

More on the Distinction Between Episodic and Semantic Memories

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In reply to a critique of the episodic/semantic distinction (McKoon, Ratcliff, & Dell, 1986), Tulving (1986) argues that the usual rules for testing theories do not apply to the classification of phenomena with respect to memory systems. But we respond that the classification methods proposed by Tulving (1986) may be detrimental to the advancement of theory and that whole domains of inquiry should not be excluded from the usual criteria of experimental psychology.

In a review of recent research and theoretical proposals concerned with the semantic/episodic distinction, McKoon, Ratcliff, and Dell (1986) came to the conclusion that the empirical and neuropsychological evidence cited by Tulving (1983, 1984) to support the distinction is weak and, further, that it will not be possible to support the distinction unless a specific theory is developed. In reply, Tulving (1986) declined to defend his previous arguments, and suggested that a new approach is needed to the problem. He proposed that the distinction should be validated not by its success according to the hypothetico-deductive method of testing theories but rather by its usefulness as a scheme for classifying various kinds of phenomena.

In response to Tulving's proposal, we make several points. First, we argue that the classification approach is not guaranteed to succeed, and in fact, that it has shown few signs of success with respect to the episodic/semantic distinction. Second, we claim that the application of an episodic/semantic classification scheme to neuropsychological evidence, as proposed by Tulving (1986), itself will assume some particular episodic/semantic theory. Finally, we express our disagreement with the pessimistic view of cognitive psychology implied in Tulving's (1986) article.

In some fields, as Tulving (1986) mentions, a classification approach has proved useful and, in general, we agree that multiple approaches are worthwhile. But in some domains, the classification approach has been detrimental. For example, Chomsky (1968) criticized structural linguistics: "It was a failure because such techniques [segmentation and classification] are at best limited to the phenomena of surface structure and cannot, therefore, reveal the mechanisms that underlie the creative aspect of language use and the expression of semantic content" (p. 19). Chomsky also noted that structural linguistics did provide an empirical base and a focus on questions of interest. But in cognitive psychology, there already exists a data base, and many questions of interest have already been identified. Thus, Chomsky's criticisms should be taken as a warning that the classification approach advocated by Tulving is not guaranteed to lead to new progress and insights and may actually hinder progress.

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Another warning comes from examination of the history of the episodic/semantic distinction in cognitive psychology. Tulving concedes that the evidence favoring the distinction is not strong (1986, p. 308). We want to point out that the evidence is just as weak whether examined from the point of view of testing the distinction as a theory or as a classification scheme. Whichever way the distinction is viewed, the years of research since 1972 have not come close to providing a convincing body of evidence. Furthermore, the fact that the evidence appears just as weak from either viewpoint suggests that the two viewpoints are not really so different. If McKoon et al. (1986) did indeed make a "category mistake" in viewing the distinction as a testable theory (Tulving, 1986), then it was a mistake of small consequence in examining past episodic/semantic research.

The weakness of the evidence stands in contrast to the evidence that could be accumulated with respect to the analogical distinction suggested by Tulving (1986) between the visual and auditory systems. A scientist on a far away world would be able to accumulate a great deal of evidence to support the visual/auditory distinction. The physical separation of the physiological mechanisms would lead to informal observations of different kinds of neural damage on different kinds of behaviors. Simple theories could be developed to predict and understand experimental dissociations, and once these were successful at predicting nonobvious outcomes, the bandwagon would begin to roll. More complete theories about the operations of the two mechanisms would begin to guide experimental work and to uncover relationships among sets of data that were not otherwise apparent (Estes, 1975, p. 271). It is probably the success of this theoretical stage that would convince the scientific community of the validity of the distinction. We believe that such success could be achieved in an amount of time equivalent to the 12 to 14 years of research that has been spent on the semantic/episodic distinction. Of course, research does not ever proceed in the idealized, orderly manner just described so optimistically; accidents, false starts, and incorrect theories would all have their impact. But once some simple version of a correct theory was discovered, then grant funding and, eventually, a consensus among researchers would follow.

Tulving (1986) suggests that much of the future evidence to support the episodic/semantic classification will come from neuropsychological observations. The problem with these observations is that they themselves are dependent on theoretical interpretation. For example, clinical intuition is involved in deciding exactly what it is that a particular patient cannot do. Diagnoses based on test results may depend on the specific battery of tests that is chosen, a choice that must be based on theoretical issues.

Also, neuropsychological evidence is quite variable; patterns of symptoms vary among patients supposedly having the same disorder, and the criteria used to classify them must be theoretically based.

Extrapolation of Tulving's (1986) arguments for the necessity of neurological evidence presents a pessimistic view of cognitive psychology. From this view, we must expect that few theories or distinctions will be validated without recourse to neuropsychological data. We do not share this pessimistic view and make two counterarguments. First, Hopfield (1984) points out that it is impossible to understand the function of a computer chip through knowledge of its circuitry, and argues by analogy that it would be impossible to understand the processing of higher mental functions through knowledge of physiology. We think that trying to understand the mind by using neuropsychological data alone would be like trying to understand the programs running on a large time-sharing computer by using a sledge hammer to create lesions. It may be that the conjunction of neuropsychological data with cognitive techniques will give some useful information, but the sledge hammer analogy suggests that the application of this information to normal cognitive processing, though potentially fruitful, will be limited in scope. Second, we reiterate that neuropsychological data are interpreted with respect to behavioral theories. For example, the same kinds of data from the same kinds of amnesic patients that were once interpreted in terms of short- and long-term memories were later interpreted in terms of levels of processing, cue-dependent retrieval, and episodic and semantic memories, and most recently have been interpreted in terms of procedural and declarative memories (cf. Crowder, 1982), though throughout this transition, advances in understanding have been made. This is not to say that tomorrow new neuropsychological data will not help resolve theoretical issues in cognitive psychology, but so far, the direction of major impact has been from cognitive psychology toward neuropsychology rather than the reverse.

Tulving (1986) is especially pessimistic about the use of reaction time measures; he casts doubt on the importance of "complex reaction time experiments." As a specific example, he cites the sensitivity of some outcomes in priming experiments to minor variations in procedure (Neely & Durgunoglu, 1985). But, contrary to the impression given by Tulving (1986), it is possible to generalize over the variations in a way relevant to the episodic/semantic distinction: Whenever episodic priming is obtained at short stimulus onset asynchronies, then so is semantic priming (McKoon & Ratcliff, 1986). We also disagree with Tulving's dismissal of reaction time experiments because they require complex interpretations and wonder whether fragment completion experiments (Tulving, Schacter, & Stark, 1982), in which subjects engage in slow, time-consuming, strategic, inferential processing, are any less complex. Finally, we point to the success of at least one class of reaction time model, the random walk models, in its ability to encompass a wide range of tasks and results (cf. Laming, 1968; Link, 1975; Luce, in press; Ratcliff, 1978, 1981, 1985).

To close, we must agree with earlier sentiments expressed by Tulving in collaboration with Bower (Tulving & Bower, 1974) in their review of techniques used to discern representations of memory traces, and we endorse a slightly modified version:

It is only in the context of a particular process model that inferences can be meaningfully drawn from the experimental data. This restriction holds up regardless of what method is used. When we noted, in discussing various methods, that the logic was not entirely clear, the difficulty usually lay in the absence of a set of statements or assumptions about the principles governing the utilization of the stored information at the time of retrieval. The process models that would help make sense out of the data on trace features [or memory systems] need not be complex or highly sophisticated or "correct." But they must enter the picture in some form. Even a bad process model is better than none at all. It would help make clear the logic of the method of specifying trace properties [or memory systems], aid communication, and facilitate cross-comparisons of data obtained with different methods. Moreover, it can be improved, revised, or replaced with a better one. A nonexistent model cannot. (pp. 296-297)

Our modest contributions to this statement are the words in brackets.

References

- Chomsky, N. (1968). *Language and mind*. New York: Harcourt, Brace, & World.
- Crowder, R. G. (1982). General forgetting theory and the locus of amnesia. In L. S. Cermak, (Ed.), *Human memory and amnesia* (pp. 33-42). Hillsdale, NJ: Erlbaum.
- Estes, W. K. (1975). Some targets for mathematical psychology. *Journal of Mathematical Psychology*, 12, 263-282.
- Hopfield, J. J. (1984). Collective processing and neural states. In C. Nicolini (Ed.), *Modeling and analysis in biomedicine* (pp. 369-389). New York: World Scientific Publishing Co.
- Laming, D. (1968). *Information theory of choice reaction time*. New York: Wiley.
- Link, S. (1975). The relative judgment theory of choice reaction time. *Journal of Mathematical Psychology*, 12, 114-135.
- Luce, R. D. (in press). *Response times and mental structure: Models of simple and choice reactions*. Oxford, England: Oxford University Press.
- McKoon, G., & Ratcliff, R. (1986). Automatic activation of episodic information in a semantic memory task. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 12, 108-115.
- McKoon, G., Ratcliff, R., & Dell, G. (1986). A critical evaluation of the semantic/episodic distinction. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 12, 295-306.
- Neely, J. H., & Durgunoglu, A. Y. (1985). Dissociative episodic and semantic priming effects in episodic recognition and lexical decision tasks. *Journal of Memory and Learning*, 24, 466-490.
- Ratcliff, R. (1978). A theory of memory retrieval. *Psychological Review*, 85, 59-108.
- Ratcliff, R. (1981). A theory of order relations in perceptual matching. *Psychological Review*, 88, 552-572.
- Ratcliff, R. (1985). Theoretical interpretations of the speed and accuracy of positive and negative responses. *Psychological Review*, 92, 212-225.
- Tulving, E. (1983). *Elements of episodic memory*. New York: Oxford University Press.
- Tulving, E. (1984). *Precis, Elements of episodic memory. The Behavioral and Brain Sciences*, 7, 223-268.
- Tulving, E. (1986). What kind of a hypothesis is the distinction between episodic and semantic memory? *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 12, 307-311.
- Tulving, E., & Bower, G. H. (1974). The logic of memory representations. In G. H. Bower (Ed.), *The psychology of learning and motivation* (Vol. 8, 265-298). New York: Academic Press.
- Tulving, E., Schacter, D. L., & Stark, H. A. (1982). Priming effects in word-fragment completion are independent of recognition memory. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 8, 336-341.

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