with the help of the original engram may not be ecphorizable with the help of the recoded one.

In GAPS, the concept of engram is a hypothetical construct. It is defined as a product of the process of encoding and as one of the sources of information on which the process of ecphory operates. Thus, it is defined in terms of its relations to other elements that comprise the conceptual framework of remembering. It does not, in a psychological analysis, exist independently of other elements.

Hintzman, Loftus & Schooler, and Morton & Bekerian comment on the concept of recoding. Hintzman questions the need for the concept of recoding. He proposes that Semon's (1923) concept of nondifferentiating homophony may suffice to account for phenomena that in GAPS are attributed to recoding.

Loftus & Schooler provide an account of the role that the concept of recoding has played in Loftus's and her associates' work on the effect of misleading questions. Morton & Bekerian criticize this work in light of the experimental findings reported by Bekerian and Bowers (1983).

I am somewhat concerned about the tendency to reify the concept of trace by all these commentators. Trace, under whatever name, is not a thing whose properties can be changed without changing the thing. I realize that it is difficult to talk about it consistently in a way that does not endow it with ontological existence, but we should all try to keep the theoretical status of the concept in mind when trying to solve puzzles in our field. Hintzman's distinction between recoding as defined in GAPS and nondifferentiating homophony as defined by Semon, for instance, seems to make sense only if memory traces have independent existence. Since they do not, the distinction is useless.

From the point of view of GAPS, there is no necessary conflict between Loftus's findings and those of Bekerian and Bowers (1983), or between the two sets of interpretation of the data. The pattern of data from Bekerian and Bowers's experiment describes an encoding/retrieval interaction rather similar to those covered in chapter 11 of *Elements*, and it can be interpreted similarly: Effectiveness of cues depends on encoding (in Bekerian and Bowers's case, recoding) conditions, while the effects of a particular encoding (here, recoding) operation depend on the nature of retrieval cues. In Bekerian and Bowers's experiment, as in all other experiments, it is not possible to specify properties of memory traces independently of retrieval conditions. For practical purposes it may be important to identify conditions under which the accuracy of overt memory performance is optimal, but from the theoretical point of view none of the many possible combinations of trace information and retrieval information allows a "true" description of the trace.

Recognition failure. The assertion that the Flexser and Tulving (1978) model, without fixing any parameters, accounts for the single constant in the recognition-failure function (Tulving & Wiseman 1975) is questioned by both Jones and Raaijmakers. Jones's reservations are centered on the use of restricted ranges of values of randomly sampled parameters. He suggests that those constraints in some sense represent estimated parameters, and that the model is therefore not quite as parsimonious as it is claimed to be.

The matter of restricted ranges has already been aired by Flexser and Tulving (1982, p. 240, n. 2). In the model as originally described, the ranges of parameter probabilities were truncated to avoid skewness of binomial distributions with small values of N, the number of potentially encodable features. Subsequent testing of the model by Arthur Flexser, however, has shown that with reasonably large values of N, letting the values of parameters vary virtually over the total possible range does not materially alter the results of model-generated experiments. The success of the model does not depend on hidden estimated parameters.

Raaijmakers criticizes the fact that, in testing the model, the randomly selected values of parameters are held constant within a given simulated experiment. He says that if the encoding parameter were permitted to vary, more dependence between recall and recognition would be observed and the model would no longer explain the constant *c* in the Tulving and Wiseman (1975) function. The thrust of Raaijmakers's criticism founders on the fact that, within reasonable assumptions regarding parameter values, the dependence is increased by intracondition variations in the probability of feature encoding at study (*p*) while it is decreased by intracondition variations in the encoding of cues at test (*r* and *s*), producing a net effect of little change. The success of the model does not depend on the assumption of constancy of parameters within an experimental condition.

Raaijmakers's other major criticism of the Flexser–Tulving model (1978) is that it does not explain the "phenomenon [that is] interesting in the first place," namely independence between recognition and recall. The point of this criticism escapes me. The two versions of the model (special and general) state explicitly under what conditions recognition failure (independence of recall and recognition) occurs and to what extent. What the model does not do is to tell us how to measure the feature overlap between different items or their cognitive representations in the real world. In this respect, we must rely on the current state of the art, as do all other models in which the concept of stimulus similarity plays an important role.

Nilsson expresses the opinion that I dealt too lightly with data showing deviations from the recognition-failure function. This is probably a misunderstanding, since I made it quite clear in *Elements* that the reported exceptions to the function are (a) real, (b) large, and (c) as yet unexplained. Exceptions do not invalidate the data that involve no exceptions.

Synergistic ecphory. The synergistic ecphory model, also discussed in publications subsequent to the writing of *Elements* (Tulving 1982; 1983a) is a rough scheme for illustrating the relation between recall and recognition within GAPS. It does not predict anything, it does not have any parameters, and it does not pretend to be able to incorporate all known facts about recall and recognition. Alternative explanations of phenomena embraced by the model certainly are possible, as pointed out by Tajika.

The major shortcoming of the model as described in *Elements* resides in the difficulty of depicting in the two-dimensional space of a book-page a structure that is more appropriately conceptualized as existing in an N-dimensional space. The two-dimensional picture of the model in Figure 6 in the Précis (Figure 14.3 in *Elements*) tempts one to think of "quantities" of trace and retrieval information, and to think of the two coordinate axes as representing variables measured at least on an ordinal scale. Raaijmakers's falsification of the model succeeds as long as the ordinality assumption is made. It may be worthwhile to explore the properties of a similar model in which both trace and retrieval information are measured on the nominal scale, although Raaijmakers's case of an encoding variable having no effect on recognition but an effect on recall would fit into a slightly modified model (Tulving 1983a) even if the ordinality assumption is kept.

Hintzman, too, ponders on the falsifiability of the synergistic ecphory model, as the model does not seem to allow people to recall an experience without recognizing it as such. According to the model, phenomena such as the cryptomnesia that Hintzman mentions (Reed 1979), unconscious plagiarism, source amnesia (Evans & Thorn 1966), and other forms of recall without recognition are not episodic memories; conscious awareness, even if only in the form of a vague feeling, of the episodic source of the recalled information is the hallmark of episodic memory. The issue is one of definitions rather than empirical facts.

Jones correctly points out that the synergistic ecphory model lacks inference-making ability, although inferences are required to fit into the model some of the critical data that suggested it in the first place. The model shows only the relation between ecphoric information and various conversion thresholds. Ecphoric information can be used, and in the case discussed by Jones may be assumed to be used, in inferential reasoning that *follows* ecphory and *precedes* the overt response. Inference-making is a part of the conversion process in GAPS.

McCauley also wonders about the role of inference in ecphory, as well as about the relation between certain putative characteristics of episodic memory (uniqueness of events, directness of encoding, and temporal organization) and the synergistic ecphory model. He raises the question as to why the model would not apply equally well to retrieval of semantic knowledge. Two reasons, among others, may be given. First, it is assumed that ecphory (some interactive conjunction of trace information and retrieval information) is a process that characterizes retrieval in the episodic system, and that in the semantic system retrieval is mostly a matter of activating existing cognitive structures. Second, it is assumed that episodic trace information includes features that embed the semantic content of the episode in self-referential time and space. It is these features that endow ecphoric information and the recollective experience with the characteristic "warmth and intimacy" that William James talked about and that determine whether a present event is felt to be a part of the past.

Summing up. Science is a collaborative enterprise. Out of the essential tension (Kuhn 1977) between those who represent the extant paradigm and those who perceive anomalies in its fabric arises a new way of looking at things. It remains to be seen whether or not the distinction between episodic and semantic memory, and the consequences of it for the study of each, will represent a genuine break with a long past. But if it does, many people will have played a role in bringing the future into the present.

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