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Remembering Ebbinghaus

A Review of

Memory: A Contribution to Experimental Psychology

by Hermann Ebbinghaus

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Reviewed by

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This year marks the centennial of Ebbinghaus's (1885/1964) great book Über das Gedächtnis, [1] which records one of the most remarkable research achievements in the history of psychology. To prepare myself for writing this retrospective review, I conducted a haphazard poll of my colleagues and some advanced graduate students in the halls of my department. The main findings of this unscientific study are (a) everyone who has (or is near) a PhD in psychology has heard of Ebbinghaus; (b) most know he studied memory and invented nonsense syllables for the purpose; and (c) a few could relate the basic ideas of his relearning and savings measures of memory and his famous forgetting curve. Finally, (d) no one, aside from a very few "psychonomes," has ever read his marvelous book.

The knowledge of my respondents is accurate so far as it goes but depressingly incomplete. It is like summarizing Skinner's contributions by saying that he measured responses of pigeons, gave them food, and taught them how to play ping-pong. Several readings of Ebbinghaus's book have convinced me that he was one of psychology's foremost pioneers, ranking with (if not ahead of) others from his time who are remembered more favorably today. In this review I will try to capture briefly his major contributions—his basic aims, methodological innovations, and most important findings—from a contemporary perspective, discuss some common criticisms of his work, and provide an evaluation.

The aim

A first notable achievement is that Ebbinghaus chose to undertake the study of memory at all. Experimental psychology was a fledgling enterprise when Ebbinghaus began his research in the late 1870s. Philosophers such as Herbart had argued that an experimental science of higher mental processes was impossible, in principle. Ebbinghaus borrowed from Herbart theoretical ideas concerning formation of associations, but not this presupposition. Ebbinghaus himself seemed doubtful about prospects for the scientific study of memory. After detailing the inadequacies of folk knowledge concerning memory in Chapter 1, he ends by writing:

> It remains to be proved whether, in spite of the clearest insight into the inadequacy of our knowledge, we shall ever make any actual progress. Perhaps we shall always have to be resigned to this. … If by any chance a way to 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 persistent, helpless astonishment in the face of their difficulties. (pp. 5–6)

Exactly how Ebbinghaus conceived his ideas and methods for studying memory is unclear; certainly he gives almost no inkling in his book. We know that he studied the British associationists' work in depth and that he discovered a copy of Fechner's (1860/1966) *Psychophysics* in a Paris bookstand. [2] His basic goal was to attempt a "natural science" of remembering by applying its exact methods. He credits Fechner for his inspiration (Boring, 1950, p. 387), but Ebbinghaus's ingenious techniques are quite different from Fechner's psychophysical methods. In Chapter 2 Ebbinghaus outlines "the possibility of enlarging our knowledge of memory" by the methods of natural science. There he discusses such issues as the ability to manipulate one factor while holding others constant, establishing sensitive and reliable measures, averaging over large numbers of observations, and the causes of error in measurements. But it is one thing to lay out general principles of how a scientific approach to memory should be conducted; it is quite another trick to develop suitable methods to accomplish this goal. Ebbinghaus's great genius was as a methodologist and experimentalist. I turn now to these contributions.

The methods

Chapters 2 through 4 of the book constitute what would today be called the Method section. In Chapter 2 Ebbinghaus lays out his general plan, in Chapter 3 he describes his specific procedures, and in Chapter 4 he evaluates the general utility of the results obtained. The achievements here are nothing short of astounding, for he brings forth a science of memory and associations where before had been only centuries of speculation. Ebbinghaus was a meticulous experimenter and provided detailed descriptions of his procedures and controls.

He chose serial learning as his task: reading aloud a series of nonsense syllables and repeating them back, all in time to a metronome. He continued learning until he could repeat a series one time perfectly and without hesitation. His materials were the famous nonsense syllables constructed to minimize the influence of prior knowledge.[3] He wanted a relatively homogeneous set of materials, but he noted that even nonsense syllables exhibited an "almost incomprehensible variation" (p. 23) in the ease with which they could be learned. Serial learning was a natural task, for it seemed to capture the associationists' concept of a flow of associations, each succeeding one produced by associative connections (the "invisible threads," as he sometimes referred to them) binding the series together.

Perhaps the most ingenious aspect of Ebbinghaus's methods is his dependent measure, savings obtained in relearning a series. As Ebbinghaus noted, if one's criterion of memory is reproduction (which is often the situation today), then only two main outcomes are possible—Either one can reproduce the series or one cannot. But suppose that a poem is learned by heart and that when the learner is tested after half a year, "no effort at recollection is able to call it back again into consciousness" (p. 8). Can the memory for such an experience never be studied? Does any trace of the experience exist? The relearning/savings method permits an answer to this question, for the poem can be relearned and one can determine if the number of trials (or amount of time) to accomplish the relearning is smaller than in the original learning. Assuming savings is shown in relearning, its magnitude reflects the amount of information retained.

In most of the actual experiments Ebbinghaus measured the number of trials or amount of time to learn a series of nonsense syllables or a poem, and then the trials (or time) to relearn it later, as a function of some independent variable such as number of repetitions of the original series. Savings could be measured either in absolute terms or relative to the amount of effort in original learning. Thus, savings permitted graded determination of retention even when material had been completely forgotten as determined by "introspective means"—that is, conscious attempts at recall. (Savings has somewhat fallen out of favor as a measure of retention these days, but it is still put to ingenious use and may be due for a resurgence [e.g., Kolers, 1976; Nelson, 1985].)

Another important contribution was Ebbinghaus's lucid summary of statistical procedures, which were just being invented as he wrote. He introduced the concepts of the mean, variability, measurement of the "probable error," sources of measurement error, and the idea of comparing performance in different conditions by seeing if the mean difference between them exceeds what would be expected on the basis of the probable error (defined as the 50% confidence interval). He also described more complicated statistical concepts, such as the sampling distribution of means (p. 18) and the law of large numbers (p. 38), although he did not use these terms.

He argues in Chapter 4 that the utility of his methods can be demonstrated in the first instance by how well his learning and savings data fit the "law of errors" (i.e., approximate a normal distribution) and by how low the variability is. He shows convincingly that the data generally fell along the normal curve, although some positive skew existed (p. 35). In addition, the variability of his observations is reasonably low; he reports that variability in his data compares favorably with that of Helmholtz in measuring the speed of the nervous impulse and with that of the physicist Joule in determining the mechanical equivalents of heat (Joule's law). Ebbinghaus's treatment of statistics was to have continuing influence, for decades later Titchener still recommended it to his students to shore up their mathematical training (Hilgard, 1964).

The actual conduct of experimental sessions was fastidious, even by modern standards. Ebbinghaus describes in detail how he strove to keep testing conditions constant, worrying about such matters as intonation in reading the series, fatigue, avoidance of special strategies, and even the influence of circadian rhythms. ("Since the mental as well as the physical condition of man is subject to an evident periodicity of 24 hours, it was taken for granted that like experimental conditions are obtainable only at like times of day" [p. 23].) Learning long lists of nonsense syllables every day and then relearning them later is described, charitably, as "a tiresome task" (p. 25) that often had unpleasant side effects. He notes that in the course of one experiment (Chapter 6) on the effects of repetition on retention, he could not take repetition of a series beyond 64 presentations: "For with this number each test requires about 3/4 of an hour, and toward the end of this time exhaustion, headache, and other symptoms were often felt which would have complicated the conditions of test if the number of repetitions had been increased" (p. 55).

The sheer magnitude of the task Ebbinghaus set himself was immense. The first series of experiments was conducted in 1879 and 1880; many of them were replicated and new experiments conducted in 1883 and 1884. For example, in the experiment on repetition mentioned above, he was required to learn and relearn 420 series of 16 syllables, with the number of repetitions of each series in original learning varying up to 64. The learning and relearning phases together required slightly over 15,000 recitations.

Another notable methodological contribution is a concise description of the problems that would today be called demand characteristics and experimenter bias. These problems were vexing to Ebbinghaus, for he served as experimenter, subject, and statistician. He properly treats this "secret influence of theories and opinions" as a source of error, and notes many steps he took to guard himself from such errors (pp. 28–29). For example, he sometimes concealed knowledge of the incoming results from himself as long as possible so as not to become biased about the outcome. Although he could not definitely determine how "such secret warpings of the truth" had affected his results, he notes that anyone who "is inclined *a priori to* estimate very highly the unconscious influence of secret wishes … will also have to take into consideration that the secret wish to find objective truth … may also claim a place in the complicated mechanism of these possible influences" (pp. 29–30). In one series of experiments concerned with remote associations (Chapter 9), he so worried that such biases might exist that he replicated the experiment under conditions in which he could not possibly know in what condition he was being tested, thus creating a double-blind experiment in which the subject and the experimenter were the same person.

Several other methodological innovations can be only briefly noted. First, Ebbinghaus performed what may have been the first competitive experimental test between two hypothetical conceptions of a situation (Chapter 9). Writing a few years later in *Principles of Psychology*, William James (1890) noted that "Dr. Ebbinghaus's attempt is as successful as it is original, in bringing two views, which seem at first sight inaccessible to proof, to a direct practical test, and giving the victory to one of them" (p. 677). Second, also in Chapter 9 Ebbinghaus outlines the logic of the transfer of training design, which is described later in this review in more detail. A final contribution, noted by Crowder (1976, pp. 415–416), is that Ebbinghaus was the first to adopt the crisp writing style of modern psychology, in which the essence of the problem, the method, the results, and their interpretation are stated succinctly. Anyone who has plodded through the early literature in *Psychological Review* and other American journals, where authors dwell on minutiae for pages, wishes that the style of Ebbinghaus had been adopted much sooner.

In sum, as a methodologist Ebbinghaus was years ahead of his time, and his work still stands as a paragon today. Unlike perusing other early literature, reading his experiments does not afflict one with the queasy feeling that though the ideas may be interesting, the experiments should be done over properly. Rather, in almost all cases the results are compelling. I turn now to his most important findings.

The results

Ebbinghaus devotes five chapters of the book to reporting experimental results. With one exception, the experiments were not motivated by specific theoretical concerns; rather, they were designed to determine the effect of basic variables on forgetting. Ebbinghaus had hopes "by means of keeping aloof for a while from any theory, perhaps of constructing one" (p. 65). By my count, he reports thirteen experimental results or findings in these chapters, although some are mentioned only in passing. Here I will mention briefly the results of Chapter 5 through Chapter 8 before considering in more detail those of Chapter 9.

The experiments of Chapter 5 are concerned with the basic question of how the length of a series affects the amount of work (number of trials) necessary to master it. To begin, it was necessary to determine how many syllables could be retained perfectly on a single reading. Ebbinghaus discovered the same "magical number" that Miller (1956) later uncovered: "What number of syllables can be correctly recited after only one reading? For me the number is usually seven" (p. 47). However, he then went on to discover that the number of trials necessary for a single errorless recitation increased rapidly with increasing list lengths. For series of 12 syllables—only 5 longer than the memory span—an average of 16.6 repetitions was needed. Values for series of 16, 24, and 36 syllables were 30, 44, and 55 repetitions, respectively. (The last involved fifteen minutes of repeating the series before it could be perfectly recalled.) In an interesting (and often forgotten) variation on his usual procedure, Ebbinghaus tried memorizing Byron's Don Juan in English. On the basis of this research, he estimated that learning meaningful material required only one tenth the effort needed for learning nonsense materials.

In experiments reported later (Chapter 6) Ebbinghaus asked how the number of repetitions affected forgetting. He learned lists of 16 syllables by repeating them either 8, 16, 24, 32, 42, 53, or 64 times and measured savings in relearning them the next day. Obviously, one might expect greater savings to be obtained with more repetitions, so the interesting question is the shape of the function relating repetitions to savings. Except for the extreme values where reproduction the next day required almost no relearning, Ebbinghaus found a quite regular relation: For every three extra repetitions on the first day, he saved one trial on the next in relearning the series. An interesting subsidiary observation was that relearning was just as easy when he failed to recollect consciously the list from the previous day as when he could (by virtue of numerous repetitions). Because the relation between conscious recollection and performance is of central concern today, these neglected observations arouse more interest than some of his more well-documented findings, although their interpretation is debated (see Slamecka, 1985; Tulving, 1985).

The experiments reported in Chapter 7 are the best known, as they constitute the basis for the classic forgetting curve (which, by the way, Ebbinghaus did not graph). Ebbinghaus points out that, a priori, several plausible shapes can be imagined, depending on one's theory of forgetting. The shape of the forgetting curve—with rapid losses at first, which gradually decrease—is too well known to require further description. Ebbinghaus wrote a logarithmic equation for the function and derived its parameters by the method of least squares, thus becoming (after Fechner) one of psychology's first mathematical modelers. Two other points raised in the forgetting experiment are of interest. First, internal analyses of his data seemed to indicate that sleep slowed forgetting relative to waking activity, a point confirmed about forty years later by Jenkins and Dallenbach (1924) in their classic paper. Second, Ebbinghaus also reports that his learning was less efficient from 6 to 8 P.M. than from 10 to 11 A.M., thus indicating the possible influence of circadian rhythms in remembering.

The experiments in Chapter 8 are concerned with retention as a function of repeated learning. Briefly, Ebbinghaus would learn a series of nonsense syllables of varying length to one perfect recitation and then relearn the series every day for six days. Two findings were noteworthy. First, the longer the series, the greater was the savings in relearning, on both an absolute and a relative scale. Second, the shape of the function of successive relearning approximated a geometric progression. However, a third discovery mentioned in passing seems most important to contemporary investigators. Ebbinghaus first demonstrated the spacing effect when he noted that

> 38 repetitions, distributed in a certain way over the three preceding days, has just as favorable an effect as 68 repetitions made on the day just previous … This makes the assumption probable that *with any considerable number of repetitions* a suitable distribution of them over a space of time is decidedly more advantageous than the massing of them at a single time. (p. 89)

The research in Chapter 9 is the competitive hypothesis testing of which James took note. The basic issue to be decided was whether serial learning proceeds only by the formation of associations directly between traces of adjacent items, or whether remote associations between nonadjacent items also play a role. That is, in learning a series represented by A-B-C-D-E-F and so on, are associations formed between A and C, A and D, and so forth, as well as between A and B? (The presupposition that an associative mechanism of some sort is the correct explanation is not questioned.) In order to discover if remote associations existed, Ebbinghaus devised his ingenious derived list experiments, the first use of the logic of transfer of training. In these experiments, subjects first learn serial lists of the form A-B-C-D-E-F-G and then learn other serial lists derived from the first. These derived lists vary in their relation to the original list in terms of the degree of remoteness of the associations involved. For example, a derived list in which one item is skipped from the original is A-C-E-G-B-D-F. If positive transfer occurs in learning this list relative to learning a control list, then a case is made for the existence of remote associations, albeit close ones.

In the actual experiments Ebbinghaus first learned many series of 16 syllables and then transferred his study to derived lists constructed by skipping 1, 2, 3, or 7 intervening items. The results showed that relative to a control condition for nonspecific transfer (another first), all of the derived lists showed positive transfer that fell off in a regular way with the degree of remoteness of the list derivation. This experiment indicates that "the associative threads, which hold together a remembered series, are spun not merely between each member and its immediate successor, but beyond to every member which stands to it in any close temporal relation" (p. 94).

The question of remote associations has vexed psychologists ever since, although the argument became heated only during the 1960s. Bower and Hilgard (1981), in their famous text on learning, concluded that "while Ebbinghaus's doctrine of remote associations led to much research, the ultimate judgment must be that it was incorrect and a misinterpretion of the data" (p. 137). Perhaps this is the penultimate judgment, for Slamecka (1985) and Capaldi (1985) argue strongly that remote associations must be postulated to account for certain types of data and that, further, they serve the crucial function of preparing organisms for upcoming events. Chapter 9 contains other empirical tidbits, on such topics as speed of learning backward compared to that of forward associations, but these have been overshadowed by the main experiments just described.

In sum, the corpus of Ebbinghaus's experimental results is large. Considering that he only began his research in the same year that Wundt founded his psychology lab and that he performed all experiments on himself and still produced such regular and compelling results, his achievement is nearly incredible.

Criticisms

Ebbinghaus has been subjected to criticism over the years, occasionally vociferous. Here I will touch on only a few lines. First, he employed only one subject—himself. If research during the last hundred years had proved that many (or any) of his results were unique to him, this point would merit discussion. But, in general, his findings are robust; besides, he worked within the honored psychophysical tradition in which variance is minimized by employing a single subject and tightly controlling conditions.

Second, and a more common criticism today, is that the artificiality of Ebbinghaus's experimental conditions guaranteed that nothing important or useful could be found from his research. His research and the tradition it spawned is alleged to lack external validity. Nonsense syllables, serial learning, associations, the emphasis on repetition—all seem irrelevant or worse to some cognitive psychologists today. The reasons for Ebbinghaus's choices were the same as for pioneering laboratory scientists in all fields; he hoped that by stripping common experience of numerous naturally occurring contaminants, he might so gain control over conditions as to be able to isolate and examine the operation of important causal factors. Of course, his choices of methods, variables, and theoretical emphasis can be attacked on the basis of current knowledge. Ebbinghaus was not omniscient, and many advances have since been made that his work did not foreshadow. The dominance of his methods in the field may even have slowed some discoveries. An example is the constructive nature of remembering as discussed by Bartlett (1932) and cleverly studied by many others more recently. But surely Ebbinghaus cannot be faulted for not anticipating every method, finding, or theory in the field, nor even for the scope of his techniques.[4] Nonetheless, Ebbinghaus's current reputation seems to be sullied by these imagined flaws.

If Ebbinghaus is to be justly criticized, it should probably be on his home ground, the study of serial learning. I offer one mild criticism: It seems remarkable (with the 20/20 vision of a century's hindsight) that he could memorize so many thousands of lists and never introduce in his book the concept of grouping, organizing, or chunking (Miller, 1956), which plays so large a role in modern discussions of serial learning (e.g., Bower & Winzenz, 1969; Estes, 1972; Johnson, 1970).

Evaluation

Throughout this review I have used terms such as *remarkable, astounding*, or *incredible* to refer to Ebbinghaus and his achievements. This might seem like hyperbole in discussing a man who is treated in most history books in only a few pages. Ebbinghaus founded no school of psychology; unlike, say, Titchener or Skinner, he did not attract apostles who would spread his word over the psychological universe. Also, his empirical contributions were few after 1885. He did provide observations on color vision, study brightness contrast, and devise the "Ebbinghaus Completion Test" as an early measure of intelligence. But if these last had been his only empirical contributions, he would be largely forgotten today. (As Woodworth [1909, p. 255] kindly put it in an obituary, "He was by no means prone to rush into print.") What he did in publishing *Über das Gedächtnis* was to initiate a whole field of research and, further, to provide a model of how laboratory research in psychology should be done. As already pointed out, he did this before formal laboratories were founded and before many were aware of the new statistical procedures that he used. His discoveries thus have a thoroughly modern flavor.

He promised further memory research in his book, but he never produced further systematic research, for reasons that are unknown. (A few further data on memory were reported in the *GrundzÜge der Psychologie*, 1902.) He spent his later years in pursuits that still deflect scholars from serious scientific research—university administration, journal editing (he founded a prominent journal in 1890), and textbook writing (his *GrundzÜge* went through numerous revisions). Unlike his more famous contemporaries, he did not have a "position" on all matters psychological to which he would stick no matter what the evidence showed. Perhaps this is the reason Boring (1950) could conclude that "while personally very influential, he left no deep imprint on the psychological world after he left" (p. 392).

As the foregoing makes clear, Boring's judgment seems inaccurate to me, for Ebbinghaus might be credited "with founding not a school of psychology, but instead a whole field of research. Surely memory would have been studied without Ebbinghaus, but just as surely it would have had an entirely different course of development. He set it on a firm scientific basis. Many great researchers in modern cognitive psychology have had the ability to take a simple, almost impoverished, experimental task and make it yield clever insights into the workings of mind by imaginative experimentation. Consider the item recognition paradigm devised by S. Sternberg or the uses to which Posner put his letter matching task. Hilgard (1964) argued that the same was true of Ebbinghaus's relearning and savings methods in remarking that "much good science is essentially simple … but it takes a genius to detect a simple solution to something which before him had always appeared so complex that one could but stand before the facts and marvel at them" (p. ix).

Even in his own day Ebbinghaus's contribution and his unwillingness to speculate wildly beyond the data were noted by Joseph Jacobs (1885) in one of the first reviews of Ebbinghaus's book in *Mind*. Jacobs's comments from a century ago will close my retrospective review as a reminder of what might have been had Ebbinghaus displayed the propensity of, say, J. B. Watson or B. F. Skinner for popularizing his discoveries:

> Herr Ebbinghaus is very sparing in hints as to the direction in which we may expect psychological novelties from his investigations; he is almost ostentatiously cautious in keeping close to details. His reticence tempts one into speculation as to the future of the new branch of psychometry which he has opened up. May we hope to see the day when school registers will record that such and such a lad possesses 36 British Association units of memory-power or when we shall be able to calculate how long a mind of 17 "macaulays" will take to learn Book II of *Paradise Lost*? If this be visionary, we may at least hope for much of interest and practical utility in the comparison of the varying powers of different minds which can now at last be laid down to scale. (p. 459)

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Footnotes

The book appeared in German in 1885 and was not translated into English until the 1913 edition, *Memory: A Contribution to Experimental Psychology*, was published by Teachers College, Columbia University. The translators were Henry A. Ruger and Clara E. Bussenius. In 1964 Dover reprinted the slim volume in paperback, with an introduction by Ernest R. Hilgard. All page numbers in this review refer to the 1964 Dover paperback.

Historians have generally maintained that Ebbinghaus obtained the book in Paris. However, his copy of Fechner is the English translation, so he may well have purchased it during his travels in England.

"Nonsense syllable" is a slight misnomer, for Ebbinghaus included some three-letter words among his 2,300 items.

He does note other memory phenomena in passing, even though they were not studied, for example, reminiscence (p. 62), output interference (p. 63), retroactive interference (p. 62), and a fragmentation theory of forgetting (p. 64).

Hermann Ebbinghaus received his doctorate in philosophy from the University of Bonn in 1873 and worked independently for the next seven years. It was during this time that he studied Fechner's *Elements of Psychophysics* and decided to apply scientific methods to the higher mental processes. In 1885, while at the University of Berlin, he published the monograph Über das Gedächtnis. He established a laboratory at the University of Breslau in 1884 and published his own method for testing the mental ability of school children, the "Ebbinghaus Completion Test," and the textbook GrundzÜge der Psychologie. Ebbinghaus, whose most important work was that on human memory, died in Halle, Germany, on February 26, 1909. Readers may find out more about the legacy of Ebbinghaus in the July issue of the *Journal of Experimental Psychology: Learning, Memory, and Cognition.* That issue contains, among other works, fourteen articles grouped as an "Ebbinghaus Symposium."

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