2 Memory Research: What Kind of Progress?

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INTRODUCTION

Understanding the human mind is an arduous task, rendered especially frustrating because of our intimate familiarity with what we are trying to understand. From personal experience, we know exceedingly well what the mind can do, the broad range of its fantastic capabilities; we are also very much aware of its limitations. Yet its understanding—why and how does it do what it does—has so far eluded us. Countless scholars, sages, and scientists have devoted their lifetimes to the study of the mind in its many manifestations, and they have written countless books and articles about it; but the fruits of their labors have been somewhat less than spectacular. Psychological study of memory, by and large, has shared a similar fate. After over two thousand years of rational speculation and a hundred years of experimental study, we can point to few achievements that promise to be of relatively permanent value. The question may be asked, therefore, as to what kind of progress we have made in understanding memory.

The present essay is an attempt to briefly survey and evaluate the accomplishments of the psychological science of memory, to assess some of its current practices, and to point to some of the sources of its metatheoretical problems. This account is necessarily selective, quite personal, and undoubtedly biased. Its purpose is simply to state an opinion with which others can agree or that they can criticize or reject. The discipline might benefit from the ensuing discussion of some of the more global concerns and strategic issues.

SOME GLIMPSES INTO THE PAST

We begin by taking a few random glances at the vast history of man's observation and thought about memory. What we see can serve as a backdrop against which we can place our more recent activity and then decide how much change there has been and how much progress.

More than two thousand years ago, Plato had an image of the mind as an aviary full of all sorts of birds, "both solitary and in groups, flying anywhere and everywhere." The aviary is empty when a person is young, but the process of learning captures the birds of knowledge and detains them in the enclosure. Today we still find it convenient, and sometimes indispensable, to think of the mind and memory in spatial terms: Memories are retained in and retrieved from the memory store if the search through it is successful.

Zedler's *Grosses Lexicon* (1732–1750) was a monumental compendium of human knowledge published in the first half of the 18th century. In it memory is defined as a special power of human reason to take up and retain ideas. A further distinction is made between memory proper—whose function is the retention of ideas—and remembering—which refers to the reproduction of the retained ideas. The article on memory contains a wealth of information. It includes, for instance, sections about the relation between ease of learning and retention (couched in terms of individual differences), constancy of memory ability throughout life, and conditions contributing to the impairment of memory. On this latter topic, we are told that one cannot meditate or learn if one has a cold or has been drunk the day before. Moreover, we are advised that in general, people who enjoy too much brandy, or smoke or snuff too much tobacco, or partake too much of the "pleasures of Venus" will soon find their memories seriously weakened, because these practices lead to the indolence of the whole body and this also impairs the "inner senses." The *Grosses Lexicon*, however, also tells its readers what to do when their memories are impaired. They are to prepare a balsam containing a number of ingredients such as castoreum, rosemary, lavender, and cuttings of fingernails, dissolved in benzoic acid, and smear it on their foreheads every morning and night. This balsam brings back lost memory and makes it stronger.

We can chuckle about some of the things well-meaning scholars wrote about memory two and a half centuries ago, but we also see that the intellectual distance separating them from us is not overwhelming. Many things of concern to students of memory then are still perfectly sensible by today's standards. For instance, the distinction between storage and retrieval is there, the problem of the relation between speed of learning and ease of retention is of current interest, and the relation between alcohol and memory is an up-to-date topic of investigation (e.g., Birnbaum & Parker, 1977). Only the relation between memory and the "pleasures of Venus" seems to

have been neglected in more recent times, and interest in memory ointments has been replaced by research into both facilitating (e.g., Dawson & McGaugh, 1973) and inhibiting (e.g., Barraco & Stettner, 1976) effects of drugs on learning and retention.

As everyone knows, Ebbinghaus did his ground-breaking, quantitative research on learning and forgetting of series of nonsense syllables about a hundred years ago, setting the tone for psychological research on memory for a long time to come. William James' insightful observations about association and memory, made at about the same time, represented the culmination of the quest for understanding of human mental processes through the method of introspection. In the shadow of Ebbinghaus' achievements, these ideas had little impact on the thinking of the developing breed of experimental psychologists. Although Ebbinghaus' influence was diluted by the interesting work soon to be done and reported on the basic processes of learning by Pavlov in Russia and Thorndike in the United States, it took a while for the burgeoning field of learning and behavior theory to put an unmistakable stamp on the study of memory. Research on memory 50 years ago was still a part of the tradition created by Ebbinghaus.

A typical annual volume of the *Journal of Experimental Psychology* at that time contained approximately 40 articles, of which perhaps four were concerned with human learning and memory. In the 1926 volume, for instance, there were two papers by Cason, one concerning backward associations and the other on remote forward associations; one article by Williams on the phenomenon of reminiscence; and one by Pan on the influence of context upon learning and recall. The 1927 volume of the *American Journal of Psychology* contained 36 research articles—one of which was on verbal learning, by Dallenbach and Jenkins on the effect of serial position upon recall. The *British Journal of Psychology* for the same year contained a total of 27 research articles, none of them on memory or verbal learning. Judging by this rather limited sample, we see that the total output of experimental psychologists fifty years ago was rather modest by today's standards and that verbal learning and memory constituted an equally modest component of the total activity. Yet the topics covered were not greatly different from those of interest to students of memory today. The study of context effects is currently a "hot" topic, reminiscence continues to interest researchers (e.g., Buschke, 1974; Erdelyi, Buschke, & Finkelstein, 1977; Madigan, 1976), and phenomena involving serial position effects figure prominently in textbooks (e.g., Crowder, 1976; Murdock, 1974). Only problems entailed in remote backward and forward associations are of less interest today, and Cason's work is unknown. His conclusions did not seem to fit into that Zeitgeist; he used prose passages as learning materials and permitted his subjects to learn these passages under conditions that were similar to the learning situations of everyday life. McGeoch and Irion (1952),

in the definitive textbook of the time, mentioned Cason's work only in a footnote, saying that they had omitted it from the general discussion of intraserial phenomena, "because the methods employed seemed to the writers to make [the] results indeterminate [p. 93]."

By the beginning of the 1950s, there was almost no work on memory reported in the literature of experimental psychology, although journals contained a fair proportion of articles on verbal learning. For instance, the *Journal of Experimental Psychology* in 1951 contained a total of 123 articles, of which one was on memory—for pleasant and unpleasant experiences—and 17 on topics that could be classified as verbal learning. Problems investigated were greatly influenced by the general learning theory: discrimination, reinforcement, delay of reward, negative incentives, work decrement, warm-up effects, and the like, as well as transfer, retroactive inhibition, and the effects of intralist similarity in paired-associate and serial learning.

We get another glimpse of the world of memory as it existed twenty-five years ago from the influential monograph by McGeoch and Irion (1952) that provided a review of the whole field. Among other things, McGeoch and Irion identified seven "persistent theoretical problems" and discussed each of these briefly. These were problems concerning the existence of general laws of learning, what is associated when learning occurs, the use of intervening variables, the role of contiguity and reinforcement in learning, the status of the principle of frequency, decremental factors in learning (inhibition and experimental extinction), and the relation between special and general theories of learning. On the question of general laws of learning, the authors offered their belief that a single set of principles of learning "will suffice at the present time," although they recognized that the factors of language and meaning "complicate the study of learning in many ways," making necessary many special, complex concepts. They also expressed the hope that eventually these complex concepts might be derivable from simpler ones. They did not offer any suggestions as to the solution of the problem of what is associated in learning, but they did point out that this problem—probably not solvable by experimental data—essentially reflected the attitude of various groups of theorists that "our words are better than your words" (McGeoch & Irion, 1952, p. 44). On the question of whether contiguity alone is sufficient for producing learning (as against the idea that reinforcement is also necessary), McGeoch and Irion thought that no crucial evidence existed either for or against the hypothesis. The issue of the role of frequency was seen as taking two forms: (1) whether frequency (repetition of the learning situation) is necessary for learning to occur; and (2) exactly what it is that has to be repeated in the learning situation. As with the other persistent theoretical problems, this one, too, appeared to be wide open. Under decremental factors of learning, McGeoch and Irion talked about forgetting and suggested that it is a function of three main factors: (1) altered stimulus

context; (2) altered set to perform; and (3) retroactive inhibition. They also made a firm statement against disuse as an explanation of forgetting.

GROWTH IN MEMORY RESEARCH

The few thin slices through the long history we have just seen provide the impression of a treelike growth: From a single, central idea or two the field has branched out in many divergent directions. Over the last couple of decades, this branching-out process has become even more conspicuous and has greatly accelerated. Indeed, perhaps the most compelling impression one gets of the field of memory research today is one of rapid growth and tremendous change. The question of interest to us in this essay concerns the nature of this growth and change, especially as we know that change is a necessary but not a sufficient condition for progress. Five specific ways in which memory research has changed are readily apparent.

First, the sheer volume of research is very much greater than it has ever been before. It is probably not an exaggeration to say that the total amount of experimental and theoretical effort devoted to human memory today exceeds the effort expended in the whole field of experimental psychology only twenty-five years ago. There has been a virtual explosion in the variety of methods, procedures, techniques, and materials that have been used in memory experiments, as well as a massive expansion of established experimental facts and phenomena. Although the significance of many of these facts may not always be clear, the enrichment of our data base most likely will enhance the probability that facts with profound theoretical implications could be identified. Moreover, since it could well be argued that a certain critical mass of active and productive researchers is necessary in any field before much progress can be expected, it may well be that we have either achieved or are approaching this critical mass now.

The second way in which the current scene in memory research differs from the past lies in the existence of more comprehensive theories not only of memory (e.g., Atkinson & Shiffrin, 1968) but also of even larger domains of the human mind (e.g., Anderson, 1976). These theories attempt to integrate many different phenomena of memory, as well as other manifestations of the human mind, and they do so in a rather detailed and elaborate fashion. They thus differ considerably from both the earlier "special theories" described by McGeoch and Irion (1952), as well as from the more general sets of theoretical ideas that are more appropriately thought of as approaches, frameworks, or orientations. Although many workers agree that any finite set of experimental facts can always be accommodated by many different general theories or specific models and that unique specification of the structure and processes underlying mental activity and attendant behavior is therefore not possible (Anderson, 1976), the mere fact that some comprehensive theories have been

constructed testifies to both increasing confidence and increasing vigor of theoretical thought.

Third, we have witnessed what could be considered a rather welcome expansion in basic approaches and orientations to the study of memory. In addition to the classical associationistic approach, we now can identify the organizational school of thought, the information-processing view, and levels-of-processing ideas, as well as various combinations of these. Associationism in recent years has assumed a rather different and a very much more sophisticated form than the one that was known, say, to McGeoch and Irion (e.g., Anderson & Bower, 1973). In the classical literature, the associative approach was adopted in the study of acquisition, transfer, and interference of simple associations, whereas its modern extensions encompass many other phenomena, including knowledge and retrieval of general facts and understanding of language. The organizational approach to memory (e.g., Mandler, 1967) can be regarded as an offshoot of classical associationism (Postman, 1972), but it has encouraged students of memory to raise issues and questions that had been neglected before.

The information-processing view of memory had its origin in the analogy between human beings and digital computers as information-processing systems (e.g., Broadbent, 1958; Norman, 1970). The metaphor of a human being as an information processor spawned the theoretical distinction between short-term and long-term memory, permitted the conceptualization of memory in new terms such as coding, storage, and retrieval, and encouraged the study of memory in experimental paradigms (e.g., recognition) that had not fitted readily into either the associationistic or organizational orientation.

The levels-of-processing ideas (Craik & Lockhart, 1972) have provided an important alternative to both associationistic and information-processing views by: (1) emphasizing the continuity between perception and memory; (2) suggesting and demonstrating the tremendous importance of the manner of studying to-be-remembered material for its subsequent retrievability; and (3) providing a powerful analytical device for dissecting the hitherto inscrutable processes underlying learning. The levels-of-processing approach also turned out to be a natural vehicle for further theoretical utilization of the important concept of memory units consisting of collections of features or attributes (Bower, 1967; Underwood, 1969; Wickens, 1970).

The fourth kind of change has to do with the development of the attempts to devise different taxonomies of memory. As recently as 1952, McGeoch and Irion represented the point of view that one and the same set of basic principles and laws of learning would apply in all learning situations and perhaps even for all organisms capable of learning. Earlier students of memory also did not differentiate between different kinds of memory and at

least implicitly adopted the view of a unitary memory. We now have several basic distinctions that many students of memory find useful. One such is the distinction between primary and secondary memory (Craik & Levy, 1976; Waugh & Norman, 1965); another one is the distinction between episodic and semantic memory, or episodic and categorical memory (Estes, 1976). In addition to these kinds of distinctions, some theorists have found it useful to postulate different basic types of information handled by the memory system (e.g., Murdock, 1974). Classification systems of this sort may be useful for several reasons. For instance, they naturally lead to questions about interrelations between different kinds of memories or different kinds of information, and the pursuit of these questions may turn out to be quite profitable. Another benefit of the classification lies in the natural boundaries within which theorists can seek generality when trying to make sense out of empirical data. It is easier to come to grips with the phenomena of short-term episodic memory, for instance, than with all phenomena under the rubric of memory.

The fifth change, and in some ways potentially perhaps the most important, concerns the increasing tendency on the part of students of memory to exhibit a critical attitude toward metatheoretical problems. Searching questions have recently been posed about the general strategy of memory research, the basic objectives of the total enterprise, the value of laboratory experimentation, the role that empirical phenomena play in attempts to understand memory, the potential contribution that knowledge gleaned from other fields might make, and even the usefulness of the currently popular memory metaphors. Thus, for instance, Newell (1973) has characterized typical research in cognitive psychology, including human memory, as taking the form of discovery and exploration of specific phenomena and their explanation by "oppositional concepts." Newell does not believe that this is a fruitful strategy. Others have expressed serious reservations about the sterility of laboratory experimentation in memory. For instance, Neisser (1976) says that phenomena that contemporary students of memory are trying to explain are "highly artificial": recall of word lists or nonsense syllables, recognition of unrelated pictures, and memorization of sentences or short prose passages. He suggests that theorizing about memory is premature until more is known about memory in the natural situations where memory develops and is normally used. As a third illustration, Bransford, Franks, McCarrell, and Nitsch (1977), as well as Turvey and Shaw (1977), have questioned the usefulness of the assumption that storage and retrieval of memory traces underlies remembering or mediates the effects of learning. As an alternative to the memory storage and retrieval metaphor, they suggest a "stage-setting" or an "attuned organism" conceptualization of remembering. This view has certain advantages over the existing ideas. For instance, it makes it easier to understand how a person can

readily perceive an object as "new"; the memory storage and retrieval metaphor would have to account for the perception of newness in terms of failure to retrieve a particular trace.

One might disagree with Newell and argue that whether or not one wins or loses when playing twenty questions with nature does not depend on the characteristics of the game but on the goodness of questions that one asks. It is also possible to disagree with Neisser and think that what is important is not whether observations about memory are made in the laboratory or in some natural setting, but rather the sense that one can make of the observations, and that there is no a priori reason to suspect that the sensibility of facts depends on the setting in which they are gathered. Similarly, one could question whether the difference between the concept of memory trace and that of an attuned organism, or storing information and setting the stage, is anything more than an instance of "our words are better than yours" (McGeoch & Irion, 1952). But in the present context these are less important points. What is more important is the fact that critical analysis directed at both the grand strategy and specific tactics of research, and at some of the most basic assumptions with which we have worked for a long time, represents a striking change in memory research. Until most recently, such free and open discussion was rare in the literature of experimental psychology and human memory. Although many contemporary students still frown on it, times are changing in this respect, too.

SOME LIMITATIONS AND SHORTCOMINGS

There has been a good deal of change and obvious growth in memory research. How about progress? The answer to this question depends on how we define progress. According to the dictionary, progress is movement toward a goal or to a higher stage; and social scientists think of it as the development of a group in a direction regarded as superior to the previous level. The goal toward which our science moves cannot be specified in advance; therefore it is difficult if not impossible to judge progress in terms of changes that take us closer to the goal. But many students of memory undoubtedly would agree that the enterprise has moved to a higher stage, in a direction that is superior to the previous level. Using the criterion of social agreement, and seeking agreement from the practitioners themselves, we would have to conclude that we have indeed made some progress. Indeed, the contrary argument—that we have made no progress whatsoever—even if it were true in some absolute sense, would be counterproductive; and for these reasons, it is better avoided.

What we can ask, however, is how much progress we have made. To guide our judgment on this issue we need some appropriate, explicitly stated criteria. Probably the single most important relevant criterion for judging

progress in a science is the extent to which the results of past work have been firmly welded into a cumulatively developing structure of knowledge. Thus, we must decide to what extent the knowledge we have gained from our research is cumulative and to what extent a relatively permanent structure of knowledge already exists. This structure of knowledge would include hard, empirical facts, a definite set of basic concepts, and theoretical solutions to empirical problems.

We do have many facts and findings in our science of memory, and many of them are reasonably hard. But it is not clear that we know what all these facts and findings mean or what they add up to. The conceptual development of our enterprise has simply not kept up with our ability to design and conduct experiments and to generate data. This is where our difficulties lie. After a hundred years of laboratory-based study of memory, we still do not seem to possess any concepts that the majority of workers would consider important or necessary. If one asked a dozen or so randomly selected, active memory researchers to compile a list of concepts without which they could not function, one would find little agreement among them, particularly if one excluded terms referring to experimental operations and data. Similarly, if one compares different current textbooks of memory, one discovers that there is little overlap among their subject indexes: It seems that important concepts of one author can apparently be easily dispensed with by another. Most of our concepts tend to be esoteric: They are used by small groups of people and either ignored or found confusing by others. There is also a good deal of terminological confusion. One and the same term may be used in rather different senses by different investigators.

Related to the absence of progress in the realm of concepts of memory is another phenomenon: The history of our science knows no generally acknowledged solutions to problems. It is difficult to think of a single instance where a problem, generally perceived to be such by the majority of the practitioners in the field, was explained by one investigator and the explanation accepted by most others. What we have inherited are not solutions but problems requiring solutions. Some readers of the present essay undoubtedly will think that my assessment of the situation is not entirely realistic. All that such a reader needs to do to prove my position untenable is to compose a list—even a short list—of problems that have been solved or explained in a nontrivial or relatively permanent sense. Until such time that someone steps forth with such a list, however, it is difficult to resist the conclusion that ours is not yet a cumulative science, that we have not yet succeeded in constructing a stable foundation of knowledge and understanding of memory, and that the progress we have made in the past, therefore, must be regarded as rather modest.

The psychological study of memory shares many characteristics with other sciences. We accept the important role played by observation, particularly

controlled observation, and measurement. We agree on rules by which claims and statements about nature are judged, inferences from observations made, data and theory related, and controversies adjudicated. We also have accepted the ethos of science as a social activity, recognizing the inviolate nature of peer judgment and the sanctity of priority of discovery. In some other matters, however, we are closer to what Thomas Kuhn (1962) referred to as preparadigmatic sciences. We lack agreement and consensus as to the importance of problems and facts; our fact-gathering activity is frequently guided by facts already available (we do experiments on experiments, rather than experiments on problems); the importance of one and the same phenomenon may be perceived differently by different investigators; we sometimes tend to evaluate goodness of research in terms of its adherence to conventional form rather than its promise to tell us something we do not yet know; in evaluating each other's research we frequently invoke philosophers of science; we tend to make a fetish out of certain research tools (for instance, statistics); and we are not always clear in our own minds as to what constitutes a useful explanation of a phenomenon. The form of the present essay does not permit me to elaborate on all of these remarks, but I would like to make a few observations about the last point, the nature of explanation.

Many researchers are fond of explanations of memory phenomena that take the form of a functioning structure that behaves in some respects like the learner or the rememberer. This functioning structure may be presented as a set of verbal statements, a flow diagram, a logic machine, a mechanical model, a set of mathematical equations, a computer program, or a mixture of these. Frequently the explanation describes internal processes in terms of activities such as search, generation, differentiation, decision making, discrimination, comparing, "accessing," locating, transforming, and so forth. The purpose is always the same, to construct a simplified model of the human being in a particular task situation in such a manner that inputs into and outputs from the model and the learner are isomorphic in some specified sense.

Explanations assuming the form of correspondences between observed phenomena and different kinds of functioning structures are very popular in psychology, including psychology of memory; but their usefulness must be questioned. Presumably those theorists who have a penchant for such explanations assume that the correspondence between the human learner and the model of that learner extends to components of the two systems that are not directly observable in the case of the learner. This is not a reasonable assumption, for several reasons. First, in almost all situations the number of different functioning structures that can be brought into some sort of isomorphic relation with a human subject performing a task in a particular situation is virtually unlimited; and the specification of one possible model, even if it is shown that in some sense it is better than a few others, is not

particularly informative. Second, we have good reasons to be skeptical of all explanations that postulate various, humanlike activities (searching, locating, discriminating, decision making, etc.) as component processes of the mind, which governs and coordinates similar activities on the part of its possessor. Because of their failure to reduce uncertainty and their typical lack of plausibility, correspondence models do not bring us any understanding of memory or its phenomena. They may provide a sense of accomplishment to their creators and some feeling of closure to those who do not question their rationale, but it is difficult to imagine how they would contribute to conceptual progress in our field. We will return to the problem of the nature of explanation in the next section, considering its more promising forms.

STEPS TO THE FUTURE

The science of memory is still looking for its first genuine Kuhnian paradigm; and there is no doubt that one day, one or more paradigms—for the science of memory or for one or more of its offshoots—will be found. When that day arrives, it would be possible to claim that there has been progress. Is there anything that we can do, individually or collectively, to hasten the pace of progress? I am sure that all active researchers have definite ideas on this question and that in some sense we all act out our convictions in our own work. In keeping with the rest of the picture, it is more than likely that there is a good deal of disagreement on what furthers the cause of progress and what hinders it. In this last section of the essay, I mention some of my own ideas on the topic. Most of the points I make have to do with the nature of explanation and understanding that we should be seeking.

First, it is probably unwise to spend too much effort and energy in developing explanations of things that make sense even without explanation. What we do wish to understand are phenomena, deviations from what is intelligible and reasonable in nature (Toulmin, 1963). For instance, because it makes good sense to believe that a living organism capable of some behavior, now is also capable of it on a subsequent occasion, it is not necessary (or even possible within the domain of behavioral analysis) to construct a theory of retention—that is, maintenance of the knowledge about a fact or an event over time. Instead, what may need explanation is the failure of such maintenance—that is, forgetting. What are the natural givens at this time in the science of memory, facts that require no explanation?

Second, it is probably not the optimal strategy to spend too much time in affirming what we already know. There is every reason to believe that all current ideas, interpretations, explanations, and theories are wrong, in the sense that sooner or later they will be modified or rejected. Propping them up, as it were, may simply delay the change that, after all, is a necessary condition for progress. Thus, supporting, retaining, and affirming existing theories

longer than necessary more often than not stands in the way of progress. Experiments aimed at existing theories should be designed to find out how, where, and in what sense these theories need revision or why they should be rejected, not to provide additional evidence for them. What theories do we have now that have survived despite the determined efforts on the part of many experimenters to find fault with them?

Third, explanations of individual phenomena, or models constructed to account for the results of single experiments, in and of themselves are not particularly valuable. As I mentioned earlier and as argued by others (e.g., Anderson, 1976), the number of possible explanations that fit individual phenomena or results of single experiments is always extremely large, and therefore describing one represents a technical accomplishment and not a scientific discovery. It is easy to agree with Postman (1975), who—when noting the remarkable proliferation of memory models—thought it fortunate that the ratio of models to experiments is still less than unity. In a trenchant review of the developments in mathematical psychology, Estes rejected the success of fitting models to data as a criterion of progress and stated: "What we hope for primarily from models is that they will bring out relationships between experiments or sets of data that we would not otherwise have perceived" (Estes, 1975, p. 271). The same criterion, of course, applies to all forms of explanatory attempts. I would like to add that another form of promising theoretical activity is analysis of more complex phenomena into their simpler components, whose existence and behavior can be verified and measured experimentally under conditions that do not require long and unwieldy chains of inferential reasoning. These components should remain invariant across different tasks and situations. We should seldom be interested in what happens and why in a particular situation; rather, we should always try to understand how what happens in one situation is related to what happens in another. Thus, we should be looking for invariance in the face of apparent variance (Stevens, 1951).

Fourth, premature formalization of whatever kind may hamper progress by providing a sense of false security. Our intellectual resources are misplaced if they are spent on the construction of elaborate and unwieldy logical and mathematical structures to explain experimental findings in situations in which the facts are soft and the basic concepts still to be invented. Preoccupation with precision of measurement and attempts to discriminate between methods yielding highly correlated measures represent another case of misplaced effort. John Platt (1964) has reminded all scientists:

> Many—perhaps most—of the great issues of science are qualitative, not quantitative, even in physics and chemistry. Equations and measurements are useful when and only when they are related to proof; but proof or disproof comes first and is in fact strongest when it is absolutely convincing without any quantitative measurement [p. 352].

We should keep these words in mind whenever we run into temptations to evaluate our own research efforts and accomplishments by the standards of mature sciences.

Fifth, we would do well to assume a skeptical attitude about theories and explanations of memory that accord with common sense. Just about the only worthwhile lesson that we can learn from the history of science is that almost invariably lasting intellectual achievements take the form of theoretical formulations that are not immediately transparent. It is possible that one of our present sources of conceptual difficulties has to do with our desire to reconcile theoretical ideas with the introspective awareness that we have of the workings of our own minds. We tend to dismiss ideas that jar our personal knowledge of memory, even when they agree with experimental observations. I am not advocating that we automatically discard all ideas that do accord with common sense; but I am suggesting that given the choice between two otherwise equivalent ideas, the one that fits less readily into what we already know may be preferable.

Sixth, it is highly desirable, wherever and whenever possible, to follow the precepts of "strong inference" (Platt, 1964). Strong inference is the name that John Platt gave to the method of inductive inference that originated with Francis Bacon. Platt thought that giving it a special name was necessary, because many scientists make little use of it, although it may be the most important ingredient of scientific progress. There are two critical elements of the method: formulating alternative hypotheses and designing crucial experiments whose outcomes will exclude one or more of the hypotheses. Thus, exclusion is the crux of the matter. In the psychological science of memory, we seldom see crucial experiments; the difficulty usually lies in devising testable alternative hypotheses, rather than in creating methods of distinguishing between them once they exist. Platt says that "in numerous areas that we call science, we have come to like our habitual ways, and our studies that can be continued indefinitely. We measure, we define, we compute, we analyze, but we do not exclude" (Platt, 1964, p. 352). I wonder how things would change in our field if it became an accepted practice for authors of hypotheses and explanations to state explicitly exactly what kinds of empirical facts would refute them.

Finally, we should be willing—perhaps "have the courage" would be a more appropriate expression—to reject ideas and hypotheses that are at variance with the data. Instead, frequently the hypotheses incompatible with the data are maintained or just mended, and mended again when they encounter further difficulties. Mending usually takes the form of adding an additional wrinkle, another qualification, or another parameter or two. If such recurrent modification continues for a while, the explanation may eventually collapse under its own weight; but in the meantime its existence has stood in the way of an active search for a better one.

It is sometimes argued that just because an experimental fact is at variance with a hypothesis or a theory need not lead to its abandonment, or even revision, and that a hypothesis or a theory can only be replaced by a better hypothesis or a theory. Such arguments are supported by the citing of certain celebrated cases from the history of science, such as the phlogiston theory, that provide neat illustrations of the point. These arguments, however, make little sense in a developing discipline such as ours in which even the most popular theories are frequently based on rather flimsy evidence. If we had theories that encompassed many diverse facts and meaningfully interrelated them, we of course would not reject them out of hand at the first appearance of some contrary evidence. Until such theories are developed, however, we would do better to play the game according to the rules of strong inference.

What we need more than anything else at the present time of the development of our science is variety: variety of questions, issues, and problems, variety of methods and techniques, variety of theoretical ideas, and variety of approaches to explanation and theory construction. For the time being, therefore, divergence of methodological, experimental, and theoretical thinking should be actively encouraged. The laws of natural selection can operate more successfully, the greater the wealth of organic—or in the case of our science, intellectual—material on which they act.

A CONCLUDING COMMENT

Jöns Jakob Berzelius, one of the towering figures of 19th-century science and a graduate of the great university whose 500th birthday the present volume helps to celebrate, gave a lecture in 1810 to the Royal Swedish Academy of Science under the title, *The Progress and Present State of Animal Chemistry.* In it he showed full awareness of the problems that would meet the inquiring mind trying to understand biological processes (quoted in Jorpes, 1966):

> If knowledge of the transformation of the blood to other fluids, which in itself is analogous to ordinary chemical phenomena, is so obscure to us, how much more so is the renewal of the solid living parts of the body, which is maintained by the constant exchange of fundamental substances. Even more astonishing is the function of the brain; thought reels at the notion that, even in its most soaring flights or when it penetrates most deeply into the secrets of nature, it depends on a chemical process which precedes it, and the slightest error in which would destroy its coherence, turn it to madness or completely destroy it . . . and yet this is an indisputable fact. But is it likely that man's intellect, the fruit of so much cultivation, which has calculated the laws of motion of far-off worlds, which has come to understand much of the beauty and the strangeness of the nature which surrounds it and which by these advances has come a little nearer to the knowledge of the nature of the Almighty, will one day come to understand itself and its own inner nature? I think not [p. 35].

Memory is an integral part of man's inner nature. Evaluation of the progress that has been made in its understanding therefore provides us with a partial test of Berzelius' prophecy. The main thesis of this essay has been that a century and two thirds later, Berzelius' prophecy still stands.

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