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ORIGINS OF THE COGNITIVE (R)EVOLUTION

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The well documented cognitive "revolution" was, to a large extent, an evolving return to attitudes and trends that were present prior to the advent of behaviorism and that were alive and well outside of the United States, where behaviorism had not developed any coherent support. The behaviorism of the 1920 to 1950 period was replaced because it was unable to address central issues in human psychology, a failure that was inherent in part in J. B. Watson's founding manifesto with its insistence on the seamless continuity of human and nonhuman animal behavior. The "revolution" was often slow and piecemeal, as illustrated by four conferences held between 1955 and 1966 in the field of memory. With the realization that different approaches and concepts were needed to address a psychology of the human, developments in German, British, and Francophone psychology provided some of the fuel of the "revolution." © 2002 Wiley Periodicals, Inc.

The facts of the cognitive revolution in psychology in mid-twentieth century have been well documented (see, for example, Baars, 1986; Greenwood, 1999, and on more special issues see Murray, 1995; Newell & Simon, 1972). What follows is intended as a further elaboration of those previous presentations. The adoption of, or return to, cognitive themes occurred in other disciplines as well, for example in linguistics, but those developments are outside the scope of this presentation. Nor do I wish to treat in detail all areas of experimental psychology; I will concentrate on approaches to human memory. I wish to add the following four arguments to our general understanding of the events surrounding the cognitive resurgence: (1) Part of Watson's program prevented the success of behaviorism and contributed to its replacement. (2) The term "revolution" is probably inappropriate—there were no cataclysmic events, the change occurred slowly in different subfields over some 10 to 15 years, there was no identifiable flashpoint or leader, and there were no Jacobins. (3) The behaviorist dogmas against which the revolution occurred were essentially confined to the United States. At the same time that behaviorism reigned in the U.S. structuralist, cognitive, and functionalist psychologies were dominant in Germany, Britain, France, and even Canada. (4) Stimulus-response behaviorism was not violently displaced, rather as a cognitive approach evolved behaviorism faded because of its failure to solve basic questions about human thought and action, and memory in particular.

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The Birth and Failure of American Behaviorism

The early twentieth century in the United States was marked by a turning inward, a new American consciousness.[1] In science and philosophy, the new twentieth century was marked by a pragmatic, antitheoretical preoccupation with making things work—a trend that was to find its expression in psychology in J. B. Watson's behaviorism. I add a remark of Alexis de Tocqueville's which is apposite of the behaviorist development and relates its origin to a more lasting tradition of American democracy: ". . . democratic people are always afraid of losing their way in visionary speculation. They mistrust systems; they adhere closely to facts and study facts with their own senses" (Tocqueville, 1889, p. 35).

I want to stress a part of J. B. Watson's arguments that has been neglected in the past. Watson's dismissal of the introspectionism of his predecessors is well known and documented (see, for example, Baars, 1986). I argue in addition that another part of his attack against the established psychology contained the seeds of the failure of his program. In his behaviorist manifesto of 1913, Watson, who had been doing animal experiments for some years, claimed to be "embarrassed" by the question what bearing animal work has upon human psychology and argued for the investigation of humans that is the exact same as that used for "animals." In the first paragraph of the article, he asserted: "The behaviorist, in his efforts to get a unitary scheme of animal response, recognizes no dividing line between man and brute" (Watson, 1913, p. 158). The manifesto was in part a defense of his own work, a way of making it acceptable and respectable. Watson's preoccupation with marking his place in American psychology was also noticeable in his treatment of his intellectual predecessors. He referred to "behaviorists," i.e., his colleagues in work on animal behavior, but there was no acknowledgment that animal researchers as G. J. Romanes, C. Lloyd Morgan, or Jacques Loeb are his conceptual predecessors and pathfinders.[2] He did give credit to Pillsbury for defining psychology as the "science of behavior."

Watson's continuing argument was clothed primarily in the attack on structuralism and E. B. Titchener's division of experience into the *minutiae* of human consciousness (Titchener, 1910, particularly pp. 15–30). However he expanded the argument for behaviorism on the basis of using animal experiments as the model for investigating human functioning. The following year, in his banner book (Watson, 1914), he complained even more strongly that his work on animal learning and related topics had not been used in our understanding of human psychology. Watson's unification of human and nonhuman behaviors into a single object of investigation prevented a psychology of the human and the human mind from being established, and in particular it avoided sophisticated investigations of human problem solving, memory, and language.[3] Eventually behaviorism failed, in part because it could not satisfy the need for a realistic and useful psychology of human action and thought.

Watson's goal was the prediction and control of behavior, particularly the latter when he equates all of psychology with "applied" psychology. There is the reasonable suggestion, made *inter alia,* that we need to be—as we have since learned to call it—methodological behaviorists, i.e., concerned with observables as the first order of business of our, as of any,

1. I have sketched the socio-political origins of American behaviorism elsewhere (Mandler, 1996, reprinted in Mandler, 1997). For a discussion of the prebehaviorist "cognitive" psychologies see Greenwood (1999) and Mandler & Mandler (1964).

2. The omission of Loeb may have been deliberate, since Watson had been warned away from him as a graduate student (Watson, 1936, p. 273).

3. Though by 1919 he was fully committed to the investigation of human emotion from a behaviorist standpoint, i.e., "that the human organism is built to react in emotional ways." (Watson, 1919, p. 223).

Table 1
Articles in the Journal of Experimental Psychology

Year	No. Articles	Nonhuman Subjects, %	Editor
1917	33	0	J. B. Watson
1927	33	6	M. Bentley
1937	57	9	S. W. Fernberger
1947	50	30	F. I. Irwin
1957	67	22	A. W. Melton
1967	87	15	D. A. Grant
1977*	20	10	G. A. Kimble

* These data are for JEP's successor journal, the *Journal of Experimental Psychology: General*

science. Post-behaviorist psychologies did not ask for the feel or constituents of conscious experience, but rather were concerned with observable actions from which theories about internal states could be constructed.

Watson's influence was probably most pervasive in his emphasis on the stimulus-response (S-R) approach.[4] The insistence on an associative basis of all behavior was consistent with much of the empiricist tradition. The exceptions were E. C. Tolman's invocation of "cognitive maps" and Skinner's functional behaviorism. However, most behaviorists seriously attempted to follow Watson's lead in insisting on the action of stimuli in terms of their physical properties, and on defining organism response in terms of its physical parameters—the basis for a popular reference to behaviorism as the psychology of "muscle twitches." The position is of course a direct result of working with nonhuman animals, for whom it was at least difficult to postulate a "cognitive" transformation of environmental events and physical action. B. F. Skinner on the other hand used functionalist definitions of stimuli and responses as eliciting/discriminative conditions and operant behavior (Skinner, 1995). However, his initial focusing on the behavior of pigeons and rats also alienated him from research on specifically human functions, and it is likely that Chomsky's review of Skinner's "Verbal Behavior" put him beyond the pale of the burgeoning cognitive community.

One of the consequences of Watson's dicta was the switch to animal work in the mainstream of American psychology. Table 1 shows the shift over decades into animal work as well as its subsequent decline in the primary journal (*Journal of Experimental Psychology*).

This rise and decline in animal research[5] took place independently of the interests of the editors, the majority of whom were in fact not doing research on nonhuman subjects. It also illustrates the basis of the developing unhappiness among many psychologists doing research on human memory and related topics at being shut out of the most prestigious publication outlets (see below). When human subjects were used, it was frequently for studies of eyelid conditioning and related topics in uncomplicated (noncognitive?) conditions and environments. For example, in addition to the 30% animal studies in the 1947 volume, another 14% were on conditioning.

At the theoretical level, very little of Hullian theory was applicable to complex human behavior. John Dollard and Neal Miller (1953) presented a major attempt to integrate per-

4. However, as Samelson's (1981) research has shown, it took some years after 1913 before one could speak of a general acceptance of behaviorism.

5. Robins, Gosling, & Craik (1999) also noted a general decline in articles on animal research during the second half of the twentieth century.

sonality theory (mostly derived from Freud) into the Hullian framework, and Charles Osgood (1950) tried to explain much of human action in terms of associationist mediation theory. The major attempt to apply Hullian principles was in the volume on a mathematico-deductive theory of rote learning (Hull et al., 1940). Apart from a somewhat naive and rigid positivism, the theory generated predictions (primarily about serial learning) that were patently at odds with existing information, the logical apparatus was clumsy, and the predictions difficult to generate. The book generated no follow-ups of any influence, nor any body of empirical research. It was irrelevant. The proposals developed few consequences, and together with the insistence that all thought processes could be reduced to implicit speech, it was generally accepted that the Hullian approach had little to offer to an understanding of human thought and action.

There is a wealth of anecdotal information about the difficulty of getting human research work into print during the behaviorist period. Much of the work was eventually reported in relatively obscure (and essentially unrefereed) journals like those of the Murchison group (e.g., *Journal of Psychology, Journal of Genetic Psychology*) and *Psychological Reports.* One example of work sidelined into secondary journals were studies on clustering (categorical and otherwise) in memory organization and related activities. W. Bousfield started these major deviations from the stimulus-response orthodoxy with a paper in 1953, a change which C. N. Cofer recognized early on as a tie to Bartlett's work (Jenkins & Bruce, 2000). When James Jenkins and Wallace Russell pursued a related topic, they published in the *Journal of Abnormal and Social Psychology* because they believed that Arthur Melton would not accept it for the *Journal of Experimental Psychology* since it was concerned with recall rather than learning (Jenkins & Russell, 1952). A few years later, Jenkins and associates sent Melton one of their papers (the subsequently widely cited Jenkins, Mink, & Russell, 1958) and were told by Melton, scribbled across their submission letter, that "this would be of no interest to my readers."[6] Another example of behaviorist hegemony was the difficulty that K. and M. Breland had in publishing any criticisms of Skinner's position on innate dispositions (Bailey & Bailey, 1980).

As I have indicated, one of the reasons why stimulus-response behaviorism and research on human memory and thought were incompatible was the physicalism of the S-R position. The eliciting stimuli were defined in terms of their physical characteristics and, in principle, responses were either skeletal/muscular events or their equivalents in theoretical terms. Such concepts as the "pure stimulus act" and r_g — the anticipatory goal response — were theoretical notions that were to act implicitly in the same manner as observable behavior and were intended to do much of the "unconscious" work of processing information. Greenwood (1999) has discussed in detail the shortcomings of Hullian psychology with respect to representation and to conceptual processing.

Whether the cognitive revolution had a specific target is debatable because the change was one of movement to a more adaptable set of presuppositions rather than the destruction of the old ones. Research on human information processing, as the cognitive movement was called early on, moved to new or neglected areas of research (such as free recall and problem solving) rather than attacking research with non-human animals. If there was a target it was the Hull-Spence position — primarily because of its preeminence in the field as a whole and its dominance over contending behaviorist positions such as Tolman's. I would argue that it

6. Personal communications from James Jenkins.

is not the case, as Amsel has argued, that the "behaviorism that cognitive scientists attack is a caricature . . . of J. B. Watson and B. F. Skinner" (Amsel, 1992, p. 67). During the 1930s and 1940s, the dominant figures of American behaviorism were Clark Hull, and eventually Kenneth Spence, and to the limited extent that the new cognitivists drew boundaries it was between them and the Hull-Spence axis. However, the latter's influence declined as behaviorism in general faded. Skinner, on the other hand, maintained some of his influence, so that in the year 2000 there were 220 literature citations for B. F. Skinner, while there were 73 for C. L. Hull and 26 for K. W. Spence.[7]

As S-R behaviorism faded, there was little in the way of Jacobin sentiments, of a radical rooting out of the previous dogmas. Certainly, a few of such sentiments found their way into print. Much was said in colloquia and in congress corridors, but the written record does not record a violent revolution. If anything qualifies as a Jacobin document it was Noam Chomsky's attack on Skinner's *Verbal Behavior* (1957), though the attack was not against the dominant Hull-Spence position (Chomsky, 1959). It might also be argued that Chomsky failed to distinguish between the stimulus-response analyses of Hull-Spence and the functionalism of Skinner.

The Limited Appeal of Behaviorism and the Seeds of Change

If it is the case, as I have implied, that behaviorism represented only an interlude in the normal flow of the development of psychological science, what was it that was interrupted and what was there to replace the behaviorist position, once it was shown to be inadequate. J. D. Greenwood (1999) has discussed one such tradition that developed out of the work of the Würzburg school, Oswald Külpe in particular, of the psychology of Otto Selz, of the work on directed thought by Ach, as well as later content and rule based psychologies.[8]

Within the United States, the 1940–1945 war created another nest of antibehaviorist developments. The war effort brought together a number of people in various projects. Of special importance to later developments was a group at MIT and Harvard, which included J. C. R. Licklider, S. S. Stevens, Ira Hirsh, Walter Rosenblith, George A. Miller, W. R. Garner, and Clifford Morgan. Their original war work was primarily in psychoacoustics and noise research, but it extended into signal detection and related topics. With the creation of the Lincoln Laboratory at MIT in 1951, this early deviation from behaviorist dogma prepared the ground for mathematical models and the commanding influence of signal detection theory in perception as well as memory and other fields (Green & Swets, 1966). By the time the revolution started, these strands were ready to contribute to a new psychology. Similar accumulations of talent occurred in other parts of the war establishment as well as in Britain (e.g., in the influence of the military interest in vigilance phenomena on D. E. Broadbent). Finally, an important influence that was not Hullian (despite its origins at Hull's Yale) was Carl Hovland's work on concept formation, attitude change, and related phenomena (e.g., Hovland, 1952).

What about the psychology that coexisted in Europe with the behaviorism of the United States? The important aspect of European psychology of the time was that not only was

7. Courtesy of the WebofScience.

8. For a discussion of and presentation of a selection of those earlier developments, see Mandler & Mandler (1964).

Europe essentially unaffected and uninfluenced by behaviorism,[9] but also that the developments in Europe became part of the American mainstream after the decline of behaviorism. There was both a general opening up of America to European ideas and the influx of European psychologists into the United States. Interestingly, if there was little influence from the United States to Europe so was there relatively little leakage of psychological theory across European frontiers. In the nineteenth century, William James was read in Europe and Wilhelm Wundt was an international figure up to the beginning of the next century. But in the twentieth century the various national groups were relatively insulated.

In Germany—apart from the early influence of the Würzburg school—the major development in the early years of the twentieth century was the advent of Gestalt psychology. Wolfgang Köhler, Kurt Koffka, and Max Wertheimer created a psychology that was concerned with an analysis of human conscious experience and with organizing structures, concepts alien to behaviorism. Gestalt psychology introduced—without apology or embarrassment—structures that controlled experience but were themselves not amenable to observation or introspection. Gestalt psychology was the earliest European influence on U.S. psychology, primarily because the advent of National Socialism eradicated German scholarship and forced the major figures of the Gestalt movement to leave the country. Most of them arrived in a behaviorist America where they failed to have any immediate influence as they were forced to make do on the fringes of the psychological academic establishment.[10] Despite their apparent marginality in a behaviorist environment, they still had an important influence on the nascent cognitive developments (see, for example, Hochberg, 1968, and Köhler, 1959).

In francophone Europe (mainly Switzerland and France), much of the work in the early twentieth century was in developmental psychology. The major figure was Jean Piaget, whose work was available in English as early as the 1920s (Piaget, 1926). Similarly, Edouard Claparède's work with children had been translated, but not his major contribution to the problem of hypothesis formation (Claparède, 1934). Binet's work on intelligence testing was well known early on. However, there was little early interest in a theoretical developmental psychology, much of the focus was on clinical developmental problems. In particular, the interest in cognitive development did not take off until well after World War II. But there is no doubt that figures like Jean Piaget were central in that development in the United States.

The most extensive cognitive developments during the behaviorist interlude in the United States occurred in Britain. It is of particular interest since no language barrier would have prevented these ideas from being generally adopted in America—but it was not to be. The early stages in the British history of cognition (see also Collins, 2001) were set by F. C. Bartlett in the 1930s, and by the brilliant Kenneth Craik who died in an accident in 1945. Craik suggested in 1943 that the mind constructs models of reality: "If the organism carries a small-scale model of external reality and its own possible actions within its head, it is able to try out various alternatives, conclude which is the best of them, react to future situations before they arise, utilize the knowledge of past events in dealing with the present and future, and in every way react to a much fuller, safer and more competent manner to emergencies which face it" (Craik, 1943, p. 57). Craik was the first director of the Applied Psychology

9. For example, George Miller (in Baars, 1986, p. 212) reports being told after a talk at Oxford in 1963 in which he had attacked behaviorism: "[T]here are only three behaviorists in England, and none of them were here today!" In 1965, I was approached by a senior British psychologist and asked, in all seriousness, whether anybody in America really believed any of the behaviorist credo.

10. For an account of the German immigration in psychology, see Mandler & Mandler (1968). For a specific account of the Gestalt psychologists in the U.S., see Sokal (1984).

Unit in Cambridge which for another half century would be a leading center for cognitive psychology. He was succeeded by F. C. Bartlett and Norman Mackworth. In 1958 Donald Broadbent became the APU director. Broadbent also anticipated the American revolution with his early work on attention in the 1950s and his work on communication (Broadbent, 1958). Another important influence in Britain was George Humphrey whose two books on the history and data on thinking summarized the field and pointed to new directions (Humphrey, 1948, 1951). And, finally, mention must be made of our British-Canadian neighbors and the influence of D. O. Hebb on the postbehaviorist psychologies in the United States (Hebb, 1949).

In summary, there was an obvious plethora of nonbehaviorist ideas available in the world during the 1930s and 1940s. Some of them were heard in the U.S. but none of them was rigorously or widely followed. It was not until the late 1950s that the failure of behaviorism made room for these "foreign" notions.

The Waxing and Waning of Associationism

In the nineteenth century, experimental psychology was initially dominated by German psychology, which, in turn, had embraced British empiricism and associationism to a large extent. That embrace was particularly evident in the experimental study of memory started by Hermann Ebbinghaus (1885). Ebbinghaus introduced the serial and associative learning paradigms that were to dominate the field for many decades.[11] With minor perturbations, the Ebbinghaus tradition smoothly merged into the functionalist tradition of the early twentieth century (McGeoch, 1942), and then into the behaviorist methodologies. The research was behaviorist in style, emphasizing stimulus-response connections and some concepts (such as reinforcement and stimulus generalization) imported from the Hull-Spence tradition. Thus, an often atheoretical neo-Ebbinghaus tradition survived the war and continued into the l950s. The preoccupations of the verbal learning psychologists were focussed on associations, their nature and strengths. Was there an alternative conception?

In fact a productive movement of work on memory had subverted the dominant associationist and behaviorist themes for some time. Historically, as Greenwood (1999) has noted, it was Locke who had pointed out that the "association of ideas" did not provide a general explanation of human reasoning. In modern times, the movement was characterized by Bartlett's work with schemas and his insistence that memory was constructive not reproductive (Bartlett, 1932) and by the associationist Thorndike's experiments demonstrating that *belongingness* ("this goes with that") was a major factor in determining what was learned and retained (Thorndike, 1932, p.72). The culmination was the publication of George Katona's book on memorizing and organizing (Katona, 1940).[12] Katona spent much time in explicating, both experimentally and theoretically, basic principles of *Gestalt* psychology such as understanding, grouping, whole-relations and the function of meaning; the final message is clear: "[O]rganization is a requirement for successful memorization. It must be present in some form in all kinds of learning" (p. 249). Organization refers to the establishment or discovery of relations among constituent elements. Katona's book characterized the organizational

11. Though it was Ebbinghaus who first noted that much of human memory, particularly in every day thought, was nondeliberate (Mandler, 1985) and not represented by the very experimental methods he had popularized—an important addendum and mostly and mistakenly ignored for nearly a century.

12. Murray and Bandomir (2000) have discussed insightfully Katona's indebtedness to G. E. Müller for many of his insights into problems of organization.

movement. It was typical of the behaviorist interlude that, in 1941, Arthur Melton, one of its gatekeepers, dismissed Katona's book as lacking operational definitions and producing unreliable results (Melton, 1941). Not surprisingly the attempts to introduce notions of organization into American psychology were not successful.

With the onset of the "revolutionary" period, new attempts were mounted to replace associationist thinking with organizational principles, i.e., that the glue that held together memorial contents were categories and organizations of words, thoughts, and concepts rather than item to item associations (Bower, 1970; Mandler, 1967, 1977, 1979; Tulving, 1962). By 1970, organization had been reinvented and became the major direction for memory research for about ten years. The new organizational psychology was probably a significant improvement over its predecessor—advances in experimental and statistical techniques and specifications of theoretical mechanisms represented significant forward steps over the *Gestalt* notions of the earlier period.[13]

The "revolution" tended to be long and convoluted, highlighted in a series of conferences. I will discuss the ones on memory below, but memory was not the only nor was it the first field of psychology to organize conferences on the new directions. One of the most direction-giving occasions was the "Special Group on Information Theory" of the Institute of Electrical and Electronics Engineers which met at MIT in 1956 (see Baars, 1986, passim). At that meeting Noam Chomsky, George Miller, and Alan Newell and Herbert Simon presented the initial papers of a trend that would be defining in the next decade. A similar pace setter, in that case for the emergence of Artificial Intelligence and its relation to cognitive processes, was the London Symposium on the Mechanisation of Thought Processes in 1958 (Anonymous, 1959). In other areas the attention to cognitive factors developed at various times during the decades following the 1950s, as in emotion (Schachter & Singer, 1962), perception (Hochberg, 1968), and personality theory (Mischel, 1968).

I now turn to a case study of the "revolution" in the memory field, which illustrates the successive steps toward a different way of looking at a discipline. The field was called "verbal learning" under the behaviorist aegis, continuing a belief that basic learning processes (no different from those operative for nonhuman animals) were being investigated. Since "learning"—the novel association of stimuli with responses—was the basic law of psychology, all behavioral phenomena, including so-called memory processes, had to be brought under the operation of that basic law.

A Case Study in Memory[14]

Deviations from the behaviorist stimulus-response orthodoxy occurred early on in the field of "verbal learning." A case history of the area is interesting because the field was populated not only by "revolutionaries" but also by large number of orthodox conservative researchers. I have already noted some of the early changes that were initiated in the early 1950s by Bousfield and others. This was followed by C. N. Cofer's convening an informal

13. It is difficult to specify exactly when organizational variables stopped attracting both theoretical and empirical attention, but by the early 1990s the status of organization as necessary for recall and recognition was all but forgotten. Studies of the "strength" of individual items and their connection to other items regained prominence and the connectionist movement (Hinton & Anderson, 1981; Rumelhart & McClelland, 1985), a highly sophisticated replay of age-old associationist themes, became dominant, but that tale is beyond the scope of this article.

14. This account is based in part on personal recollections, as well as on the published record and two informal records (Jenkins, 1955; Musgrave, 1959), which have been deposited with Special Collections, Library, University of California, San Diego.

"Group for the Study of Verbal Behavior" (GSVB) that met at the fringes of conventions. The GSVB established the early deviations from the "verbal learning" dogma by providing a forum for the discussion of such "revolutionary" topics as free recall (the occurrence of responses without discernible stimuli) and categorical clustering. We can track further developments in four conference at Minnesota in 1955, at Gould House in New York state in 1959 and 1961 (I shall refer to these as Gould1 and Gould2), and at the University of Kentucky at Lexington in 1966. I show first the players at the four conferences:

At the 1955 Minnesota Conference the following presented papers: W. A. Bousfield, C. N. Cofer, D. H. Howes, J. J. Jenkins, L. J. Postman, W. A. Russell, S. Saporta.

At Gould1 (1959) all of the above with the exception of Howes and Saporta were present, together with J. Deese, A. E. Goss, G. Mandler, A. W. Melton, B. S. Musgrave, C. E. Noble, C. E. Osgood, and B. J. Underwood.

At Gould2 (1961) the members of Gould1 came with the exception of Bousfield and Osgood (who had been invited but were unable to attend), and the following were added: R. W. Brown, G. A. Miller, B. B. Murdock, Jr., L. R. Peterson, R. N. Shepard, A. W. Staats, and D. D. Wickens.

At the Kentucky (1966) conference attendees from the previous conferences were: Cofer, Deese, Jenkins, Mandler, Osgood, Postman, and Underwood (who withdrew his paper), together with: S. Asch, W. F. Battig, T. G. Bever, L. E. Bourne, T. R. Dixon, D. E. Dulany, J. A. Fodor, M. Garrett, D. L. Horton, N. F. Johnson, F. H. Kanfer, H. H. Kendler, G. Keppel, P. M. Kjeldergaard, H. Maltzman, D. McNeill, H. Pollio, E. Tulving, and R. K. Young. The list includes people interested in language, some additional ones in the memory/verbal learning area, as well as general behaviorists.

There was obvious continuity among the four conferences. Five (out of seven) speakers at the Minnesota conference were at Gould1, four of them were at Gould2, and Gould2 was designed to be a continuation of Gould1 with only two Gould1 speakers unable to attend. More interesting are the additions that appear in Gould2. With the exception of Staats, they were all significant contributors to the "cognitive" psychology of the next 30 years. Staats was a fundamentalist behaviorist who tried to defend the status quo with a spirited defense of a physicalistic S-R psychology, sprinkled with such pejorative comments about cognitive concepts as "improper method" with "mentalistic overtones" (Cofer & Musgrave, 1963, pp. 272–273).

Seven members of the original Gould1 group were at Kentucky. However, the object of the latter conference was to gather specialists in the area of verbal behavior and to address the relation of their work to general behavior theory (interpreted as S-R theory). That goal was, as we shall see, anachronistic at best.

The early setting for the transformation was set in 1955 at the Minnesota Conference, which is described in Jenkins (1955) and Jenkins & Postman (1957). Apart from the novel interaction with a genuine linguist (Saporta), the conference contents heralded the changes that were about to happen. There was still some preoccupation with the nature and manipulation of associative responses, but these were put in terms of different contexts, norms, and instructions. The influence of the linguistic environment was mirrored in a new interest in understanding grammatical categories and the functions of syntax. And in Davis Howe's presentation there were the first glimmers of the coming mathematical models. But the old traditions of learning lists of nonsense syllables seemed to be well on their way out.

Then in 1958, in discussions with C. N. Cofer, the Office of Naval Research (ONR) which had supported many of the researchers in verbal behavior, offered to fund a conference of some of its grantees and other interested parties.[15] Gould1 met in the fall of 1959. With a couple of notable exceptions (G. A. Miller was invited but unable to attend) the attendees represented the range of interests and ages of the field. Commitments to the status quo ranged from Arthur Melton (the *eminence grise* of verbal behavior) to a trio of young Turks (James Deese, Jenkins, and Mandler). Hard line S-R behaviorism was represented by Albert Goss. The conference proceedings were published in 1961 (Cofer, 1961) and a verbatim record of most of the discussions exists in Musgrave (1959).

The topics in Gould1 were themselves a deviation from the 75-year history of the field since Ebbinghaus initiated the experimental study of verbal learning. There was relatively little about the use of nonsense syllables and much about language and meaning. It can be argued that the major new interests developed by this conference arose out of the repeated consideration of semantics and syntax. The latter in particular was initiated by discussions of Goss's view of sentence production (in the context of an S-R discussion of conceptual schemes). The behaviorist implication that sentences were sequences of stimulus-response chains mediated by verbal labels was strongly attacked and disputed. At one point of the discussion a summary of the syntactical problems discussed was generally accepted: "The occurrence of a new word in a syntactic structure determines its position and form in most other syntactic structures in that language. This constraint cannot be explained in terms of the distribution of response probabilities or contingent probabilities between encoded units" (Cofer, 1961, p. 78). This was a direct rejection of associationist positions and heralded the importance of organizational processes in the next decade. The interest in language was strong enough to generate a request that Jenkins prepare a short bibliography, which was appended to the report volume.

The report of the conference also included a general statement, usually facetiously referred to as the "manifesto," initiated and probably authored by Deese, Jenkins, and Mandler, that supported experimental approaches to an associationist critique. Determining the actual authorship of the "manifesto" illustrates some of the difficulties of accurate historical reporting. In the text (Cofer, 1961), Mandler is given as initiator and author of this statement. The same is true of the transcript of the conference (Musgrave, 1959). Mandler's recollection is that the statement was written in the course of an evening of discussions that involved him, James Deese, and James Jenkins. In a personal communication, Jenkins has recalled a discussion in which it was asserted that the "speculative naming of mental states and entities" would not add to our knowledge (hence the manifesto was sometimes called the "anticognitive manifesto"). The three were then directed to prepare a memorandum but for some reason he (Jenkins) was unable to join Deese and Mandler that evening. He recalls that Mandler and Deese prepared the statement, presented it the next day, and it was generally assented to. Memory is truly constructive, even for its practitioners.

The manifesto did not bear comparison with truly programmatic statements, such as Watson's. It was primarily addressed to problems of the psychology of language. The statement questioned whether "syntactical problems can be adequately handled" by an associative orientation, or whether "conceptual schemas which depend on verbal labels [can] explain the general problem of syntactic structure." In general, any attempts to explain syntactic structures by currently available approaches were rejected. At the same time it attacked the "glib invocation" of mental mechanisms and rejected "facile criticisms and the mere postulation of

15. In contrast to later years when the cold war dominated American science, ONR was prepared to support work unrelated to its military mission.

new processes." (Cofer, 1961, p. 80) The "manifesto" was an attempt to undermine associationist dogma on the one hand, and to quiet the fears of the conservative establishment of theoretical excesses on the other. And even though it was addressed to problems of language and syntax it was understood then and invoked later as a general critique of associative approaches to complex mental phenomena. Jenkins, Deese, and Mandler piped up again in Gould2 when they decried the inadequacy of an attempt by Staats to treat purpose in terms of S-R concepts (Cofer & Musgrave, 1963, p. 290).

In his summary of the Gould1 conference, Cofer noted the following points of relatively novel emphasis: the problem of response integration (acquisition of a response independent of the S-R connection); emphases on one-trial learning; the recognition that nonsense syllables are "complex affair(s)"; the notion that recall is a constructive and guessing process (a point only glossed by Cofer but of great emphasis in later years); and the attempt to assess meaning experimentally (not very successful). At the same time long held assumptions, such that frequency of experience determines associative probabilities or that responses are always acquired in the context of stimuli, were questioned and often put aside. Approaching footsteps of other developments in the coming "revolution" were a single passing reference to Chomsky's *Syntactic structures,* and a mention of the impending and influential book by Miller, Galanter, and Pribram (Chomsky, 1957; Miller, Galanter, & Pribram, 1960).

Apparently, Gould1 generated enough light (and some heat) for ONR to sponsor a follow-up conference. In Gould2, the new orthodoxies had just about arrived. The conference was held in June 1961 and its report was published in 1963 (Cofer & Musgrave, 1963). Apart from the character of the conference participants, mentioned above, not only the topics, but also the flavor of the discussion acknowledged the changed climate. Among the formal papers presented there were an analysis of recognition by a Yale Ph.D.—Bennet Murdock—that was essentially devoid of S-R concepts, a discussion of the acquisition of syntax by Roger Brown (and Colin Fraser) that was both naturalistic and nonbehaviorist, a discussion of purpose by Russell, and an influential paper on immediate memory (not verbal learning!) by Lloyd Peterson. The most "modern" of the presentations was Miller's discussion of Postman's paper on one-trial learning. Significantly it ended with a presentation of Edward Feigenbaum and Herbert Simon's EPAM (Elementary Perceiver and Memorizer) theory as an example of "human cognitive processes." EPAM was one of the earliest (if not the earliest) attempts to develop computer oriented models of human memory, originally presented in 1959 (Feigenbaum, 1959).

In the summary of the conference Wickens noted that the discussion had been divided into two opposing camps—one of these "clearly reads S-R," but he could not identify the other, it was not quite Gestalt or structuralist or functionalist and Wickens ended up calling it "non-S-R, or should it be anti-S-R?" (quoted in Cofer & Musgrave, 1963, p. 374). He characterized the two groups as showing (a) a difference in generating research problems, with the S-R group looking for problems to which their theory can be applied, whereas the Antis were indifferent to current psychological theory; (b) that the S-R group applied "whenever possible the timeworn concepts of their system" whereas the Antis were "receptive to . . . theoretical formulations which are new to psychology . . . ;" and (c) the commitment of the S-R group to physiology, associationism and Pavlovian conditioning, whereas the Antis had no "residual . . . sentiment for this physicalistic way of thought" (pp. 375–376).

As noted above, the goals of the Kentucky conference (funded by the National Science Foundation) were broader than those of the other three—an integration under the aegis of S-R principles. However, it was too late for such an effort—most of the papers were departures from stimulus-response orthodoxy. The proceedings of the conference were published in D. L. Dixon and T. R. Horton (1968). In fact, these papers (and the often fiery disputations at the conference) showed that the result was a contentious confrontation between quasibehaviorist associationism and an assertive attack by the new cognitive practitioners.[16] Some 15 years after the initial signs of change in American psychology it was now possible to say such things as: "Is anybody really willing to assume that the general laws of habits, as developed in simple behavior in lower animals, apply to verbal behavior in man?" (p. 110). Central to the attack was, on the one hand, a rejection of associationism and, on the other, the new distantiation of language from the other traditional verbal behavior concerns. The attack on associationism and S-R approaches in several papers centered on the claim that association was a descriptive term, that associations did not explain anything but were something to be explained.[17] As Asch noted: "It may even be in order to entertain the possibility that it is not necessary, nor perhaps fruitful, to be an associationist in the study of associations" (p. 227). The new approaches to language, fuelled by now by Chomsky's contributions, rejected associationism out of hand and required new logical structures for the study of language. And seven years after Gould1, the rejection of the "glib invocation of 'schemas,' 'structures,' and 'organization'" (to quote a phrase from the 1959 manifesto) had been replaced by principled discussions of these feared concepts.

In their summary of the conference, Dixon and Horton noted that instead of an integration within some general behavior theory the conference produced "significant objections concerning [the] restrictions and adequacy of [S-R theory]" (p. 573). They noted the "heated discussion" and concluded: "[I]t appears that a revolution is certainly in the making" (p. 580). One can argue that the (r)evolution had already taken place. On the other hand, the feeling of many of the cognitive participants that not just behaviorism but also associationism had been defeated was clearly in error. As George Humphrey suggested in 1951, the history of the psychology of thinking consists mainly of an unsuccessful revolt against the doctrine of associationism.

The conferences were followed by the change of the Group for the Study of Verbal Behavior (GSVB) into the organizing group for a new *Journal of Verbal Learning and Verbal Behavior* (Cofer, 1978), but the name of the journal was not changed into the contemporary *Journal of Memory and Language* until 1982.

One might conjecture what would have happened in the field of memory if behaviorism had not dominated American psychology for 40 years. Among the possibilities is that mechanisms like schemas would have been adopted early on from Bartlett and Piaget, that Bousfield's notions about clustering together with Katona's book on organization would have resulted in an early attention to organizational factors, that Kenneth Craik's thoughts about representation would have been attended to, that questions about the structure of syntax and semantics would have been addressed. All of this happened eventually, but some decades later. What followed eventually in the last decade of the twentieth century was a period of consolidation and general quietude in the memory field.

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16. In fact, B. J. Underwood was so angry at the tone of the conference that he withdrew his paper and is not listed as a participant.

17. "Associationism" refers to the broad theoretical assertion that there exists a general mechanism whereby any two events (usually within a specific modality) can be brought into dependent occurrence (usually by such mechanisms as repetition, reinforcement, etc.). An opposition to associationism does not deny the importance of the co-occurrence of two or more mental (or behavioral) events, but it asserts that such co-occurrence is based on principles other than traditional "association." For example, for the defining method of paired-associates learning it has been shown that these "associations" depend on the development of unifying (meaningful) links joining the two items (Mandler, Rabinowitz, & Simon, 1981).

Epilogue

I close with a reminder that psychology, just as many other intellectual endeavors, conforms to Hegel's view of the spiral of thought, with topics recurring repeatedly in the history of a discipline, often at a more sophisticated or developed level. The advent of connectionism has already shown a return of associationism in modern clothing. At the turn of the century we are in the midst of a preoccupation with neurophysiological reduction, a concern that psychology had previously displayed at a periodic cycle of some 40–50 years. The notion of recurring cycles is alien to a recent attempt to see the future of the "cognitive revolution" (Johnson & Erneling, 1997). The mirror that book displayed is cloudy indeed with a variety of different predictions. The most unlikely is the one presented by the keystone chapter of the book in which Jerome Bruner endorses a postmodern view of cognitive science (Bruner, 1997), which is the one position least likely—given its postulates—to foresee any future at all. But psychology has been one of the disciplines that have essentially been unchanged by postmodern attempts (in contrast, for example, to literature and anthropology). The most likely case is that psychology will—as it has in the past—muddle along, encountering other revolutions, whether cognitive or not.

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