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KENNETH WARTINBEE SPENCE  
*1907—1967*

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*A Biographical Memoir by*  
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*Biographical Memoir*

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# KENNETH WARTINBEE SPENCE

*May 6, 1907–January 12, 1967*

BY ABRAM AMSEL

**I**N 1964 WHEN KENNETH SPENCE moved from the University of Iowa to the University of Texas he must have thought he was embarking on a long, new phase of his career. His parents were both long-lived and he was then only in his middle fifties. Three years later, on January 12, 1967, at the age of 59 he died of cancer, ending a distinguished career as a theorist, experimenter, and teacher, and toward the end of his life, as an editor in collaboration with his wife, Janet Taylor Spence.<sup>1</sup>

## PERSONAL HISTORY

Spence was born on May 6, 1907, in Chicago, where his father was an electrical engineer. The family moved to Montreal when he was a young child and Kenneth spent his youth and adolescence there. At West Hill High School in an area of Montreal called Notre Dame de Grace he was active in basketball, track, and tennis. Later at McGill University he injured his back during track competition and, as part of his therapy and convalescence, he went to live with his grandmother in LaCross, Wisconsin. He attended LaCross Teachers College and majored in physical education. There he met and married Isabel Temte. The couple had two

children, Shirley Ann Spence Pumroy and William James Spence.

He returned to McGill, switched his major to psychology, and took his B.A. in 1929 and a master's degree in 1930. (As a personal aside, sixteen years later I completed a master's degree at McGill under the supervision of Robert B. Malmö, a Yale Ph.D. who knew about Spence's work and his association with Clark L. Hull. I took a seminar with Chester E. Kellogg, who had been Spence's graduate advisor and was very proud of it. For these reasons I found myself heading to Iowa City to study and work with Spence.)

From McGill Spence went to Yale University, where he was a research assistant in the laboratory of Robert M. Yerkes. Under Yerkes' direction he completed a dissertation on visual acuity in the chimpanzee and received the Ph.D. degree in 1933. As Hilgard reports,<sup>2</sup> during his years at Yale Spence began an intellectual association with Clark L. Hull that was, in part at least, a product of a graduate course in experimental psychology that Hilgard was then teaching. With Walter Shipley he performed an experimental test of one of Hull's deductions concerning the difficulty of blind alleys in maze learning in the rat. This led to other papers on maze learning which, as Hilgard writes, Spence published on the side while doing his dissertation on visual acuity in the chimpanzee. These papers revealed Spence's great promise at designing experiments relative to theory, and this feature of Spence's style became the hallmark of his theoretical-experimental work. Indeed, students who worked with Spence at Iowa roughly from 1940 to 1964 usually referred to their Ph.D. degrees as being in theoretical-experimental psychology.

From Yale Spence went on a National Research Council fellowship to the Yale Laboratories of Primate Biology at Orange Park, Florida, where he spent four years and did

his seminal work on discrimination learning in the chimpanzee, about which more later. In 1937 he was offered a one-year assistant professorship at the University of Virginia to fill in for someone on leave. The next year he went to the State University of Iowa, where he spent twenty-six years, twenty-two as department head, before moving to the University of Texas in 1964.

#### PROFESSIONAL HISTORY

Kenneth Spence was one of the major learning theorists of his time. Although his name and Hull's appeared together on a paper just once, in a methodological article in 1938 dealing with the differences between correction and non-correction procedures in maze learning, their names are usually linked to identify the most influential neobehavioristic theory of the 1940s and 1950s that encompassed conditioning, learning, and motivation. Spence's contribution to this theory was explicitly acknowledged by Hull in the preface to *Principles of Behavior*,<sup>3</sup> but it can also be inferred from the level of correspondence maintained by the two men. The volume, the time span, and the theoretical content of this correspondence make it, from an historical point of view, perhaps the most extensive and important in the history of the psychology of learning.<sup>4</sup> One can, however, begin to appreciate Spence's independent contribution to learning theory simply by reviewing the thirteen papers he published in the *Psychological Review* between 1936 and 1966.

Spence's contributions fall into three major categories: (1) learning and motivation theory, (2) the experimental psychology of learning and motivation, and (3) methodology and philosophy of science. (In some of the writings on methodology and philosophy of science Gustav Bergmann was a major collaborator.) In this latter area one of Spence's

contributions was to help clarify for all of us the role in psychology of operationism and the nature of theory construction, and to point out the difficulties that exist in the formulation of psychological theories. Among his insights was that psychologists, unlike physical scientists, are faced with the necessity of constructing theories even at the level of trying to establish the basic laws of behavior; because of the nature of their observations and the fact that they do not work in closed systems, psychologists cannot in most cases begin with simple empirically derived generalizations.

In the introductory portion of his Silliman lectures, Spence (1956) made clear his position on psychology as a scientific discipline, including other than methodological factors that impeded its too-slow progress. This point of view, offered in classrooms and privately on many occasions, was that these impediments lay within the discipline of psychology itself—in the holists and the humanists, particularly, who ranted against artificial laboratory situations, and in the practitioners (the clinicians, mainly) who were beginning to dominate the American Psychological Association and were generally disdainful of theoretical-experimental psychology and paid little if any attention to its findings.

Spence's contributions to learning theory, apart from his collaboration in the Hull-Spence system, were of two kinds. His first contribution was as a systematist, as a commentator on and interpreter of the characteristics of the theories and systems of others. His chapter in the edited volume of Stone (1951) is an example of this skill, as is his contribution to the Stevens *Handbook of Experimental Psychology* (1951). Edward Tolman, whose theorizing in animal learning and motivation provided at the time the major alternative to the Hull-Spence position, is reported to have said he never fully comprehended the structure of his theory until he saw Spence's analysis of it.

The second, and Spence's main contribution, was to the body of theory itself, beginning with the famous early papers on discrimination learning. These papers included the derivation of transposition in discrimination learning from stimulus-response gradients of excitation and inhibition, and the derivation of seemingly sudden solutions to discrimination problems from principles of continuity in learning. As is the case in the work of so many distinguished scientists, this early work of Spence's, a product of his time at the Orange Park Primate Laboratories, was, as we shall see, the focus of much of the research in the Iowa laboratory in the 1940s, and it will remain perhaps his most influential.

Spence's more formal, theoretical contributions to the study of learning and motivation are summarized in his Silliman lectures at Yale University, published as *Behavior Theory and Conditioning* (1956). They reveal a substantial difference between himself and Hull in theoretical style. As Kendler<sup>5</sup> points out:

In essence . . . Spence's formulation, as compared to Hull's, shifted in the direction of paying more attention to the behavior of the animal in interpreting the theoretical consequence of a given experimental variable. This difference seems inevitable if it is remembered that Hull was resolute in his determination to present his theory in a formal manner. No doubt this methodological commitment encouraged him to select postulates that could be stated simply and neatly. Spence, in contrast, more sensitive to the fine nuances of experimental data and more aware of the provisional nature of psychological theorizing, did not feel any compulsion to offer anything resembling a final solution. His aspirations were in touch with the realities of his subject matter and within these constraints he worked to interpret available data and predict new findings. His pragmatic approach to theorizing is brilliantly revealed in the concluding chapter of *Behavior Theory and Conditioning* in which he demonstrated how fundamental principles of conditioning can be applied profitably to the analysis of complex learning tasks.

This difference between Spence and Hull in pragmatism

of approach was revealed in another way. Spence was not, after Hull's death, vigorous in pursuit of Hull's later interests in the quantification of reaction potential; like Hull, however, he did continue to try to reduce learning phenomena to mathematical equations (1952, 1954). In these attempts he was, in substance if not in exact form, in tune with developments in mathematical psychology which, from about 1950, were given new impetus by Estes at Indiana and by Bush and Mosteller at Harvard. A quarter century after Spence's death a genuine mathematical psychology of learning of any generality seems still in the (perhaps distant) future.

Like so many scientists of his caliber and standing, Spence's published work does not reflect all of his scientific interests. Many of the unpublished ones were covered in his seminars, and in many cases they were the source of Ph.D. dissertation topics for his students. One of Spence's interests that at first surprised some of his students was his attempt at a neobehavioristic interpretation of perception. As we thought about it, however, we saw that this was a topic he carried over from his early work on vision and on theories of discrimination learning in the chimpanzee. It re-emerged at Iowa in the 1940s in the work surrounding the two major theoretical issues, to which I have already alluded, that Spence brought to Iowa from his work at the Orange Park laboratories. (Indeed, Spence and his students at Iowa, and not Hull and his students at Yale, were the protagonists on the S-R-behaviorist side against Tolman and his followers at the University of California, Berkeley, on the cognitive-behaviorist side in these and other issues, for example, the controversy surrounding latent learning.)

The first issue was whether discrimination learning was relational or specific. This addressed the role of transposition raised by a number of American psychologists in the

first two decades of this century, but usually attributed to the Gestalt psychologists, particularly Kohler,<sup>6</sup> who showed that in discriminating between stimuli on a dimension, the hen, chimpanzee, and human child appear to respond to the relational aspect of the stimuli. The animal learns, according to this view, to respond not to one specific stimulus and not to another (large versus small circle, dark versus light shade of gray), but to the relation between them (to the larger or the darker of two stimuli). Spence's (1937) famous nondirectional S-R analysis of transposition was a tour de force whose power continues to this day to be recognized in psychological theories of discrimination learning.

The second issue was whether discrimination learning was a gradual process or a sudden event. This issue divided the insight theorists at Berkeley and the Hull-Spence view that differences in habit strength accrued gradually through successive reinforcement and nonreinforcement of responses. To argue this point Spence (1940) invented the presolution phase of discrimination learning, a phase during which the subject was exposed to both of the discriminative stimuli, but only for a number of trials too small for learning to be apparent behaviorally. The presolution phase was followed by a phase of reversal of the positive (reinforced) and negative (nonreinforced) stimuli and this phase was carried to the point of clear-cut discrimination learning. These experiments showed that, even without any apparent learning, presolution discrimination training retarded solution in the reversal phase, proving that excitatory and inhibitory potentials had been building up to the two stimuli in the presolution phase even though these were subthreshold for response evocation and were not reflected in discriminative behavior. According to Spence the insight pro-

ponents, Krechevsky<sup>7</sup> in particular, would not make this prediction.

In addressing both these issues Spence emphasized what he called the receptor-exposure act. This emphasis was an example of Spence's attention to the non-obvious specifics of the experimental arrangements that were employed (a feature of Spence's style that, as Kendler<sup>8</sup> pointed out, differed greatly from Hull's greater interest in the more formal, abstract aspects of theorizing). Spence's argument was that the apparent rapidity with which rats learn a discrimination on a Lashley jumping stand will depend on where the stimuli are placed, as they tend to look at where they are jumping. Because they jump to land on a platform the two stimuli between which they must choose should be placed near the bottom of the stimulus panels they face rather than higher up—a small point, but critical to how quickly the discrimination is learned and how sudden the learning seems to be. The receptor-exposure idea was an element in Spence's never-published theory of perception.

In light of this interest in perception and its relation to discrimination in animals, Spence always insisted that his theory of discrimination learning was a theory about inarticulate organisms and should not be applied directly to humans (sometimes with an aside that perhaps college freshmen, frequently the subjects in psychological research, might be an exception). He was explicit in stating that, as children gained symbolic skills and language, new factors arose. Spence was pragmatic and cautious and did not make the claim that the Hull-Spence (in this case, the Spence) theory could with minor additions be extended to explain these skills and behaviors. A dissertation by Margaret Kuenne,<sup>9</sup> directed by Spence, relating language to transposition in young children, addressed these particular concerns, as did a body of later work by Tracy and Howard Kendler.

If we think of Spence's research career as spanning about a thirty-year period (apart from his early work in maze learning as a graduate student at Yale), it can be divided into two major phases. The first phase, beginning in the middle 1930s and ending about 1950, is marked by the work described above on discrimination learning in the chimpanzee and later in the rat and by some preoccupation with philosophical-methodological matters. From about 1950 on, almost all of Spence's own research papers involved human subjects and involved classical (Pavlovian) eyeblink conditioning. During this period much of the other research from the Iowa laboratory was on instrumental learning in the rat and consisted of master's theses and doctoral dissertations that Spence supervised, much of it on interactions between motivation and reinforcement. (To my knowledge Spence's name never appeared as a co-author on a journal article based on a student's Ph.D. dissertation, and I believe this was also generally true of articles based on master's theses. The student was frequently given the problem to work on or it was suggested by Spence in his classes and seminars. He gave advice and helped with the writing, but the publications belonged to the student.)

The eyeblink conditioning experiment employed by Spence in much of his own later work was for him the closest he could come to a "psychological vacuum" for teasing out the most fundamental principles of association and the relative roles of habit and drive in simple learning. While I don't remember his ever having said this in just these terms, some of the very last work he did with this procedure supports this assertion. Spence demonstrated with great clarity that human eyeblink conditioning data could be "contaminated" by cognitive factors (a little air creeping into the vacuum) and that such factors accounted for the greater extinction rates in Pavlovian conditioning in humans than in animals.

He and his students showed that if human subjects were told a cover story to mask the true purpose of the conditioning procedure, the rate of extinction, the decline in responding when the unconditioned stimulus was omitted, was very much slower than when the subject was aware of the experimental sequence and could detect the transition from reinforced acquisition to nonreinforced extinction. The vacuum under these masked conditions was restored and one presumably got closer to revealing the most fundamental laws of association.

Kenneth Spence did not live to see the full flowering of the cognitive revolution in psychology, which can be dated from about 1960, and his stance vis-à-vis the cognitivists is not well understood. Influenced by Pavlov and by the early (1913-19) brand of Watsonian behaviorism, Spence was not a thoroughgoing behaviorist in the mold of the later, more doctrinaire Watson of 1924<sup>10</sup> or of the post-1950 B. F. Skinner.<sup>11</sup> Spence's position, like Hull's and Tolman's before him, is now characterized as a form of neobehaviorism. (He was nevertheless a behaviorist in every methodological sense.) Like other neobehaviorists he did not take the more extreme positivistic stance of the later Skinner—of avoiding the use of empirical constructs defined operationally. This is particularly clear in the fact that, as we have seen, a substantial part of his work, particularly in the 1950s, had as its major purpose the separation of habit and motivational or drive factors in the eyeblink conditioning experiment. Some of his work involved the concept of level of anxiety, defined by a subset of items taken from the Minnesota Multiphasic Personality Inventory that became known as the Manifest Anxiety Scale.<sup>12</sup> This work at the University of Iowa was in collaboration with I. E. Farber, Janet A. Taylor (later Janet Taylor Spence), and others. Anxiety defined in this way was shown on the one hand to have generalized

drive properties to facilitate simple (eyeblink) conditioning, but on the other hand to have disruptive properties to retard or interfere with more complex (e.g., paired-associate, multiple-unit maze) learning, and a neat theory was developed to account for this apparent paradox.

Spence's work is still among the best of its kind, and is frequently cited, though not as often as in the six-year period from 1962 to 1967 (the year he died), when he was the most cited psychologist in a survey of fourteen journals judged to be the most prestigious in the field.<sup>13</sup>

In any account of his intellectual history one must not overlook, and cannot overestimate, another facet of Kenneth Spence's contribution—the seventy-five doctoral students who came out of his laboratories, a large number of whom have gone on to make significant contributions of their own.

#### TEACHING

As head of the Department of Psychology at Iowa, which he became in 1942 following the untimely death of John A. McGeoch, Spence inherited a relatively small group of colleagues with diverse interests. Carl Seashore, who had been dean of the graduate school, maintained an office in the department, and one of each of several specialties in psychology were represented: history and systems, social psychology, psychoacoustics, statistics and measurement, clinical psychology, and conditioning and learning. However, after a few years, at least by 1946 when I was there as a student, Spence's interests in the theoretical-experimental psychology of conditioning and learning and motivation dominated the department, particularly the graduate curriculum.

Spence took his own teaching very seriously. His lecture

notes were meticulously prepared and were updated from year to year. In the years I was at Iowa he taught a two-semester course in learning that was taken by every first-year student, regardless of major area of interest. During each spring semester he offered a graduate seminar on special topics in learning that reflected his major interest of the moment. And in the summer sessions he alternated courses in theories of learning and theories of motivation. Although he was regarded by outsiders as very doctrinaire, a vigorous proponent of the Hull-Spence position, his students knew that, particularly in his seminars and in his summer courses, he covered the various theories of learning and motivation other than Hull's and his own in great detail and with great insight. He took fierce pride in the graduate education provided at Iowa. I have often told the following story to illustrate how Spence felt about the Iowa education.

At one of the first meetings of the newly formed Psychonomic Society (I think in Chicago in 1961), Kenneth said to me, "I hear you have reviewed Mowrer's book." (Spence had some theoretical differences with O. H. Mowrer.) When I admitted I had done such a thing, Spence added accusingly, "And I hear you gave it a favorable review." I thought my review had on balance been favorable, so feeling trapped and fighting for time, I asked him if he would actually read the review. He said he would and, breathing relief, I said I would send him a copy. Scene two is some months later at a spring meeting, and I asked Kenneth, "Did you read my review of Mowrer's book?" Yes, he had. "And did you think it was a favorable review?" He gave me one of his penetrating looks and said, "No, I didn't, but who but an Iowa graduate would have known it was not favorable?"

## HONORS

Kenneth Spence was the recipient of many honors starting in his years as a graduate student at McGill University when he was awarded the Prince of Wales Gold Medal in Mental Sciences and the Governor General's Medal for Research. Later he was elected to the Society of Experimental Psychologists and received its Howard Crosby Warren Medal for outstanding research in psychology and was elected to the National Academy of Sciences. He received the Distinguished Scientific Contribution Award of the American Psychological Association the first year it was awarded. (The story goes that this APA award was created, in part at least, to honor Spence after he had been urged to run for its presidency four or five times and, not having been elected, refused to run again.) But perhaps the honor Spence cherished most was his invitation to deliver the Silliman lectures at Yale University. He is the only psychologist ever selected for this honor.

## NOTES

1. This memoir owes much to two obituaries. One by E. R. Hilgard in *Amer. J. Psych.* 80:314-18 (1967) and one by H. H. Kendler in *Psych. Rev.* 74:335-41 (1967).
2. E. R. Hilgard. Kenneth Wartinbee Spence. *Amer. J. Psych.* 80:315 (1967).
3. C. L. Hull. *Principles of Behavior*. New York: Appleton-Century-Crofts (1943).
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5. H. H. Kendler. Kenneth W. Spence. *Psych. Rev.* 74:340 (1967).
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