

Ecphoria excelsa

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Elements of Episodic Memory (Tulving 1983) is a stimulating book, as stimulating as any in the century that has passed since the Ebbinghaus monograph got the experimental study of memory underway. It tells the story of one man's struggle to come to grips with the nature of remembering and with the conditions under which it comes about. The story, which spans a quarter of a century, is unsurpassed in originality and significance. It details an honest and brave attempt to construct a truly general theoretical framework, one that distinguishes itself by being guided not only by experimental observation but also by the experimental aspect of memory. The style, too, deserves tribute, for it is both scholarly and personal, as well as careful and entertaining.

Since extended praise is tiresome, let us quickly turn to one or two apparent difficulties. At the most general level, there is less clarity than there could be about the kind of theoretical framework being proposed, and in particular, about whether or how far it conforms to the information-processing metaphor. This metaphor, with its process-structure dualism, is rich enough to accommodate any kind of data in a variety of ways, and such flexibility is not without problems. Tulving appears to avoid this difficulty with the claim that all his concepts, including those he gives structural names, are really functions. For example, he says that memory traces are dispositions, processes frozen in time. This is an odd stance. What is to be gained by labeling processes with terms usually reserved for structures or physical entities? Since not everyone will hold on to such definitional conundrums when reading other parts of the book, even assuming they read them in the first place, it might have been prudent to sacrifice the advantages of concise expression and concrete metaphor in the interest of precision.

The proposals toward a taxonomy of memory will inevitably raise questions. Tulving sees the relation between episodic and semantic memory as more intricate than he did when he first distinguished the two, as shown for example by the key role he now assigns to the semantic system in remembering the content of episodes. The first section of the book is devoted, in substantial measure, to a thorough discussion of such developments and to what others have had to say about the distinction. Less thorough, however, is the discussion of how the distinction relates to the ideas taken up in the remaining sections of the book. As would be expected from the book's title, these sections deal with episodic memory. Yet, as Tulving occasionally hints (see pp. 228 and 321), even concepts as central as encoding specificity and ecphoric information may be no less applicable to semantic memory. Obviously, to the extent that the main ideas of the book do not differentiate between episodic and semantic memory, the case for distinguishing between the two is weakened.

Another feature of Tulving's taxonomy of memory is the grouping of episodic and semantic memory into propositional (knowing that) memory, which is to be distinguished from procedural (knowing how) memory. Although highly important, this development is rather sketchy and not without anomalies at this time. For instance, it seems that deciding whether a letter string is a word or a nonword is a propositional memory task (p. 34), whereas deciding whether a wine is a claret or a burgundy is a procedural memory task (p. 28). More generally, distinguishing propositional from procedural memory is complicated by the fact that the former can be made manifest only through the operation of the latter.

With respect to the Synergistic Ecphory Model, the concept of conversion threshold – at least as it is schematized in Figure 14.3 (Figure 6 of the *Précis*, Tulving 1984) – is puzzling. The puzzle revolves around the scaling of trace and retrieval infor-

mation. To make sense of the diagram, the scales must be assumed to have at least ordinal properties: b represents more trace information than does a , y represents more retrieval information than does x . But this assumption is contrary to the very essence of the encoding specificity principle. Thus, earlier in the book (see pp. 239–41) Tulving specifically denies the propositions that "memory traces have strength" and that "retrieval cues of class X are more effective than retrieval cues of class Y." Instead he argues that "traces do not have strength independently of the conditions under which they are actualized" and that the "effectiveness of any cue with respect to a particular target event depends on the event and on the condition of its encoding and reencoding." He bases these conclusions on a large number of demonstrations that the relative effectiveness of two (or more) kinds of retrieval cues can differ for two (or more) kinds of events. Such interactions would appear to be precluded by the concept of thresholds illustrated in Figure 14.3. If, when combined with retrieval information x , trace information b yields more ecphoric information than does trace information a , then the same relation must hold when the trace information is combined with retrieval information y . To render the figure compatible with the encoding-specificity principle, it would appear necessary to reduce the scaling of trace and retrieval information from an ordinal to a nominal level, and to draw the threshold in a third dimension – represented, perhaps, by a third axis or by shading of the small intersection areas.

The theoretical framework presented in the *Elements* hardly resolves the problems of memory. To take but one example, Hermann Ebbinghaus, were he to return to pass judgment, would probably be unimpressed by the light that a book written amidst the fruits of a hundred years of research sheds upon his forgetting function. But such limitations should not be dwelled upon, for to do so would be to miss the point. Tulving does not purport to offer a finished theory, but merely a theoretical framework, a perspective from which current problems can be viewed and new questions can be asked. Moreover, no claim is made for the framework's validity in the sense of an eternal truth; the framework as presented is merely a progress report to be modified by additional thought and additional evidence. It should be evaluated, then, not by how well it lays specific issues to rest, but rather by the questions it raises and the research it directly or indirectly inspires. By this criterion, the *Elements* will be an unmitigated success.

Author's Response

No eternal truth in GAPS

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Watkins has good things to say about *Elements* (Tulving 1983; 1984), but also expresses four sorts of misgivings about some of its contents. First, he charges me with ambivalence toward process-structure dualism. I deny the charge, and have no idea why he thinks that I claimed all my concepts to be "really functions." As long as I remember, I have believed that any psychological theory of memory that does not rely on *both* structural concepts and functional ones is doomed to failure. The history of our field offers ample testimony to the simple fact that neither category suffices by itself.

Watkins's second complaint is that the distinction between episodic and semantic memory in Part I is not related well to the work on episodic memory that I describe in Parts II and III. That is not quite true. The latter two parts of the book illustrate the usefulness of the distinction as well as the synergistic involvement of both systems in memory performance. For example, one of the basic tenets of the notion of encoding specificity is that a strong semantic association between items A and B is no guarantee that one is an effective episodic retrieval cue for the other. For another example, two major concepts of the synergistic ephory model are the episodic memory trace and the semantic retrieval cue. But Watkins's complaint is justified in that I failed to treat the connection formally, or at least explicitly. Watkins is also right in his perception that retrieval processes in episodic and semantic memory had become more similar in 1983 than they had been in 1972, as well as his observation that such a state of affairs weakens the distinction between the two.

In his third criticism, regarding the uncertainties of the distinction between procedural and propositional memory, Watkins slips into the 1983 error of assuming some sort of a one-to-one correspondence between tasks and systems. Since the assumption is generally recognized to be wrong in 1988, the criticism misses the mark.

Watkins's most telling criticism is the fourth one: the "puzzle" of the conversion threshold in the diagram of the synergistic ephory model. I offered that diagram in *Elements* as a graphic illustration of the essential components of the model. When it is taken literally, trouble arises. Watkins makes the problem clear: The diagram depicted in Figure 14.3 invites the assumption of ordinal properties of the two scales; this assumption is at variance with the conceptualization of cues and traces as qualitatively different feature bundles. I was not totally unaware of the problem, but responded to its existence by equivocation. Thus, I said in *Elements* (p. 314) that the diagram depicts the model "in only a very rough fashion," and that its "scale units cannot be specified." Watkins's suggestion for the modification of the diagram is an excellent one.

References

- Tulving, E. (1983) *Elements of episodic memory*. Clarendon Press/Oxford University Press. [MJW, ET]
(1984) *Precis of Elements of episodic memory*. *Behavioral and Brain Sciences* 7:223-68.

Commentary on Daniel R. Vining, Jr. (1986) Social versus reproductive success: The central theoretical problem of human sociobiology. *BBS* 9:167-216.

Abstract of the original article: The fundamental postulate of sociobiology is that individuals exploit favorable environments to increase their genetic representation in the next generation. The data on fertility differentials among contemporary humans are not consistent with this postulate. Given the importance of *Homo sapiens* as an animal species in the natural world today, these data constitute a particularly challenging and interesting problem for both human sociobiology and sociobiology as a whole.

The first part of this paper reviews the evidence showing an inverse relationship between reproductive fitness and "endowment" (i.e., wealth, success, and measured aptitudes) in contemporary, urbanized societies. It is shown that a positive relationship is observed only for those cohorts who bore their children during a unique period of rising fertility, 1935-1960, and that these cohorts are most often cited by sociobiologists as supporting the central postulate of sociobiology. Cohorts preceding and following these show the characteristic inverse relationship between endowment and fertility. The second section reviews the existing sociobiological models of this inverse relationship, namely, those of Barkow, Burley, and Irons, as well as more informal responses among sociobiologists to the persistent violation of sociobiology's central postulate, such as those of Alexander and Dawkins. The third section asks whether the goals of sociobiology, given the violation of its fundamental postulate by contemporary human societies, might not be better thought of as applied rather than descriptive, with respect to these societies. A proper answer to this question begins with the measurement of the pace and direction of natural selection within modern human populations, as compared to other sources of change. The vast preponderance of the shifts in human trait distributions, including the IQ distribution, appears to be due to environmental rather than genetic change. However, there remains the question of just how elastic these distributions are in the absence of reinforcing genetic change.

Reproductive success and adaptation

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Only two commentators (co-authors of a single commentary) accept Vining's (1986) contention that the "fundamental premise" of "sociobiology" is that human action should be designed to promote reproductive success. Most of the commentators emphasize that human action cannot be expected to be so designed in environments that differ radically from the environment of evolutionary adaptedness (EEA), that is, the Pleistocene world of nomadic foragers in which human nature evolved. It seems to me, however, that both the target article and the commentaries

on it, including my own, illustrate how a narrow focus on reproductive differentials can obscure the real issue: adaptation.

Among nonhuman animals in their EEA, high status individuals are, on the average, more fit than low status individuals because the former typically have access to greater resources and convert these resources into offspring. High status or low status, however, each individual is presumably converting resources into offspring about as efficiently as it can, given its particular opportunities and constraints. In such circumstances, comparing the reproductive rates of high and low status individuals might conceivably illuminate some aspect of adaptation.

Analogous reproductive data on human beings in modern industrial societies would be likely to illuminate adaptation only if there were, in the first place, some reason to believe that the individuals in these societies (like nonhuman animals in their