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INTERFERENCE AND FORGETTING

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I know of no one who seriously maintains that interference among tasks is of no consequence in the production of forgetting. Whether forgetting is conceptualized at a strict psychological level or at a neural level (e.g., neural memory trace), some provision is made for interference to account for at least some of the measured forgetting. The many studies on retroactive inhibition are probably responsible for this general agreement that interference among tasks must produce a sizable proportion of forgetting. By introducing an interpolated interfering task very marked decrements in recall can be produced in a few minutes in the laboratory. But there is a second generalization which has resulted from these studies, namely, that most forgetting must be a function of the learning of tasks which interfere with that which has already been learned (19). Thus, if a single task is learned in the laboratory and retention measured after a week, the loss has been attributed to the interference from activities learned outside the laboratory during the week. It is this generalization with which I am concerned in the initial portions of this paper.

Now, I cannot deny the data which show large amounts of forgetting produced by an interpolated list in a few minutes in the laboratory. Nor do I deny that this loss may be attributed to interference. But I will try to show that use of retroactive inhibition as a paradigm of forgetting (via interference) may be seriously questioned. To be more specific: if a subject learns a single task, such as a list of words, and retention of this task is measured after a day, a week, or a month, I will try to show that very little of the forgetting can be attributed to an interfering task learned outside the laboratory during the retention interval. Before pursuing this further, I must make some general comments by way of preparation.

Whether we like it or not, the experimental study of forgetting has been largely dominated by the Ebbinghaus tradition, both in terms of methods and materials used. I do not think this is due to sheer perversity on the part of several generations of scientists interested in forgetting. It may be noted that much of our elementary knowledge can be obtained only by rote learning. To work with rote learning does not mean that we are thereby not concerning ourselves with phenomena that have no counterparts outside the laboratory. Furthermore, the investigation of these phenomena can be handled by methods which are acceptable to a science. As is well known, there are periodic verbal revolts against the Ebbinghaus tradition (e.g., 2, 15, 22). But for some reason nothing much ever happens in the laboratory as a consequence of these revolts. I mention these matters neither by way of apology nor of justification for having done some research in rote learning, but for two other reasons. First, it may very well be true, as some have suggested (e.g., 22), that studies of memory in the Ebbinghaus tradition are not getting at all of the important

[1] Address of the president, Midwestern Psychological Association, St. Louis, Missouri, May, 1956.

[2] Most of the data from my own research referred to in this paper were obtained from work done under Contract N7 onr-45008, Project NR 154-057, between Northwestern University and The Office of Naval Research.

phenomena of memory. I think the same statement—that research has not got at all of the important processes—could be made about all areas in psychology; so that the criticism (even if just) should not be indigenous to the study of memory. Science does not deal at will with all natural events. Science deals with natural events only when ingenuity in developing methods and techniques of measurement allow these events to be brought within the scope of science. If, therefore, the studies of memory which meet scientific acceptability do not tap all-important memorial processes, all I can say is that this is the state of the science in the area at the moment. Secondly, because the bulk of the systematic data on forgetting has been obtained on rote-learned tasks, I must of necessity use such data in discussing interference and forgetting.

Returning to the experimental situation, let me again put in concrete form the problem with which I first wish to deal. A subject learns a single task, such as a list of syllables, nouns, or adjectives. After an interval of time, say, 24 hours, his retention of this list is measured. The explanatory problem is what is responsible for the forgetting which commonly occurs over the 24 hours. As indicated earlier, the studies of retroactive inhibition led to the theoretical generalization that this forgetting was due largely to interference from other tasks learned during the 24-hour retention interval. McGeoch (20) came to this conclusion, his last such statement being made in 1942. I would, therefore, like to look at the data which were available to McGeoch and others interested in this matter. I must repeat that the kind of data with which I am concerned is the retention of a list without formal interpolated learning introduced. The interval of retention with which I am going to deal in this, and several subsequent analyses, is 24 hours.

First, of course, Ebbinghaus' data were available and in a sense served as the reference point for many subsequent investigations. In terms of percentage saved in relearning, Ebbinghaus showed about 65 per cent loss over 24 hours (7). In terms of recall after 24 hours, the following studies are representative of the amount forgotten: Youtz, 88 per cent loss (37); Luh, 82 per cent (18); Krueger, 74 per cent (16); Hovland, 78 per cent (11); Cheng, 65 per cent and 84 per cent (6); Lester, 65 per cent (17). Let us assume as a rough average of these studies that 75 per cent forgetting was measured over 24 hours. In all of these studies the list was learned to one perfect trial. The percentage values were derived by dividing the total number of items in the list into the number lost and changing to a percentage. Thus, on the average in these studies, if the subject learned a 12-item list and recalled three of these items after 24 hours, nine items (75 per cent) were forgotten.

The theory of interference as advanced by McGeoch, and so far as I know never seriously challenged, was that during the 24-hour interval subjects learned something outside the laboratory which interfered with the list learned in the laboratory. Most of the materials involved in the investigations cited above were nonsense syllables, and the subjects were college students. While realizing that I am viewing these results in the light of data which McGeoch and others did not have available, it seems to me to be an incredible stretch of an interference hypothesis to hold that this 75 per cent forgetting was caused by something which the subjects learned outside the laboratory during the 24-hour interval. Even if we agree with some educators that much of what we teach our students in college is nonsense, it does not seem to be the kind of learning that would interfere with nonsense syllables.

If, however, this forgetting was not due to interference from tasks learned outside the laboratory during the retention interval, to what was it due? I shall try to show that most of this forgetting was indeed produced by interference—not from tasks learned outside the laboratory, but from tasks learned previously in the laboratory. Following this I will show that when interference from laboratory tasks is removed, the amount of forgetting which occurs is relatively quite small. It then becomes more plausible that this amount could be produced by interference from tasks learned outside the laboratory, although, as I shall also point out, the interference very likely comes from prior, not interpolated, learning.

In 1950 a study was published by Mrs. Greenberg and myself (10) on retention as a function of stage of practice. The orientation for this study was crassly empirical; we simply wanted to know if subjects learn how to recall in the same sense that they learn how to learn. In the conditions with which I am concerned, naive subjects learned a list of ten paired adjectives to a criterion of eight out of ten correct on a single trial. Forty-eight hours later this list was recalled. On the following day, these same subjects learned a new list to the same criterion and recalled it after 48 hours. This continued for two additional lists, so that the subjects had learned and recalled four lists, but the learning and recall of each list was complete before another list was learned. There was low similarity among these lists as far as conventional symptoms of similarity are concerned. No words were repeated and no obvious similarities existed, except for the fact that they were all adjectives and a certain amount of similarity among prefixes, suffixes, and so on must inevitably occur. The

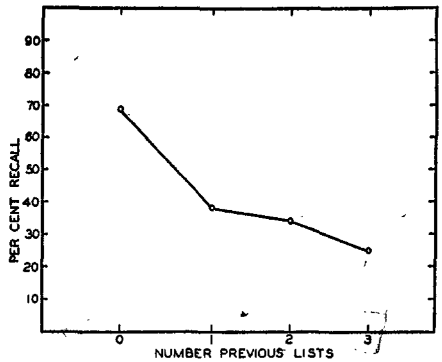

FIG. 1. Recall of paired adjectives as a function of number of previous lists learned (10).

recall of these four successive lists is shown in Fig. 1.

As can be seen, the more lists that are learned, the poorer the recall, from 69 per cent recall of the first list to 25 per cent recall of the fourth list. In examining errors at recall, we found a sufficient number of intrusion responses from previous lists to lead us to suggest that the increasing decrements in recall were a function of proactive interference from previous lists. And, while we pointed out that these results had implications for the design of experiments on retention, the relevance to an interference theory of forgetting was not mentioned.

Dr. E. J. Archer has made available to me certain data from an experiment which still is in progress and which deals with this issue. Subjects learned lists of 12 serial adjectives to one perfect trial and recalled them after 24 hours. The recall of a list always took place prior to learning the next list. The results for nine successive lists are shown in Fig. 2. Let me say again that there is no laboratory activity during the 24-hour interval; the subject learns a list, is dismissed from the laboratory, and returns after 24 hours to recall the list. The percentage of recall falls from 71 per cent for the first list to 27 per cent for the ninth.

In summarizing the more classical data on retention above, I indicated that a rough estimate showed that after 24 hours 75 per cent forgetting took place, or recall was about 25 per cent correct. In viewing these values in the light of Greenberg's and Archer's findings, the conclusion seemed inescapable that the classical studies must have been dealing with subjects who had learned many lists. That is to say, the subjects must have served in many conditions by use of counterbalancing and repeated cycles. To check on this I have made a search of the literature on the studies of retention to see if systematic data could be compiled on this matter. Preliminary work led me to establish certain criteria for inclusion in the summary to be presented. First, because degree of learning is such an important variable, I have included only those studies in which degree of learning was one perfect recitation of the list. Second, I have included only studies in which retention was measured after 24 hours. Third, I have included only studies in which recall measures were given. (Relearning measures add complexities with which I do not wish to deal in this paper.) Fourth, the summary includes only material learned by relatively massed practice. Finally, if an investigator had two or more conditions which met these criteria, I averaged the values presentation

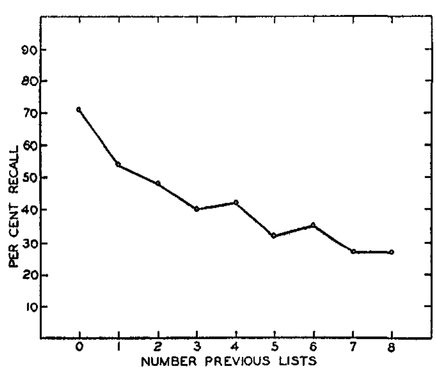

FIG. 2. Recall of serial adjective lists as a function of number of previous lists learned. Unpublished data, courtesy of Dr. E. J. Archer.

in this paper. Except for these restrictions, I have used all studies I found (with an exception to be noted later), although I do not pretend to have made an exhaustive search. From each of these studies I got two facts: first, the percentage recalled after 24 hours, and second, the average number of previous lists the subjects had learned before learning the list on which recall after 24 hours was taken. Thus, if a subject had served in five experimental conditions via counterbalancing, and had been given two practice lists, the average number of lists learned before learning the list for which I tabulated the recall was four. This does not take into account any previous experiments in rote learning in which the subject might have served.

For each of these studies the two facts, average number of previous lists learned and percentage of recall, are related as in Fig. 3. For example, consider the study by Youtz. This study was concerned with Jost's law, and had several degrees of learning, several lengths of retention interval, and the subjects served in two cycles. Actually, there were 15 experimental conditions and each subject was given each condition twice. Also, each subject learned six practice lists before starting the experimental conditions. Among the 15 conditions was one in which the learning of the syllables was carried to one perfect recitation and recall was taken after 24 hours. It is this particular condition in which I am interested. On the average, this condition would have been given at the time when the subject had learned six practice lists and 15 experimental lists, for a total of 21 previous lists.

The studies included in Fig. 3 have several different kinds of materials, from geometric forms to nonsense syllables to nouns; they include both paired-associate and serial presentation, with differ-

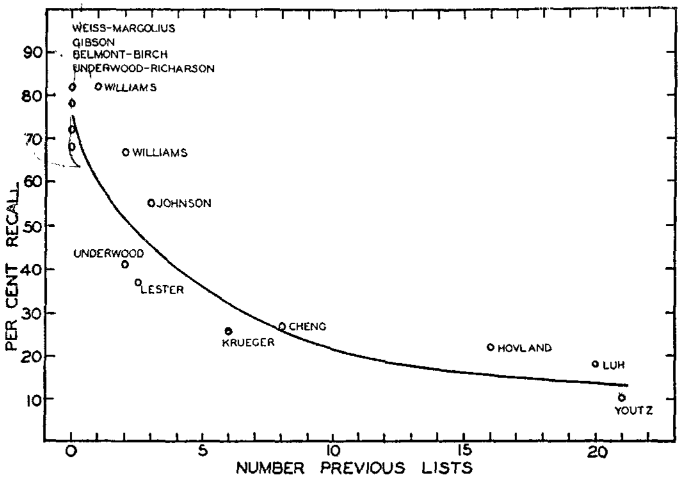

FIG. 3. Recall as a function of number of previous lists learned as determined from a number of studies. From left to right: Weiss and Margolius (35), Gibson (9), Belmont and Birch (3), Underwood and Richardson (33), Williams (36), Underwood (27, 28, 29, 30), Lester (17), Johnson (14), Krueger (16), Cheng (6), Hovland (11), Luh (18), Youtz (37).

ent speeds of presentation and different lengths of lists. But I think the general relationship is clear. The greater the number of previous lists learned the greater the forgetting. I interpret this to mean that the greater the number of previous lists the greater the *proactive* interference. We know this to be true (26) for a formal proactive-inhibition paradigm; it seems a reasonable interpretation for the data of Fig. 3. That there are minor sources of variance still involved I do not deny. Some of the variation can be rationalized, but that is not the purpose of this report. The point I wish to make is the obvious one of the relationship between number of previous lists learned—lists which presumably had no intentionally built-in similarity—and amount of forgetting. If you like to think in correlational terms, the rank-order correlation between the two variables is − .91 for the 14 points of Fig. 3.

It may be of interest to the historian that, of the studies published before 1942 which met the criteria I imposed, I did not find a single one in which subjects had not been given at least one practice task before starting experimental conditions, and in most cases the subjects had several practice lists and several experimental conditions. Gibson's study (1942) was the first I found in which subjects served in only one condition and were not given practice tasks. I think it is apparent that the design proclivities of the 1920s and 1930s have been largely responsible for the exaggerated picture we have had of the rate of forgetting of rote-learned materials. On the basis of studies performed during the 1920s and 1930s, I have given a rough estimate of forgetting as being 75 per cent over 24 hours, recall being 25 per cent. On the basis of modern studies in which the subject has learned no previous lists—where there is no proactive inhibition from previous laboratory tasks—a rough estimate would be that forgetting is 25 per cent; recall is 75 per cent. The values are reversed. (If in the above and subsequent discussion my use of percentage values as if I were dealing with a cardinal or extensive scale is disturbing, I will say only that it makes the picture easier to grasp, and in my opinion no critical distortion results.)

Before taking the next major step, I would like to point out a few other observations which serve to support my general point that proactive inhibition from laboratory tasks has been the major cause of forgetting in the more classical studies. The first illustration I shall give exemplifies the point that when subjects have served in several conditions, forgetting after relatively short periods of time is greater than after 24 hours if the subject has served in only one condition. In the Youtz study to which I have already referred, other conditions were employed in which recall was taken after short intervals. After 20 minutes recall was 74 per cent, about what it is after 24 hours if the subject has not served in a series of conditions. After two hours recall was 32 per cent. In Ward's (34) well-known reminiscence experiment, subjects who on the average had learned ten previous lists showed a recall of only 64 per cent after 20 minutes.

In the famous Jenkins-Dallenbach (13) study on retention following sleep and following waking, two subjects were used. One subject learned a total of 61 lists and the other 62 in addition to several practice lists. Roughly, then, if the order of the conditions was randomized, approximately 30 lists had been learned prior to the learning of a list for a given experimental condition. Recall after eight waking hours for one subject was 4 per cent and for the other 14 per cent. Even after sleeping for eight hours the recall was only 55 per cent and 58 per cent.

I have said that an interpolated list can produce severe forgetting. However, in one study (1), using the A-B, A-C paradigm for original and interpolated learning, but using subjects who had never served in any previous conditions, recall of the original list was 46 per cent after 48 hours, and in another comparable study (24), 42 per cent. Thus, the loss is not nearly as great as in the classical studies I have cited where there was no interpolated learning in the laboratory.

My conclusion at this point is that, in terms of the gross analysis I have made, the amount of forgetting which might be attributed to interference from tasks learned outside the laboratory has been "reduced" from 75 per cent to about 25 per cent. I shall proceed in the next section to see if we have grounds for reducing this estimate still more. In passing on to this section, however, let me say that the study of factors which influence proactive inhibition in these counterbalanced studies is a perfectly legitimate and important area of study. I mention this because in the subsequent discussion I am going to deal only with the case where a subject has learned a single list in the laboratory, and I do not want to leave the impression that we should now and forevermore drop the study of interference produced by previous laboratory tasks. Indeed, as will be seen shortly, it is my opinion that we should increase these studies for the simple reason that the proactive paradigm provides a more realistic one than does the retroactive paradigm.

When the subject learns and recalls a single list in the laboratory, I have given an estimate of 25 per cent as being the amount forgotten over 24 hours. When, as shown above, we calculate percentage forgotten of lists learned to one perfect trial, the assumption is that had the subjects been given an immediate recall trial, the list would have been perfectly recalled. This, of course, is simply not true. The major factor determining how much error is introduced by this criterion-percentage method is probably the difficulty of the task. In general, the overestimation of forgetting by the percentage method will be directly related to the difficulty of the task. Thus, the more slowly the learning approaches a given criterion, the greater the drop on the trial immediately after the criterion trial. Data from a study by Runquist (24), using eight paired adjectives (a comparatively easy task), shows that amount of forgetting is overestimated by about 10 per cent. In a study (32) using very difficult consonant syllables, the overestimation was approximately 20 per cent. To be conservative, assume that on the average the percentage method of reporting recall overestimates the amount forgotten by 10 per cent. If we subtract this from the 25 per cent assumed above, the forgetting is now re-estimated as being 15 per cent over 24 hours. That is to say, an interference theory, or any other form of theory, has to account for a very small amount of forgetting as compared with the amount traditionally cited.

What are the implications of so greatly "reducing" the amount of forgetting? There are at least three implications which I feel are worth pointing out. First, if one wishes to hold to an interference theory of forgetting (as I do), it seems plausible to assert that this amount of forgetting could be produced from learning which has taken place outside of the laboratory. Furthermore, it seems likely that such interference must result primarily from proactive interference. This seems likely on a simple probability basis. A 20-year-old college student will more likely have learned something during his 20 years prior to coming to the laboratory that will interfere with his retention than he will during the 24 hours between the learning and retention test. However, the longer the retention interval the more important will retroactive interference become relative to proactive interferences.

The second implication is that these data may suggest greater homogeneity or continuity in memorial processes than hitherto supposed. Although no one has adequately solved the measurement problem of how to make comparisons of retention among conditioned responses, prose material, motor tasks, concept learning, and rote-learned tasks, the gross comparisons have indicated that rote-learned tasks were forgotten much more rapidly than these other tasks. But the rote-learning data used for comparison have been those derived with the classical design in which the forgetting over 24 hours is approximately 75 per cent. If we take the revised estimate of 15 per cent, the discrepancies among tasks become considerably less.

The third implication of the revised estimate of rate of forgetting is that the number of variables which appreciably influence rate of forgetting must be sharply limited. While this statement does not inevitably follow from the analyses I have made, the current evidence strongly supports the statement. I want to turn to the final section of this paper which will consist of a review of the influence of some of the variables which are or have been thought to be related to rate of forgetting. In considering these variables, it is well to keep in mind that a variable which produces only a small difference in forgetting is important if one is interested in accounting for the 15 per cent assumed now as the loss over 24 hours. If appropriate for a given variable, I will indicate where it fits into an interference theory, although in no case will I endeavor to handle the details of such a theory.

Time. Passage of time between learning and recall is the critical defining variable for forgetting. Manipulation of this variable provides the basic data for which a theory must account. Previously, our conception of rate of forgetting as a function of time has been tied to the Ebbinghaus curve. If the analysis made earlier is correct, this curve does not give us the basic data we need. In short, we must start all over and derive a retention curve over time when the subjects have learned no previous materials in the laboratory. It is apparent that I expect the fall in this curve over time to be relatively small.

In conjunction with time as an independent variable, we must, in explanations of forgetting, consider why sleep retards the processes responsible for forgetting. My conception, which does not really explain anything, is that since forgetting is largely produced by proactive interference, the amount of time which a subject spends in sleep is simply to be subtracted from the total retention interval when predicting the amount to be forgotten. It is known that proactive interference increases with passage of time (5); sleep, I believe, brings to a standstill whatever these processes are which produce this increase.

Degree of learning. We usually say that the better or stronger the learning the more or better the retention. Yet, we do not know whether or not the *rate* of forgetting differs for items of different strength. The experimental problem is a difficult one. What we need is to have a subject learn a single association and measure its decline in strength over time. But this is difficult to carry out with verbal material, since almost of necessity we must have the subject learn a series of associations, to make it a reasonable task. And, when a series of associations are learned, complications arise from interaction effects among associations of different strength. Nevertheless, we may expect, on the basis of evidence from a wide variety of studies, that given a constant degree of similarity, the effective interference varies as some function of the strength of associations.

Distribution of practice. It is a fact that distribution of practice during acquisition influences retention of verbal materials. The facts of the case seem to be as follows. If the subject has not learned previous lists in the laboratory, massed practice gives equal or better retention than does distributed practice. If, on the other hand, the subject has learned a number of previous lists, distributed practice will facilitate retention (32). We do not have the theoretical solution to these facts. The point I wish to make here is that whether or not distribution of learning inhibits or facilitates retention depends upon the amount of interference from previous learning. It is reasonable to expect, therefore, that the solution to the problem will come via principles handling interference in general. I might also say that a theoretical solution to this problem will also provide a solution for Jost's laws.

Similarity. Amount of interference from other tasks is closely tied to similarity. This similarity must be conceived of as similarity among materials as such and also situational similarity (4). When we turn to similarity within a task, the situation is not quite so clear. Empirically and theoretically (8) one would expect that intratask similarity would be a very relevant variable in forgetting. As discussed elsewhere (31), however, variation in intratask similarity almost inevitably leads to variations in intertask similarity. We do know from a recent study (33) that with material of low meaningfulness forgetting is significantly greater with high intralist similarity than with low. While the difference in magnitude is only about 8 per cent, when we are trying to account for a total loss of 15 per cent, this amount becomes a major matter.

Meaningfulness. The belief has long

been held that the more meaningful the material the better the retention—the less the forgetting. Osgood (21) has pointed out that if this is true it is difficult for an interference theory to handle. So far as I know, the only direct test of the influence of this variable is a recent study in which retention of syllables of 100 per cent association value was compared with that of zero association value (33). There was no difference in the recall of these syllables. Other less precise evidence would support this finding when comparisons are made among syllables, adjectives, and nouns, as plotted in Fig. 3. However, there is some evidence that materials of very low meaningfulness are forgotten more rapidly than nonsense syllables of zero association value. Consonant syllables, both serial (32) and paired associates (unpublished), show about 50 per cent loss over 24 hours. The study using serial lists was the one mentioned earlier as knowingly omitted from Fig. 3. These syllables, being extremely difficult to learn, allow a correction of about 20 per cent due to criterion overestimation, but even with this much correction the forgetting (30 per cent) is still appreciably more than the estimate we have made for other materials. To invoke the interference theory to account for this discrepancy means that we must demonstrate how interference from other activities could be greater for these consonant syllables than for nonsense syllables, nouns, adjectives, and other materials. Our best guess at the present time is that the sequences of letters in consonant syllables are contrary to other well-established language habits. That is to say, letter sequences which commonly occur in our language are largely different from those in consonant syllables. As a consequence, not only are these consonant syllables very difficult to learn, but forgetting is accelerated by proactive interference from previously well-learned letter sequences. If subsequent research cannot demonstrate such a source of interference, or if some other source is not specified, an interference theory for this case will be in some trouble.

Affectivity. Another task dimension which has received extensive attention is the affective tone of the material. I would also include here the studies attaching unpleasant experiences to some items experimentally and not to others, and measuring retention of these two sets of items. Freud is to a large extent responsible for these studies, but he cannot be held responsible for the malformed methodology which characterizes so many of them. What can one say by way of summarizing these studies? The only conclusion that I can reach is a statistical one, namely, that the occasional positive result found among the scores of studies is about as frequent as one would expect by sampling error, using the 5 per cent level of confidence. Until a reliable body of facts is established for this variable and associated variables, no theoretical evaluation is possible.

Other variables. As I indicated earlier, I will not make an exhaustive survey of the variables which may influence rate of forgetting. I have limited myself to variables which have been rather extensively investigated, which have immediate relevance to the interference theory, or for which reliable relationships are available. Nevertheless, I would like to mention briefly some of these other variables. There is the matter of *warm-up* before recall; some investigators find that this reduces forgetting (12); others, under as nearly replicated conditions as is possible to obtain, do not (23). Some resolution must be found for these flat contradictions. It seems perfectly reasonable, however, that inadequate set or context differences could reduce recall. Indeed, an interference theory would predict this forgetting if the set or context stimuli are appreciably different from those prevailing at the time of learning. In our laboratory we try to reinstate the learning set by careful instructions, and we simply do not find decrements that might be attributed to inadequate set. For example, in a recent study (33) subjects were given a 24-hour recall of a serial list after learning to one perfect trial. I think we would expect that the first item in the list would suffer the greatest decrement due to inadequate set, yet this item showed only .7 per cent loss. But let it be clear that when we are attempting to account for the 15 per cent loss over 24 hours, we should not overlook any possible source for this loss.

Thus far I have not said anything about forgetting as a function of characteristics of the subject, that is, the personality or intellectual characteristics. As far as I have been able to determine, there is not a single valid study which shows that such variables have an appreciable influence on forgetting. Many studies have shown differences in learning as a function of these variables, but not differences in rate of forgetting. Surely there must be some such variables. We do know that if subjects are severely insulted, made to feel stupid, or generally led to believe that they have no justification for continued existence on the earth just before they are asked to recall, they will show losses (e.g., 25, 38), but even the influence of this kind of psychological beating is short lived. Somehow I have never felt that such findings need explanation by a theory used to explain the other facts of forgetting.

Concerning the causes of forgetting, let me sum up in a somewhat more dogmatic fashion than is probably justified. One of the assumptions of science is finite causality. Everything cannot influence everything else. To me, the most important implication of the work on forgetting during the last ten years is that this work has markedly *reduced* the number of variables related to forgetting. Correspondingly, I think the theoretical problem has become simpler. It is my belief that we can narrow down the cause of forgetting to interference from previously learned habits, from habits being currently learned, and from habits we have yet to learn. The amount of this interference is primarily a function of similarity and associative strength, the latter being important because it interacts with similarity.

SUMMARY

This paper deals with issues in the forgetting of rote-learned materials. An analysis of the current evidence suggests that the classical Ebbinghaus curve of forgetting is primarily a function of interference from materials learned previously in the laboratory. When this source of interference is removed, forgetting decreases from about 75 per cent over 24 hours to about 25 per cent. This latter figure can be reduced by at least 10 per cent by other methodological considerations, leaving 15 per cent as an estimate of the forgetting over 24 hours. This estimate will vary somewhat as a function of intratask similarity, distributed practice, and with very low meaningful material. But the overall evidence suggests that similarity with other material and situational similarity are by far the most critical factors in forgetting. Such evidence is consonant with a general interference theory, although the details of such a theory were not presented here.

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