Memories of Psychology in the Twenties

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When I began my graduate work at Harvard University in 1926, I had a teaching job at Tufts College (now Tufts University) in two parts of that institution—the College on the Hill in Medford, Massachusetts, and the Medical–Dental School in Boston. Because I had never taught before, this meant the time-consuming preparation of two courses each semester, with very little background for the task. The first year was the hardest.

At Harvard I began my studies with a course in psychophysics under Professor Pratt and a course in motivation with Professor Troland. The textbook in the former course was E. B. Titchener's *Experimental Psychology*. We used the *Student's Manual of Quantitative Experiments*, a book beyond my comprehension; luckily it was only sampled in the course. Professor Pratt put us immediately to work in pairs to demonstrate for ourselves three well-known procedures for studying human sensitivity to stimuli: the methods of *limits*, *constant stimuli*, and *average error*. In this connection he talked to us informally now and then, supplied us with our apparatus, went over our reports, and graded our final examinations. My partner in this enterprise was Paul E. Huston, another first-year man, with an engineering background, and with whom I formed a lasting friendship.

I was happy to receive a B for my performance in this course, because I had little interest in sense-organ function. The main thing that I learned was to respect the scientific rigor of Titchener and the introspectionists in laboratory research. Although convinced by Watson that their "mind" was but a ghost, I was impressed by their procedures for investigating the acuity of our senses.

Professor Troland's course in moti-

vation was a disappointment. Perhaps the world's leading authority at that time on human vision, he was more of a subjectivist than Titchener himself. He talked to us of mind and matter, the conscious and unconscious, and how they were related to each other. He described the nature of sensation and its relation to the central nervous system, with special reference to what happens when we see. The nearest that he came to motivation of the sort I had expected was when he treated *hedonism*—in particular, hedonism "of the past." The reason we play golf this afternoon, said Troland, is the pleasure such behavior gave us in the past under similar conditions. I don't believe he mentioned the law of effect in this connection, but its relevance was apparent to anyone who knew about Professor Thorndike's cats in puzzle boxes—studies of concern to me at Tufts in my comparative psychology course.

Actually I was teaching two Tufts courses in that term. One was an introduction to experimental psychology, similar to the one that I had taken in the preceding year. That course had included eight experiments on human vision—the blind spot, color zones, color mixture, after-images, and the like. Then came touch and the related senses, stimulated with pig's bristles, needles, blunted nails, and immersion of the index fingers in water, hot and cold. The sense of equilibrium was investigated with a swivel chair, causing nausea for a few of us. After this there were a few experiments on perception, word association, imagery, and emotion—that is, pleasantness and unpleasantness. Wearing blindfolds, we drew lines of a requested length (6 inches, for example) on sheets of paper, while we imagined pleasant or unpleasant situations. Pleasant ones got the longer lines.

In my 1926 version of this course, I used a smaller number of experiments and preceded each one with a talk that aimed to vitalize the topic, with what

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Excerpts from an account of the author's early years of graduate study in psychology and his concurrent teaching of the discipline to undergraduates.

effect I cannot say. Also, near the course's end, I tried to demonstrate hypnosis to the class. The girl who volunteered to be my subject was, however, quite resistant. I had concluded that the demonstration was a failure, when I suddenly discovered that another girl had "gone to sleep"—was under my control. In the following term, I used her in a demonstration of hypnosis for the students in my Boston course.

In my other course on Medford Hillside, I used John Watson's textbook, *Behavior—An Introduction to Comparative Psychology*. I drew my lectures from another book—Margaret Floy Washburn's *The Animal Mind*. I didn't believe that animals had minds, or that human beings did, but the book was full of information on experiments with all the species, regardless of their mental life (which I could leave out in my lectures). I made up charts with which I filled the blackboard, showing the presence or absence of different sensory functions throughout the animal kingdom. It was a tiresome business for my students and for me.

In the year that followed, I tried to brighten up this course with experiments on animal learning. One of my pupils, Jester Hairston, bought some waltzing mice at a Boston pet shop in order to study their ability to learn an elevated wooden maze. These animals have an ear condition that forces them to move in figure-eights, rather than straight ahead. Jester's maze paths were so narrow that the animals could not behave as usual. The result was a picture of frustration, for both Jester and the mice.

My own experiment for the class had no better outcome. I bought a small green turtle at the shop where Jester bought his mice and tried to teach it to "run" through the open doors of a three-compartment maze that I constructed on the floor of a copper wash-boiler. As a reward for running I used everything I could think of—darkness, water, food, what-have-you—all to no avail. I concluded that it was hard to teach a tortoise; a hare would have done much better.

In my second term at Harvard, I took Professor Boring's course in History and

Systems of Psychology and Professor Pratt's Aesthetics. The history course was one of four that I would have with our department head, and it had a great effect upon me. At the very first meeting of the class, I realized that I would work as I had never worked before. I was staggered by the suggested readings on the blackboard—three histories of psychology (Otto Klemm, G. S. Brett, and M. Dessoir) and a book of excerpts from early classics (chosen by Benjamin Rand, our librarian on the floor below). I marvelled at Professor Boring's outline of the work ahead—the "family tree" that he constructed for us. We had no textbook of our own to lean upon; Professor Boring was writing it at the time. The illustration at right is the outline of our course as I recorded it in my notes on February 9, 1927.

These names, half of which were strangers to me, were only the beginning. The suggested reading, some of it in French and German, was beyond my reach. So I took notes as fast as I could write and repaired them later with the help of friends. At the end of the term I reduced them to an outline that served as a mnemonic system. (The only time I ever cheated on an examination was when I peeked at it in the lavatory to give myself a starting cue.) Professor Boring gave me a B-minus in the course. A lower grade would have meant disaster.

In the aesthetics course with Carroll Pratt, I earned a B-plus and enjoyed myself. We read H. S. Langfeld's *Aesthetic Attitude*, which recognized behavior, and parts of Santayana's *Sense of Beauty*, a delightful treatise, made more so for me when he distinguished between "literary psychology" and "scientific psychology," calling the latter "the study of behavior."

Professor Pratt, himself, was a colorful lecturer, with a resonant speaking voice. Note-taking made no problem for me because we were examined mainly on our reading. Also, he provided colorful demonstrations now and then to make his points. One of these was written on the blackboard, illustrating Titchener's "context theory" of meaning:

Pas de leur Rhone que nous.

This “French” may take on meaning when spoken rapidly aloud—when auditory *context* is added to a visual *core*.

On the afternoon of the day that I attended my first class in aesthetics, I began to teach my first course in general psychology to the Tufts premedical and pre-dental students assembled in The Barn on Huntington Avenue in Boston. This course was part of a short-cut curriculum that permitted high school graduates to prepare for medicine or dentistry within a 2-year period of study.

My classroom had once been part of an actual barn, and its homely origin could sometimes be detected by the nose as well as by the eye. My pupils seemed well-suited to this atmosphere. Before my lecture the air was occasionally filled with flying objects, particularly chalk, and the teacher who used the blackboard in his talk did so at his peril. It was said that Leonard Carmichael, my predecessor,

had been compelled to threaten a failing grade for any marksman whom he caught.

The course that I constructed for these sons of Aesculapius is difficult to describe. I don’t recall the textbook that I used; I had never taken an introductory course myself. I had to build a set of lectures based on my reading of John Watson, what I had learned at Tufts, and the courses I was taking or had taken in my Harvard classes. The anatomy and physiology that I had acquired were useless, because the Medical School had courses on these subjects.

I began with Aristotle and his soul—the nutritive, the sensitive, and the rational. I explained his views on personality as revealed in the shapes of noses. I talked about the problem of the Body and the Mind, which I illustrated on the blackboard with two circles. I told them what psychology was *not*—palmistry, numerology, and phrenology must have taken up a lecture. I noted that psychol-

ogy was useful in practical affairs—in writing advertising, for example, and in selecting men and women for various occupations. I listed the conditions of *attention* (the miller never hears his mill-wheel until it stops); I defined the IQ (MA/CA); and I distinguished between idiots, imbeciles, and morons. I discussed the senses very lightly, but gave a lot of time to visual illusions; and I described the ways in which René Descartes and Bishop Berkeley treated the third dimension of space—how we could judge the distance of objects from us. I talked of imagery, with special reference to Francis Galton's Breakfast Table Questionnaire—"Consider carefully the picture that arises before your mind's eye. . . . Is the image fairly dim or fairly clear?"

I told my class about Ebbinghaus's nonsense syllables—*zik, wup, jiz*, et cetera—and their value for our science; about the Hampton Court Maze and its numerous descendants in the animal laboratory; and about Professor Thorndike's puzzle-box experiments with cats. I talked about the James-Lange theory of emotion and John Watson's studies of fear, rage, and love in babies. I discussed astronomy and the study of reaction times; hypnosis (with a demonstration); hallucinations and delusions; neurosis and psychosis. In my final lecture I was able to relate this to the moral that we should all be tolerant of one another. At the end of the term I was invited to the Pre-Med Banquet.

Among my Harvard teachers, my behavioristic bent did not find much support. Professor Boring gave a lecture on the topic at the end of his history and systems course, almost as if it were an afterthought. Professor Troland pointed out a serious flaw in Watson's theory of the learning process; and I don't believe the subject was even mentioned by Professor Pratt.

This did not discourage me from studying the position as thoroughly as I could. I read all of Watson's writings as well as articles by others, including K. S. Lashley, Max Meyer, W. S. Hunter, and A. P. Weiss—all behaviorists of a sort. I read Dorsey's *Why We Behave Like Human Beings*, a popular account, and Ro-

back's *Behaviorism and Psychology*, according to which behaviorism was neither new, true, nor accepted in Great Britain. I read, and tried to understand, philosophers on the subject, pro and con; and I argued for the cause with other graduate students when the opportunity arose. One or two of the latter listened to me now and then, but the prevailing student viewpoint was in a different tradition—one that had its roots in British associationism and German physiology and came to the United States by way of Titchener, an Englishman, at Cornell University, and was represented in the teachings of my principal mentor, Professor E. G. Boring.

Besides the regular academic fare at Harvard in 1926–1927, there were numerous enrichments. In the 3-month period of which I have a record, at our Wednesday afternoon colloquiums in the department or at special lectures, I heard Alfred Adler, from Vienna, speak on "felt inferiority"; A. V. Hill, from London, on the "physiology of work"; Karl S. Lashley, Watson's pupil, on "brain mechanisms and intelligence"; Walter Cannon, from the Harvard Medical School, on "bodily changes in emotion"; and several others, all distinguished figures of the day.

Titchener himself came to Harvard that year, for a meeting of the Experimental Psychologists, a distinguished group that he had played a part in organizing several years before, and membership in which was quite restricted. Nonmember faculty and graduate students in our department were invited to attend the program, which was conducted in the open hallway on the third floor of our building. I recall the moment clearly when, with all of us assembled, "T" came puffing up the stairs, followed closely by a Japanese student from Cornell with a box of his (Titchener's) cigars. The meeting could then begin.

My reading on infant emotions in Watson's textbooks encouraged me to write to Watson himself in the winter of 1926, telling him of my interest in the experimental study of *adult* human emotions in an objective fashion and asking if he

knew of relevant research. He was kind enough to answer:

I don't believe anybody has ever made the attempt suggested in your letter of February 23rd. I once took a medical man and gave him two years' training in behavioristic psychology to get him to do just that very thing. He then went over to the Government Hospital for the Insane and became deeply engrossed in Psychoanalysis. His name is Dr. N. D. C. Lewis. So far he hasn't come out with any kind of textbook on Psychiatry.

I do think that to do this job requires medical education to forestall criticism if for nothing else.

In the following year, when I went to Professor Boring and told him of my interest in studying within this field, he said that he would be glad to sponsor such research if I would state the problem for him. Before I figured out exactly what he meant, I had turned to other matters. The kind of study that I had in mind was carried out successfully by another man, Carney Landis, who would one day be my colleague at Columbia.

An important event for my career occurred in the fall of 1927, when Walter Hunter came to Harvard from Clark University to teach a graduate course in animal behavior. This course, and the seminar that followed, gave an impetus and direction to my life that had before been lacking. Professor Hunter occupied a chair at Clark, where Professor Boring had been a teacher and Professor Pratt had been a graduate student. He had a PhD from the University of Chicago, as had John Watson; he had taught at Texas and at Kansas before he came to Clark; and, most significantly for me, he was a behaviorist, as he informed us in his opening lecture.

I still retain a vivid picture of that hour. Professor Hunter was a handsome man of 38 years, of medium height, with a black mustache. He was neatly dressed and informal in his bearing as he stood at the end of the dais in our classroom. He placed his gray felt hat upon the counter, but did not remove his pearl-gray gloves at any time throughout the lecture. It was as if he hadn't decided to stay throughout the entire 50-minute hour. (Years later, at a meeting of the Eastern Psychological Association, I asked for and received an explanation: "Didn't you

know why I did that? I had poison ivy on both hands!")

When he identified himself as a behaviorist, I was filled with glee. Here was a psychologist, one of some distinction, whose theoretical bias was the same as mine; I was sure that I would profit from the course that he then went on to outline for us. For the remainder of that hour and at later meetings of the class, he spoke informally and clearly on each topic, with little if any appeal to notes. I must have taken very few myself, because I have no record of them in my files. Also, he talked about experiments in progress, something that our Harvard teachers almost never did.

Several of my friends were enrolled in this course. One was Jim Coronios, new that year at Harvard, having just received his Tufts degree. Another was Paul Huston, my partner in psychophysics the year before. The three of us were so impressed by Hunter that, later in the course, we volunteered to carry out a project he suggested. We undertook to train three white rats to run an elevated T-maze, the units of which were brought to us from Clark by our professor.

Our "experiment" was not a great success. We lost one rat because of a stomach disorder, probably brought on by the diet of bread and milk and Quaker Oats, and we barely saved another with a dose of castor oil; the eight-unit maze was quite unsteady on the floor of our tiny attic room in Emerson Hall; and the presence of student observers from time to time, some of them smoking cigarettes, probably did not facilitate the learning process. Even when their errors seemed to be eliminated and the times were short from start to finish of each run, our subjects were upset when we took them to Professor Hunter's classroom to demonstrate their skill. Their performance, though convincing, was not as smooth as we would have liked.

Professor Hunter classified the problems of behavior in terms of four main "functions": sense-organ functions, unlearned (reflex) functions, learned functions (habits), and representative functions ("symbolic processes," such as

language). He dealt with each of these to some extent in his lectures, but his main interest at that time was in the question of *representation* at the subhuman level—do animals possess symbolic functions?

Many early students of animal behavior had no difficulty in assuming the existence of ideas, images, or thoughts, even language, in certain species. Everyday observation seemed to show that such existed. The rhythmic twitching of the legs, the muffled bark, and the rolling eyes of a sleeping dog; the vocalizing of a hen “in search of” her chicks—these were taken as evidence of dream-images, memory ideas, and the like. Later on, however, as observers used more parsimony in their interpretations, it was argued that the existence of any such process representing absent stimulation must be demonstrated by experimental methods.

Hunter was in the vanguard of this research. In 1912 he had received his PhD at the University of Chicago for his development of a method, the “delayed reaction,” for studying the problem in the laboratory; he had used this new technique with rats, raccoons, dogs, and children, obtaining different results with different species. Raccoons were more successful than rats, and children outdid raccoons in their performance, for example.

Soon after classes had begun that fall at Harvard, probably in Professor Boring’s course on Association and Determination (as near as he came to present-day learning and motivation), I met a new addition to our student group, with whom I quickly found a common cause. The student’s name was Skinner; he had just returned from Paris; he had wanted to become a writer; and his reading of John Watson had been partly instrumental in leading him back to school. These things recommended him to me.

Burrhus Frederic Skinner was his full name, although he didn’t use the Burrhus and was usually called Fred. Burrhus was his mother’s maiden name. When he told me that a classmate at Hamilton College had dubbed him Burrhus de Beerhus, I was impressed, not because of the ref-

erence to beer, but because I pictured a knight in armor on a horse, carrying a pennon on his lance. I began to use the given name shortly after that, partly in fun and partly because we both were Freds, a source of occasional confusion. We both enrolled in Professor Hunter’s course that fall, a seminar on animal experimentation.

Hunter’s teachings of the year before made me want to follow in his footsteps, starting with a study of delayed reaction. In his classic treatment of this method, he had tested animals in a situation where they would be successful in reaching a food reward only if they went directly to that one of three doors which had been lighted briefly at a given time before; they had to make the correct response without an outside cue to guide them. Success with the problem was said to depend upon the animal’s possession of an inner process, a function that *represented* the absent light—a function like that which *language* might supply.

In a 1917 study with his 16-month-old daughter, in a two-choice situation, the child’s success with the problem raised the question of the earliest age at which positive results with children could be expected. Would it coincide with the emergence of language, for example? With Hunter’s approval, I decided to explore the literature on this topic, with an eye to following up his research with still younger children, those not yet vocalizing.

By February 12th of 1929, I had reviewed the literature on delayed reaction and was ready for research. On that day I wrote to Hunter:

I have an appointment tomorrow morning with Supt. Jones of the New England Home for Little Wanderers, at 160 South Huntington Avenue, Roxbury, after which I’ll let you know the outcome. Children of suitable ages are quartered there. . . .

If I cannot get an experimental room to myself at the Home, would it not be satisfactory to use a screen, during periods of comparative quiet, in one of the larger rooms? Would the use of an attendant or assistant in helping to handle the very young be against the rules of individual research?

My proposal was approved, the Home gave its permission, and a pilot study was undertaken “with the purpose of finding out the earliest age at which the human

infant shows behavior having the characteristics specified by Hunter as constitutive of the non-oriented delayed response. . . ." In other words, how old did an infant have to be in order to recall in which of two familiar places a toy or cookie had been hidden, when some amount of time had elapsed between the hiding and the choice—time in which it was impossible for the infant to keep "pointed" toward the hiding place.

On Monday and Wednesday afternoons in February and March of 1929, with the help of an assistant (my hypnotic subject at the Barn, now a graduate student at Harvard), I developed a procedure and collected data from six children at the Home in order to design a way of dealing with two others—BW, a 1-year-old boy, and VJ, a 9-month-old girl.

Our technique was simple. With the infant seated at or near the end of its crib, a stimulus-object (a rattle, ball, or nipple, for example) was presented, just out of reach, until there was a reaction to it—looking, reaching, crawling towards it. Then the toy was dropped into one of two little boxes, also out of reach, and a delay began, timed with a stop-watch. Disorientation was achieved during short delays by lifting the child from its crib and turning it about; with long delays it might be carried to the window or distracted in some other manner.

With the help of my assistant, 228 tests were made with BW and 198 with VJ. In my 26-page report to Hunter, I cautiously concluded that BW "showed rather definite ability to make successful delayed reactions [with] delays of thirty seconds in duration; that the subject VJ . . . showed some indication of the capacity to make delays of nine-second duration. There is insufficient evidence in each case for a more positive assertion at this time." I concluded with a list of mistakes in our technique and some possible improvements in further research.

I never solved this problem. Other, more important, matters intervened, including my discovery that a graduate student, C. N. Allen, at Columbia, working with Professor Poffenberger, had also started an attack upon this problem, with

better facilities and a larger number of available subjects than did I.

I hated to leave the Home for Little Wanderers. I had become especially attached to little Bernard—BW in my protocols. The presence of two lower incisors only, together with an upright wisp of yellow hair, gave him a droll, Happy Hooligan appearance when he smiled, which was often. One day he was asleep in his crib when we arrived, but his eyes were open. I bent over till my face was in the plane with his and smiled. He didn't awaken, but gave me half a smile in answer. On another day, just before our testing started, I let him play for a moment with a rattle that I planned to use in the experiment. He handled it a moment, then placed it in one of the two boxes and raised his arms for me to pick him up for the delay!

Walter Hunter's influence on my work continued into the thirties and determined the nature of my doctoral research—a further study of "symbolic processes," this time with white rats as subjects and a "temporal maze" of his invention as my apparatus. My friendship with Burrhus Skinner also grew during those years and well beyond; he has described it in *The Shaping of a Behaviorist* (1979). But these are matters of another decade.

In 1929 I left my instructorship at Tufts to become a teaching assistant and tutor at Harvard. Also in that year I became an Associate Member of the American Psychological Association—an organization of 549 full members, according to my count, and 345 associates. On almost every page of my little 1929 Yearbook is the name of someone whom I knew or knew about. There were no subdivisions of the Association then, although there was a Clinical Section with some autonomy of function. In 1991 the membership was in the thousands, there were 40 different divisions, and the number of *new* members and associates was 533 for the month of January alone. I prefer to think about psychology in the twenties!

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