NATIONAL ACADEMY OF SCIENCES

JAMES OLDS
1922–1976

A Biographical Memoir by

RICHARD F. THOMPSON

Biographical Memoirs, VOLUME 77

PUBLISHED 1999 BY
THE NATIONAL ACADEMY PRESS
WASHINGTON, D.C.

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May 30, 1922–August 21, 1976

BY RICHARD F. THOMPSON

James Olds was one of the most important psychologists of the twentieth century. Indeed, many of us feel that his discovery of the "reward" system in the brain is the most important single discovery yet made in the field concerned with brain substrates of behavior. In retrospect, this discovery led to a much-increased understanding of the brain bases and mechanisms of substance abuse and addiction. Jim also was a pioneer in the study of neural substrates of learning and memory and the first to show that neurons in the hippocampus become substantially engaged in basic associative learning.

James Olds was born in Chicago on May 30, 1922, and grew up in Nyack, New York, and Washington, D.C. Jim's father was an economist who had been appointed by Franklin D. Roosevelt to be chairman of the Federal Power Commission. Jim held various summer jobs and spent a year as a reporter for the International News Service. After three years of military service with the Persian Gulf Command in Teheran and Cairo, Jim returned to the United States and finished his B.A. at Amherst College in 1947. In 1946 he married Marianne N. Olds, nee Egier, a student at Smith College. They had one daughter, Nicole Jacqueline Olds, now a psychiatrist on the faculty of Harvard Medical School, and a son, James L. Olds, now himself a prominent neuroscientist.

Jim received his master of arts degree in 1951 and his Ph.D. in 1952 in psychology at Harvard University, where he remained for one additional year as a lecturer and research associate in the laboratory of social relations. He then received a U.S. Public Health Service fellowship to do postdoctoral research at McGill University from 1953 to 1955. Jim then spent an additional two years (1955-57) as an associate research psychobiologist in the anatomy department of the University of California, Los Angeles, following up on his discovery of the brain reward pathways in the exciting environment of the Brain Research Institute founded by Horace Magoun and Donald Lindsley. In 1957 Jim was appointed associate professor of psychology at the University of Michigan and was promoted to full professor in 1959. He remained at the University of Michigan until 1969, when he moved to the California Institute of Technology, where he held the position of Bing professor of behavioral biology. He remained at Cal Tech until his untimely death in 1976.

Jim's professional career is a fascinating story of the growth and development of an extraordinarily creative mind and talented experimental scientist. In his graduate work at Harvard, his mentor was the experimental psychologist Richard Solomon. He also came under the influence of Talcott Parsons, who hired him to edit one of his books. Jim's contribution was so extensive that Parsons made him co-author, and kept up a lifetime relationship to discuss theoretical problems. An example of Jim's theoretical interests is reflected in his early paper on "a neural model for sign-gestalt theory" (*Psychological Review* 61(1954):59-72).

From his Harvard years and from the profound influence of D. O. Hebb's book *The Organization of Behavior* (New

York: Wiley, 1949) Jim developed a deep and abiding interest in motivation (see Jim's book *The Growth and Structure of Motivation*, Glencoe, Ill.: Free Press, 1956). By the time Jim received his Ph.D., "he was a convinced neuroscientist even if not an expert in all the techniques necessary to carry on research in the field. It was clear to him that psychological theory had to be derived from CNS function, and would constitute as such a realistic foundation for normative behavior. It was thus logical that after he obtained his Ph.D. he sought further training in physiological methods, and to do so in a setting (McGill) in which such an approach was an integral part of the work of Hebb, Jasper, and Penfield" (M. E. Olds, personal communication).

Jim arrived at McGill to work with Donald Hebb, who gave him free reign. The Hebb laboratory was on the second floor of the Doonner Building. Jim received a key to a storage area in the basement where pieces of wood and old equipment were kept. Jim had the impression Hebb would return a few months later to see what he might have discovered.

Jim was also given a McGill undergraduate, Ralph Morrison, as a helper. At that time everyone was interested in the reticular activating system (RAS), and thus Jim elected to record from that system. A simple electrode was made from standard insulated wire and a homemade connector was rigged up. There was much discussion at that time of the motivational bias impinging on the RAS activity; therefore, the first step in the project was to stimulate using the implanted electrode, to determine whether such stimulation was neutral or had positive or negative effects. The implantation of the electrode was done during the week, and on Sunday morning Jim decided to go to the lab to see whether everything was ready for the test to be given Monday morning by him and Morrison. The rat was placed in an open field, the electrode connector was attached, and a train of

60-Hz sine waves was applied for 0.25 seconds using a hand-held button to apply brief electrical shocks. The insight came when Jim noticed that the rat kept returning to the area in the open field where the last shock had been given. The shocks were repeated in that area but not elsewhere in the open field. The upshot was that, upon returning home, Jim announced that he had made a discovery, a real one, one that would not evaporate the next day. The phenomenon was demonstrated on Monday to the members of the laboratory, and in time was followed by testing for the positive effect of brain stimulation in a Skinner operant chamber. P. Milner was at that time a third- or fourth-year graduate student in Hebb's laboratory working on the neural basis of timing in the rat. His contribution to Jim's training was invaluable in terms of showing him the techniques of implantation, stimulation, and recording and, in general, contributing his knowledge of physiological techniques to the training of a postdoc more schooled in the theoretical than the experimental aspects of that field.

The course of this discovery is an extraordinary example of a creative mind seizing on an unexpected and serendipitous observation. In the words of Neal Miller, himself a leading scientist in the field, "His initial and greatest discovery resulted from having the wit to notice and exploit a totally unexpected outcome—an important aspect of science and inadequately understood by the general public or by those legislators who believe that it is efficient to concentrate most research on specific planned programs to attack targeted practical problems" (N. E. Miller. Forward in J. Olds. *Drives and Reinforcements: Behavioral Studies of Hypothalamic Functions.* New York: Raven Press, 1977). The circumstances of the discovery of the brain reward system are vividly described by Jim in his article in *Scientific American* (1956, p. 107-108):

With the help of Hess's technique for probing the brain and Skinner's for measuring motivation, we have been engaged in a series of experiments that began three years ago under the guidance of the psychologist D. O. Hebb at McGill University. At the beginning we planned to explore particularly the mid-brain reticular system—the sleep-control area that had been investigated by Magoun.

Just before we began our own work, H. R. Delgado, W. W. Roberts, and N. E. Miller at Yale University had undertaken a similar study. They had located an area in the lower part of the mid-line system where stimulation caused the animal to avoid the behavior that provoked the electrical stimulus. We wished to investigate positive as well as negative effects (that is, to learn whether stimulation of some areas might be sought rather than avoided by the animal).

We were not at first concerned to hit very specific points in the brain, and, in fact, in our early tests the electrodes did not always go to the particular areas in the mid-line system at which they were aimed. Our lack of aim turned out to be a fortunate happening for us. In one animal the electrode missed its target and landed not in the mid-brain reticular system but in a nerve pathway from the rhinencephalon. This led to an unexpected discovery.

In the test experiment we were using, the animal was placed in a large box with corners labeled A, B, C, and D. Whenever the animal went to corner A, its brain was given a mild electric shock by the experimenter. When the test was performed on the animal with the electrode in the rhinencephalic nerve, it kept returning to corner A. After several such returns on the first day, it finally went to a different place and fell asleep. The next day, however, it seemed even more interested in corner A.

At this point we assumed that the stimulus must provoke curiosity; we did not yet think of it as a reward. Further experimentation on the same animal soon indicated, to our surprise, that its response to the stimulus was more than curiosity. On the second day, after the animal had acquired the habit of returning to corner A to be stimulated, we began trying to draw it away to corner B, giving it an electric shock whenever it took a step in that direction. Within a matter of five minutes the animal was in corner B. After this the animal could be directed to almost any spot in the box at the will of the experimenter. Every step in the right direction was paid with a small shock; on arrival at the appointed place the animal received a longer series of shocks.

Next the animal was put on a T-shaped platform and stimulated if it turned right at the crossing of the T but not if it turned left. It soon learned to turn right every time. At this point we reversed the procedure, and the animal had to turn left in order to get a shock. With some guidance from the experimenter it eventually switched from the right to the left. We followed up with a test of the animal's response when it was hungry. Food was withheld for 24 hours. Then the animal was placed in a T, both arms of which were baited with mash. The animal would receive the electric stimulus at a point halfway down the right arm. It learned to go there, and it always stopped at this point, never going to the food at all!

After confirming this powerful effect of stimulation of brain areas by experiments with a series of animals, we set out to map the places in the brain where such an effect could be obtained. We wanted to measure the strength of the effect in each place. Here Skinner's technique provided the means. By putting the animal in the "do-it-yourself" situation (i.e., pressing a lever to stimulate its own brain) we could translate the animal's strength of "desire" into response frequency, which can be seen and measured.

The first animal in the Skinner box ended all doubts in our minds that electric stimulation applied to some parts of the brain could indeed provide a reward for behavior. The test displayed the phenomenon in bold relief where anyone who wanted to look could see it. Left to itself in the apparatus, the animal (after about two to five minutes of learning) stimulated its own brain regularly about once very five seconds, taking a stimulus of a second or so every time. After thirty minutes the experimenter turned off the current, so that the animal's pressing of the lever no longer stimulated the brain. Under these conditions the animal pressed it about seven times and went to sleep. We found that the test was repeatable as often as we cared to apply it. When the current was turned on and the animal was given one shock as an hors d'ouevre it would begin stimulating its brain again. When the electricity was turned off, it would try a few times and then go to sleep.

The discovery of the brain reward system led to an explosion of research in the field and for a period of years it was the most widely studied topic in physiological psychology. Other investigators attacked Olds's basic notion of a reward system on every conceivable ground, a not uncommon phenomenon in science when a major discovery has been made. The best work in the field continued to be done by Olds and associates.

In the initial observations by Olds and Milner (1954) the septal area appeared to be the region of greatest reward value. However, Olds (1962) completed a detailed mapping study of the reward value of various regions of the brain. Reward value could be determined by the rate at which the rat delivered shocks to its brain. In certain regions of the hypothalamus, for example, the animal would self-stimulate at a rate of 2,000 responses per hour (1958). The mapping study identified the general region of the medial forebrain bundle and lateral hypothalamus as the most reliable regions.

One objection to Olds's notion was that self-stimulation was simply a "forced motor" seizure. Olds (1956) showed that rats will learn mazes to obtain electrical brain stimulation in a manner essentially identical to hungry rats learning the same maze for food reward. Another objection was that the brain shock was simply activating a "feeding center" (i.e., that self-stimulation activated natural reward systems in the brain). In a most important study Olds, Allan, and Briese (1971) introduced the use of the microelectrode to stimulate very localized regions of brain tissue. They examined self-stimulation and electrical feeding and drinking behavior. Results indicated that these behaviors could in fact be differentially elicited. Stimulation of an anterior region of the hypothalamus elicited only drinking. Eating alone was elicited by stimulation of the more dorsal portion of the middle lateral region of the hypothalamus. Electrical self-stimulation alone was obtained from a fairly wide lateral region occupied by the medial forebrain bundle. Stimulation of the ventromedial nucleus (the "satiety" center) tended to inhibit or disrupt eating and did not elicit self-stimulation. However, stimulation in the middle lateral region of the hypothalamus produced mixed effects. In short a partially separable reward system did appear to exist in the brain.

When Jim moved to Cal Tech in 1969 a major focus of his work became brain substrates of learning and memory. He pioneered methods of single unit recording in the behaving animal (rat). At the time, movement artifact was a very serious problem in such studies. For Jim, one of the advantages of Cal Tech was the superb engineering talent. As John Disterhoft describes it:

> I recall his excitement when, in collaboration with one of the electrical engineers from the Jet Propulsion Laboratory, he designed what must have been one of the earliest telemetry systems for multiple single unit recording. The idea was to transmit signals from ten microwire electrodes simultaneously without danger of cable artifacts. The rat looked a little ungainly with the miniature transmitter on his head, but the system worked pretty well. Jim was always trying to come up with a better operational amplifier. . . . He also got involved in troubleshooting things like electronic waveform identifiers—he always wanted ours to work better, to be simpler and more state-of-the-art (J. F. Disterhoft, personal communication).

In addition to pioneering electronic methods to obtain movement artifact-free recordings, Jim also approached the problem from the other side with typical ingenuity. He arranged the training situation for the rats—he was using differential discriminations with auditory cues for food reward—such that the rat had to remain motionless when the conditioned stimuli were presented.

In his initial single neuron studies of learning, Olds and his associates recorded from a variety of brain regions, including the hippocampus, reticular formation, and midbrain (see, e.g., Mink, Best, and Olds, 1967; Phillips and Olds, 1969; Olds, Mink, and Best, 1969; Hirano, Best, and Olds, 1970; Olds, Disterhoft, Segal, Kornblith, and Hirsh, 1972; Segal, Disterhoft, and Olds, 1972; Segal and Olds, 1972; Segal and Olds, 1973; Kornblith and Olds, 1973). These were pioneering studies showing learning-related changes in neuronal activity in a number of brain regions. I believe these studies were the first to show clear learning-related changes in patterns of neuronal discharge in the hippocampus, as well as in other brain structures.

I will give an example of an extremely insightful analysis of unit activity in the midbrain (ventral tegmentum and reticular formation) during classical conditioning (Brauth and Olds, 1977). The procedure involved pairing one frequency of tone (CS) with rewarding brain stimulation (UCS). Results indicated that only neurons that responded to the CS before training showed learning-related changes in response patterns, a striking result. The authors concluded: "This implies that although the behavioral response of the animal arises de novo as a result of learning, only those midbrain units that possess connections to the CS pathway participate in conditioning process. This effect constitutes strong evidence in favor of a model of learning based on the intersection of CS and UCS pathways." This is a remarkably prescient conclusion, which has been strongly supported in recent years.

In this work, Olds and his colleagues wrestled with a fundamental problem, namely, how to distinguish between neurons whose discharge rates are influenced by nonspecific factors like arousal versus learning and how to distinguish between neurons that coded learning and neurons simply influenced by other neurons that coded learning. In brief, how can one localize the sites of memory formation? Olds took the approach of focusing on the shortest latency changes in patterns of neuronal discharge following CS onset (see Olds, Disterhoft, Segal, Kornblith, and Hirsh, 1972). This led to unit studies in the auditory system (e.g., Disterhoft and Olds, 1972).

Special note must be made of Jim's wife Marianne, who collaborated with him on the pharmacological properties of the sites where brain stimulation was rewarding. She had received training in neurophysiology from T. Bullock at UCLA, and had been a postdoc with Edward Domino, a professor of pharmacology at the University of Michigan Medical School working on the function of the acetylcholine transmitter. Their collaboration continued until Jim's untimely death.

Jim had a number of students and postdoctoral fellows who went on to become distinguished neuroscientists themselves. To name a few students: Aryeh Routtenberg, Menahem Segal, Bob Wurtz, Ralph Norgren; to name a few postdocs: Philip Best, John Disterhoft, Michael T. Phillips, T. Hirano, Paul Shinkman. He was a superb mentor.

Jim received a number of honors and awards in his career, beginning with the Newcomb Cleveland Prize from the American Association for the Advancement of Science in 1956. He was awarded the Hofheimer Award from the American Psychiatric Association in 1958; the Howard Crosby Warren Medal from the Society of Experimental Psychologists in 1962; and the Distinguished Scientist Award from the American Psychological Association in 1967. Jim was elected to the National Academy of Sciences at the young age of forty-five in 1967 and was elected president of Division 6 of the American Psychological Association in 1971. In my opinion Jim's discoveries are of such fundamental importance that he merited a Nobel Prize.

I close with personal recollections from people who worked with Jim. Paul Shinkman, now a distinguished professor of psychology at the University of North Carolina, spent a postdoctoral year (1965-66) in Jim's lab at the University of Michigan:

Jim, as you know, was a small man with bright sparkling eyes and quick, agile gestures and movements. He was also possessed of a keen, finely developed sense of humor. One day in the lab he was telling a few of us about the way he had discovered rewarding brain stimulation 12 years earlier. He delivered the brain stimulus with a hand-held button. On one particular occasion the (newly implanted) rat crept cautiously across the floor of the testing chamber. At this point in telling this story, Jim assumed the role of the rat, moving furtively across the room while continuing the narrative. When the first brief brain stimulation was delivered, the rat stopped abruptly, took two careful steps backwards, and peered up directly at Jim. (Here Jim looked up over his shoulder in a bemused position). "The rat," said Jim, "seemed to say, 'I don't know what I just did, but whatever it was, I want to do it again.' " Jim immediately stopped thinking about elicited behaviors and began on the spot to attempt informal shaping of emitted behaviors (P. G. Shinkman, personal communication).

Philip Best, now a distinguished professor of psychology at Miami University of Ohio, joined the Olds Brain Research Laboratory at the University of Michigan for a postdoctoral fellowship in 1965, following his Ph.D. at Princeton. He states:

On a typical day, M. Olds would come to the laboratory early in the morning to set up her experiment, and following that, to discuss the status of the projects carried out by one or two technicians. Jim would typically work at home in the morning, and would come in for lunch. They would eat lunch in their office, usually without anyone else present. Occasionally they would ask someone to come in and discuss some particular issue, but they preferred to eat alone. It seemed to be a protected time together. After lunch, Marianne would either leave for the day or return to her work and Jim would do his rounds. His first stop was usually the unit room. He would then typically go to the machine shop or the electronics shop to discuss design changes, and then would visit with the graduate students and postdocs. Afterward he would handle business with the office manager, and then would come back to the unit room to discuss current problems.

To me, he always seemed most intense and eager when discussing both technical and theoretical issues in the unit room. Early on we had many technical problems, and the most frequent topic of discussion was how to solve them. Often the discussions would become very heated. It was very easy to become frustrated by the technical difficulties or defensive if your solution did not work, or if the others rejected it. Yet, as intense as Jim could become, he was the least likely to get hooked into anger or defensiveness. While he was very eager to make progress, he was amazingly patient and circumspect about the problem or “screw-up” of the day. He seemed to be able to treat each new problem as just another step in the process, and as something that soon would be solved. He showed persistence, and persistent optimism in the face of some pretty horrendous problems, and some pretty cantankerous junior colleagues.

Every week, on Saturday mornings at 10:00 A.M. we had a lab meeting. Usually one of the graduate students or postdocs would present a progress report on their experiments. Making the presentation could be rather stressful, but the mood was usually upbeat, because Jim set the tone of the meeting. He saw it as an opportunity to generate new ideas and to engage in group problem solving behavior. If you were afraid you did not have enough progress to report, the easiest way to get through the meeting was to raise a few hypothetical questions that would get Olds speculating. That was also the situation where he would shine the brightest. At times he would be Socratic, but he could do so without being pedantic or patronizing. The discussion was most fun when he would get off on a tangent, completely unrelated to the topic at hand. As I said before, he had such a fertile mind and was so undefensive that everyone risked speculating and criticizing the ideas of others. At around noon, a few impatient wives would call to find out when we would come home.

He loved to play with ideas, and loved to argue over anything. I remember a few occasions when we would be discussing one of his ideas that I thought was particularly groundless. A few minutes later, it would occur to me that I was now defending his idea and he was attacking it. When he recognized my delayed realization, he would start laughing, and say something like “I just wanted to see if I could convince myself that it was as bad an idea as you originally thought,” or “I just wanted to see if you could come up with better arguments than me to refute yourself.” I never saw him become defensive about his ideas or impatient with the thinking of others, even if they were quite lame speculations. Frequently at the end of one of these arguments, I would marvel at how many good ideas I had, only to realize later that most of my best ideas were indeed his (P. J. Best, personal communication).

John Disterhoft, a distinguished professor of neuroscience at Northwestern University School of Medicine, spent two and a half years (1970-73) as a postdoctoral fellow in Jim’s lab at Cal Tech:

Jim was immersed in his work. He loved science, especially as it concerned the brain and how it functioned. If he had one frustration, it was that he had not spent enough of his life immersed in learning facts about the brain. He felt that, the more facts he had stored with which to make associations, the more significant the insights that would be possible from his theoretical speculations. He spent a good bit of time thinking, talking, and writing about how the brain worked.

Jim was also fascinated with computers and electronics. Many of his ideas about brain function (e.g., his speculations about memory storage function in the hippocampus) used computers and their memories as analogies. Our laboratory was well equipped with computers, and a good bit of time was spent on developing and testing software and hardware. The burst of information we were able to gather in a relatively short period of time came from using a combined hardware-software system simultaneously to study a large number of brain regions in animals engaged in learning the same task. This was coordinated strategy, as Jim was well aware of the strengths of the system he had set up.

The portion of the laboratory where I worked was set up with four training stations. Jim always had one assigned to him and carried on a series of experiments separate from those of the postdoctoral fellows and graduate students. He spent a fair amount of time traveling, and so had a technician help him. But when he was in town, he came in every morning to check the rat that was being trained in his station and to check the setting of the waveform discriminators on the unit channels being used. Jim was very demanding about the quality of data he and the people in his group gathered. He was a firm believer that high quality findings came from high quality data. Our system had numerous checks for electronic noise and various other artifacts. He also took an intense interest in the experiments as they were being run. We all lived fairly close to the laboratory. The experiments we were running with freely moving rats ran from the evening until early morning, the peak of the rats' diurnal cycle. I often came in during the evening to check on how things were going to discover that Jim had been there shortly before. Almost invariably when I came in early on Sunday (before going to the beach), Jim had already been in the lab and made some notes or adjustments on the computer or the printout. The experiments in which we were involved were truly a joint effort in which he took an active role.

Another thing that I remember vividly and very much enjoyed when I was in the laboratory was the almost daily data meetings. We were gathering a lot of data and we were all trying to keep on top of it. So we got together in Jim's office every afternoon to discuss our data and what they meant. These meetings often included theoretical discussions that ranged far from the data at hand. They also included discussions of appropriate strategies to use in our ongoing experiments. Jim was intimately involved with me in designing and developing the software routines we used on the mainframe computer for summarizing the data for individual rats and for groups of animals. He didn't just assign me to go over to the computer center and come back with data reduction routines. He went along during the discussions about how the routines should be set up, plotted, and what kinds of error checking we should incorporate into the programs. This was at a time when the minicomputers (DEC PDP/8s) that ran our experiments were not powerful enough to do the data reduction either during or after the experiment.

Some of my fondest memories of Jim Olds are the personal ones. He was a gracious, urbane person. He had a good sense of humor and often had a smile on his face. I never saw him use his position to intimidate or denigrate those working with or for him. He was pleasant not only to those he considered his academic or scientific peers and trainees, but also to the secretaries and staff at Cal Tech. I am the oldest in a relatively large family, and during my stay in his lab my parents and several of my siblings came to visit. I was always impressed by how Jim made a special effort to make my family members feel welcomed and at ease by taking time to say hello when they came to investigate what I spent my time doing. For example, he spent time one beautiful Pasadena afternoon explaining the appeal of neurophysiology to my father by guessing that my father was a fisherman and comparing hunting for cells to waiting for a fish to bite—not a bad analogy at all (J. F. Disterhoft, personal communication).

THE FOLLOWING RESOURCES were very helpful in writing this biographical memoir: Olds, J. *Drives and Reinforcements: Behavioral Studies of Hypothalamic Functions.* New York: Raven Press, 1977; Thompson, R. F. *Introduction to Physiological Psychology*. New York: Harper & Row, 1975; Thompson, R. F. James Olds: 1922-1976. *American Journal of Psychology*, 92(1979):151-52; the biography in the files of the Home Secretary of the National Academy of Sciences; personal communications from Philip J. Best, John F. Disterhoft, Marianne E. Olds, and Paul G. Shinkman; and the many publications by James Olds and his associates.

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