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Ebbinghaus's Memory: What Did He Learn and Remember?

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Ebbinghaus held a unitary view of learning and memory. Yet it is possible to discern rudimentary evidence for different kinds of learning and memory even in Ebbinghaus's original research. The different kinds include memory underlying recollective experience (episodic memory) that has been declared to be epiphenomenal by Slamecka (1985). I argue that all the different kinds could have been studied by Ebbinghaus, and can be studied now, using the same general methods that Ebbinghaus adopted for his own epoch-making work a hundred years ago.

The sharp focus of Ebbinghaus's experimental work belies the breadth of his vision of memory. In his epoch-making monograph (Ebbinghaus, 1885/1964) and especially in his masterful textbook of psychology (Ebbinghaus, 1902), he treats memory in its broadest sense, doing so in an enlightened manner befitting a great thinker. The analysis of the concept of association is interwoven with anecdotes and examples of memory phenomena in everyday life; the discussion of the role of similarity relations in metaphors is juxtaposed with ponderings about memory in animals; and references to what we would now refer to as source amnesia and the important role that organization plays in memory are made in passing as if they were facts beyond any dispute.

True to his own time, Ebbinghaus regarded memory as a *mental* phenomenon. The two terms he used most frequently to refer to the general category of manifestations of memory were "Vorstellungen" and "Seelische Gebilde", usually translated into English as "ideas" and "mental states." Both these concepts lose slightly in translation, inasmuch as the German terms connote the involvement of imagery and consciousness in the mental structures they designate in a way that their English equivalents do not quite do. This was important to Ebbinghaus in that he was greatly concerned with the relation between memory and consciousness. He dealt with the relation on the very first two pages of his 1885 monograph, equating "remembering" with the return to consciousness of earlier mental states.

Consciousness and Recollective Experience

In *Elements of Episodic Memory* (Tulving, 1983), I suggested that, with respect to consciousness, Ebbinghaus's thinking was ahead of succeeding generations of toilers in the field that he created, who, until most recently, have seldom raised questions concerning the role of consciousness in memory (cf. Klatzky, 1984; Tulving, 1985b; Underwood, 1979). Now Slamecka, in his wide-ranging review of Ebbinghaus, his work, and his influence on the field (Slamecka, 1985) remonstrates that I unjustly blamed Ebbinghaus for the shortcomings of his successors. I had said that, "Had Ebbinghaus . . . clearly articulated the difference between remembering past episodes and knowledge of their symbolic contents, the history of verbal learning and memory might have been quite different" (Tulving, 1983, p. 129). Slamecka maintains that "Ebbinghaus did clearly articulate the difference, and even went on to report the first data ever collected on its behalf" (Slamecka, 1985, p. 430).

Ebbinghaus, in one of his experiments, had observed an apparent discrepancy between what he remembered about learning the series of nonsense syllables and what his saving measure indicated about the retention of the learned series. (I will describe the situation somewhat more fully later). Although Ebbinghaus himself chose not to draw

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any "general conclusions" from his observations, confining his treatment of them "to pointing out this noteworthy fact" (Ebbinghaus, 1885/1964, p. 59).[1] Slamecka has seen fit to draw one such conclusion, together with a provocative corollary. The conclusion is that recollective experience in Ebbinghaus's experiment(s) was an "epiphenomenon" (Slamecka, 1985, p. 431). The corollary consisted of Slamecka's opinion that it is difficult to study recollective experience anyhow, because of "the problem of a lack of any independent, objective, accuracy indicator" (Slamecka, 1985, p. 431).

Having made a number of statements regarding the central role that recollective experience plays in episodic remembering in *Elements*—statements that Slamecka (1985, p. 430) quotes—I obviously cannot sit idly by when the field of study that our revered founder created appears to be condemned forever to a fate of endless quantification of the extent to which people are capable of behaving like time-delayed copy machines. In this article, I will argue that Ebbinghaus's "noteworthy fact" can be regarded as evidence for the dissociation of two kinds of memory, and that another fact that emerged from his research points to the existence of a third kind of memory. I will further argue that none of the three kinds of memory is epiphenomenal and that all can be studied in a manner of which Ebbinghaus would have approved.

The question of whether memory is in some sense unitary or whether it consists of different kinds, or different systems, is currently under scrutiny (e.g., Oakley, 1983; Tulving, 1984, 1985a; Weinberger, McGaugh, & Lynch, in press). This is why this particular issue raised by Slamecka (1985) may be more than just of historical interest. We can air it under the rubric of the question posed in the title of this article: What did Ebbinghaus learn and remember?

One type of evidence for the existence of different kinds of memory is provided by dissociations in memory performance: An independent variable affects one performance but not another, or two or more groups of subjects perform similarly on one task but not on another. Ebbinghaus's (1885/1964) original research reveals evidence of two kinds of dissociation. One has to do with the relation between semantic and procedural knowledge, the other one with the relation between semantic and procedural knowledge on the one hand and episodic knowledge on the other.

Ebbinghaus's Method

In order to understand the first dissociation in Ebbinghaus's findings, we must be quite clear about the method that he used. Slamecka (1985) has reminded us what it was. Its truly unique aspect was the very rapid rate at which Ebbinghaus, as the single subject of all of his experiments, recited the series of nonsense syllables: 150 syllables per minute, or .4 s per syllable. Ebbinghaus considered a series mastered when he could proceed through its constituent syllables at this very rapid rate without a single error or hesitation. He does not tell us why he adopted this rate, but we may surmise that it could have been a part of his overall grand strategy of creating conditions under which the effects of all prior knowledge were eliminated and the learning of a series carried out "solely by the influence of the mere repetitions upon the *natural memory*" (Ebbinghaus, 1885/1964, p. 25, italics added).

Many people, even among those who have always known that Ebbinghaus recited his syllables at the rate of .4 s/syllable, may not quite realize what a feat it is to recite by heart a series of nonsense items at this pace. The best way to find out what it means is to try to learn, or observe someone else learn, a serial list of nonsense items at Ebbinghaus's rate. It is difficult, taxing, and stressful. Even Ebbinghaus, highly motivated and experienced as he was, referred to his task as "tiresome" (Ebbinghaus, 1885/1964, p. 25). Most other people, especially in today's world, are probably incapable of mastering any longer lists of nonsense syllables under these conditions.

An Ebbinghausian Experiment

To convey an impression of the task, let me briefly describe a simple, informal dem-

[1] Throughout this essay, page numbers given for Ebbinghaus, 1885, refer to the 1964 Dover edition of the translation of "Über das Gedächtnis".

onstration experiment that I conducted as a "special project" for the present article. Six students and assistants from the laboratory were given the task of reciting the alphabet backwards. They were told that the objective was to see how quickly they could learn to go through the 26 letters, from *Z* to *A* in 10.4 s.

The experiment turned out to be a failure inasmuch as 2 of the 6 subjects became visibly upset and distressed after their performance steadily deteriorated, and I had to terminate it for the sake of values higher than those of psychological science. But observations were available from all 6 subjects on 12 trials, and I describe these here.

Subjects were tested individually. They were initially given a card with a listing of letters of the alphabet in reverse order, and they were asked to read it at a brisk rate. At the end of the first reading, they were told what their time was and asked to either speed up or slow down on the next reading. When they came close to reading the sequence in 10.4 s, they were told to make a mental note of the pacing—their objective was to learn to recite the letters by heart at the same rate—and the experimental trials commenced.

For the first six experimental trials, subjects recited the letters while holding the card with a listing of the letters in their hand. They were permitted to consult the card whenever they "got stuck." After six trials, the card was removed, and the subjects were told that they were on their own, although the experimenter would prompt them whenever they took too long to say the next letter and would correct them whenever they skipped any letters or made a transposition error. In these latter cases they had to go back to the last correct letter and proceed from there.

Even among only 6 subjects, individual differences were large. The response times of 2 of the subjects, who found the task particularly offensive, increased dramatically from the first block of six trials to the second, counteracting the improvement shown by the other subjects in the course of the 12 trials. As a consequence, for the small group as a whole there was no improvement in the mean amount of time required to go through the list: the mean was 24 s on both Trials 1 and 12, with most subjects making a number of omission and commission errors on all trials. Only 1 of the 6 subjects came close to reaching the criterial performance on Trial 12, going through the list without error, and at a steady pace of 11.2 s.[2]

The point of the experiment is this. Here is a known series of items: All subjects know the names of all the items as well as their positions in the series before they even begin to learn the series. They know the series in the sense that they could write down the sequence in a way—faultlessly and reasonably rapidly—that they possibly could not do with many other series of items, for instance, the names of the days of the week in German, or in Estonian. The learning involved in the task given to them, therefore, did not entail any acquisition of new semantic knowledge. Instead, it entailed the acquisition of procedures required for the expression of existing knowledge in a particular fashion, reciting the members of the series at a very rapid pace. The subjects' semantic knowledge of the series is only a necessary but not a sufficient condition of the specific procedural knowledge. In this sense, the two kinds of knowledge and memory are dissociated in this Ebbinghausian experiment.

Knowledge and Its Expression

The presence of two dissociable components in an experimental situation such as the one just described makes it reasonable to assume that the two components—semantic and procedural—were also entailed in what Ebbinghaus (1885/1964) learned in his own experiments. Because he himself relied on only a single measure of performance—time required to learn or relearn a series to "mastery"—he missed the dissociation. But the heavy involvement of pure response production in Ebbinghaus's learning has not escaped the attention of others. Thus, for instance, Woodworth, in his well-known textbook of

[2] Any reader who believes that the task may have been more difficult than reciting a list of *neutral* syllables, because of possible negative transfer from the subjects' knowledge of the alphabet in its normal order, should feel free to repeat the experiment with such a list. It may be prudent, however, to employ subjects who are remunerated for their services rather than risk testing one's students, colleagues, or friends.

experimental psychology, observed that when Ebbinghaus read and recited a particular series over and over again, such recitation became more and more fluent, until finally he could rattle off the series automatically: "The series has become a *motor habit*" (Woodworth, 1938, p. 30, italics added).

The point I am making here is that Ebbinghaus's learning entailed both motor habit and mental associations, both the procedural knowledge, expressed by rattling off a rapid sequence of nonsense sounds, and semantic knowledge, knowledge concerning the symbolically representable contents of learned series that could have been expressed procedurally in a number of ways (cf. Spear & Isaacson, 1982). It is reasonable to assume that Ebbinghaus, in learning a series, acquired the semantic knowledge before he reached his self-imposed criterion of procedural mastery.

We can glean one bit of evidence in support of these ideas from Ebbinghaus's own work. The relevant data come from experiments described in Sections 22 and 23, as well as Section 41 of his monograph, with the data summarized in tables (pp. 52–58). (Ebbinghaus, 1885/1964, p. 56 and p. 116). Slamecka has described the first and the second parts of these data in his review (Slamecka, 1985 pp. 430–431 and pp. 417–418). The data concern the learning of both the original and derived series of 16 syllables on Day 2, following variable amounts of learning of the corresponding series on Day 1. These data are graphically depicted in Figure 1. Retention of learned series, measured in terms of savings in relearning on Day 2, is shown as a function of number of repetitions of the series on Day 1. For the original series the relation is strikingly orderly and linear, the kind of data that all graduate students working on their theses dream about. Thus, for instance, as the number of repetitions of the series increased from 32 to 64 on Day 1, the saving in relearning of the same lists increased proportionally from 407 to 816 s. For the derived series, however, in which the series learned on Day 2 were derived from the original series by skipping one syllable, increasing the number of repetitions on Day 1 from 32 to 64 had only a negligible effect on saving in (re)learning of the derived lists, from 149 to 161 s.

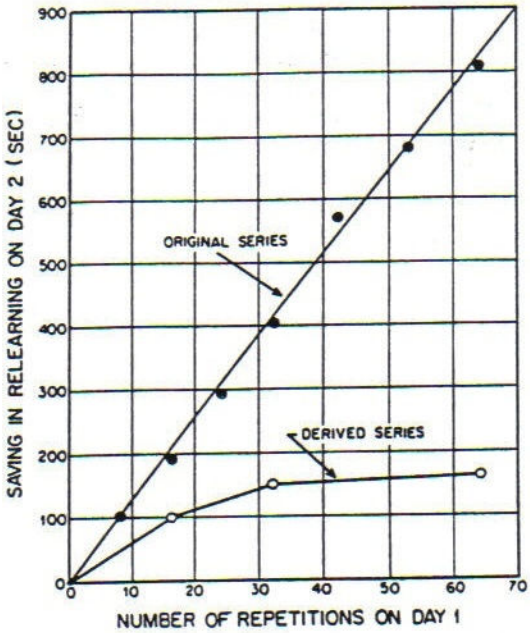

Figure 1. Savings in relearning on Day 2 as a function of number of repetitions on Day 1, for original and derived series. (Data from Ebbinghaus, 1885/1964).

A reasonable conclusion from this interaction between level of original learning and type of list is that some unknown product of overlearning contributed greatly to the relearning of the original series but not to the learning of derived series: That unknown something was apparently not required for, or not relevant to, the learning of derived lists. The data suggest that relearning of the original series benefitted from the previous acquisition of both semantic and procedural knowledge, whereas learning of the derived series benefitted primarily from the earlier acquisition of relevant semantic knowledge.

Retention and Remembering

Superimposed on the hypothesized dissociation just discussed is another, one between the retention of the series as reflected in the savings measure (entailing both procedural and semantic components) and the recollective experience of the original learning. As Slamecka (1985, pp. 430–431) has pointed

out, Ebbinghaus was not only aware of this dissociation but discussed it. With respect to the data on relearning of the original series in Figure 1, Ebbinghaus said that the series repeated 8 or 16 times on Day 1 had become unfamiliar to him on Day 2 and that he "did not recognize them". The series repeated 53 or 64 times, on the other hand, were "old acquaintances", they were "remembered distinctly". The important point, for both Ebbinghaus and Slamecka, is then put by Ebbinghaus as follows: "Nothing corresponding to this difference is evident in the times for memorization and for savings of work respectively. They are not smaller *relatively* when there is no possibility of recollection nor larger *relatively* when recollection is sure and vivid" (Ebbinghaus, 1885/1964, p. 59). It is this lack of correlation between objectively measured retention and subjective feelings of familiarity that led Slamecka to pronounce recollective experience to be an epiphenomenon and to express doubts about the possibility of its study by the methods of science.

Slamecka's conclusion and his discouraging prognosis for recollective experience as an object of scientific investigation are open to question. We would not want to declare, say, color vision an epiphenomenon simply because it is not correlated with other visual capabilities of organisms, such as their ability to detect and discriminate fine spatial details. Similarly, a lack of correlation between two memorial functions constitutes a poor reason for thinking of one of them as unreal or unimportant.

It seems more reasonable to assume that the lack of correlation between recollective experience and the objective indices of retention used by Ebbinghaus implies the existence of two different kinds of memory. One kind—we could call it *episodic*—is reflected in the vividness and compellingness of the learner's phenomenal awareness of past events, the other—semantic plus procedural—is defined by the amount of time saved in reaching a specified level of mastery of a complex sequence of spoken sounds. Ebbinghaus's noteworthy fact simply means that episodic remembering can be dissociated from semantic retention expressed through particular procedures. Even Ebbinghaus was fully aware of this fact, discussing it with the aid of several examples from real life (Ebbinghaus, 1902, p. 616).

What about the possibility of studying recollective experience? We have good reasons to believe that the prospects for doing so are bright. All we need to do is to follow Ebbinghaus's own principles of application of the scientific method to the study of mental processes. There were three such principles: First, the mental happenings had to be translated into observable behavior. Second, the observable behavior had to be reliably quantifiable. And third, behavior thus quantified had to vary systematically with conditions of observation. Psychophysical observations concerning the relation between physical and sensory magnitudes provide a good example. In measuring subjective loudness of tones, for example, the experimenter is interested in subjective experience. He or she cannot possibly invoke the criterion of veridicality or "accuracy" of the report because they are precluded by the very nature of the enterprise. The important findings pertaining to subjective loudness are that the subjects' judgments are quantifiable, reliable, and reproducible, and that they are systematically related to the directly measurable properties of sound stimuli (e.g., Stevens, 1956; Stevens & Tulving, 1957).

The situation is very much the same with respect to recollective experience. To think otherwise would be tantamount to ignoring the most important lesson that Ebbinghaus taught the world: even complex mental phenomena can be embraced by the methods of science. We have seen that his own recollective experience was systematically related to the independent variables of list length and study time: he remembered the longer series but not the shorter ones. More recently it has been shown that the extent to which people remember—rather than know on some other basis—miniature events that they recall or recognize in the memory laboratory, varies systematically with variables such as the amount of relevant information in the retrieval environment and the length of the retention interval (Tulving, 1985b). The logical status of such observations is not different

from that of the relations observed in psychophysical experiments on magnitude estimation or many other comparable situations.

What Would Ebbinghaus Think?

How would Ebbinghaus react to the suggestion that he learned and remembered several different things in his experiments that could have been measured separately and that the dissociations among the measures point to the existence of different kinds of memory? From what we know about him, he probably would have reacted favorably. He would not have resisted the idea that memory is only a general name for a number of separate but interrelated mental functions, or systems, which together serve to help the organism to benefit from past experience, but which individually do so in somewhat different ways. He would certainly approve of the pursuit of such an hypothesis, for he was an open-minded scientist, forever ready to reach beyond what is known. In discussing the difficulty of studying memory and the inadequacy of existing knowledge concerning it, he expressed what must have been a fundamental conviction when he said that

> If by any chance a way to a deeper penetration into this matter should present itself, surely, considering the significance of memory for all mental phenomena, it should be our wish to enter that path at once. For at the very worst we should prefer to see resignation arise from the failure of earnest investigations rather than from persistent, helpless astonishment in the face of their difficulties. (Ebbinghaus, 1885/1964, p. 6)

A hundred years later we, too, should be more willing to face the possibility of failure of the scientific study of as yet little understood facets of memory than to give up in resignation without trying.

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