

HISTORY OF BIOPSYCHOLOGY AT MICHIGAN

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This history was originally written in 1993 in response to a request by Biopsychology graduate students for information on the history of biopsychology at the University of Michigan. It contained a brief history of the University of Michigan, a description of the evolution of the Psychology Department, a history of Biopsychology (including a list of Ph.D. dissertations), and lastly, a number of biographical sketches that illustrate the diversity of backgrounds and careers of the Biopsychology faculty and some of the more recent retirees. The history was updated in 2000, in 2003, and in 2008 to include new faculty and graduates and to reflect changes in the Area and in the Department.¹

THE UNIVERSITY OF MICHIGAN

The University likes to date its beginning to 1817, but at that time the University was more a dream than a realization. In 1817, Michigan was a territory ruled by a governor and judges appointed by the President. The population of the territory was about 7,000 people and Detroit, which had been occupied by the British during the War of 1812, had a population of less than 1,500. Nevertheless, in 1817, the governor and the judges proclaimed:

“Be it enacted by the Governor and the Judges of the Territory of Michigan that there shall be in said Territory a Catholepistemiad...or University.”

Considering what was possible without an educated populace, the term Catholepistemiad was pretentious, implying an all-encompassing educational institution. Not much came of these beginnings in the first two decades, aside from a few temporary classes in Detroit, except that

1 Those interested in more of the history of the University should read Howard Peckham's *The Making of the University of Michigan, 1817-1992* (updated by M. & N. Steneck, 1997) and Ruth Bordin's *The University of Michigan: A Pictorial History*, both published by the University of Michigan Press. A good source for the early history of the Psychology Department is Alfred C. Raphelson's unpublished two volumes entitled: *PSYCHOLOGY AT THE UNIVERSITY OF MICHIGAN, 1852-1950*. Also relevant is B.W. Agranoff's article, *The neurosciences at the University of Michigan* (TINS, March 1979). The more recent history of biopsychology at Michigan was obtained from interviews and correspondence with former students and staff, and some of the more senior current staff members. Please contact either Kent Berridge or Elliot S. Valenstein to suggest any corrections, additions or changes. Susan Douglas was enormously helpful in revising this document in 2008.

they established the principle, if not reality, of a publicly supported university for Michigan.

By 1837, the population of Michigan had reached approximately 100,000 and the territory was admitted into the union as a state. During the same year, it was decided (for the second time) that there should be a University of Michigan, and plans were made for a campus. Ann Arbor was chosen over competing communities as the site of the University after the property-owning citizens of Ann Arbor contributed 40 acres for that purpose (still the center of the campus). At the time, Ann Arbor was 14 years old and had a population of only 2,000 inhabitants.

Classes were first offered in the fall of 1841 to six freshmen, one sophomore, and twenty-three students in the preparatory school. The University Building was completed in 1841 and renamed, in 1843, Mason Hall after Stevens T. Mason, Michigan's first state governor.²

Before the end of the century, the University of Michigan had become one of the most successful examples of a non-sectarian, state-supported university. It thus became a model for many other state universities. In July 1887, *Harpers Weekly* published an article on the University of Michigan which includes the following appraisal:

“The most striking feature of the University is the broad and liberal spirit in which it does its work. Students are allowed the widest freedom consistent with sound scholarship in pursuing the studies of their choice; they are held to no minute police regulations, but are treated as persons with high and definite aims from which they are not easily to be diverted. No religious tests are imposed, but devotional exercises are held at stated times, which no one is compelled to attend against his choice, though all are welcome. Women are admitted to all departments on equal terms with men; the doors of the University are open to all applicants who are properly qualified, from whatever part of the world they may come.”

Throughout the years, the University of Michigan's reputation as an eminent educational and scholarly institution grew steadily. As a result, it attracted some of the most promising students from around the United States and from abroad. Perhaps best exemplary of that reputation is the fact that the prominent Wallenberg family of Sweden chose to send their son Raoul to Michigan because, in their view, it combined a democratic tradition and excellence better than any other university they considered. Among

2 The original building no longer exists. In 1950, a student arsonist burned down Haven Hall. It was decided to use the opportunity to rebuild the old structures located behind Angell Hall. Two large new buildings, Mason Hall and Haven Hall, both connected to Angell Hall, were completed in 1952. According to Bill McKeachie, there was still a building named University Hall behind Angell Hall in the late 1940's.

Michigan graduates are many eminent scientists including several Nobel Prize recipients, a U.S. President, a Supreme Court Justice, leading literary figures, astronauts, and accomplished people of all walks of life.³

The University of Michigan has also had a proud tradition of being a leading cultural center among universities. Over the years, many prominent persons in the Arts have been in residence here, including the poets Robert Frost and Joseph Brodsky. In 1892, the Polish pianist/politician, Ignace Jan Paderewski, played a benefit at the University and during the ensuing years the performers at the University have included Renata Tebaldi, Pablo Casal, Yehudi Menuhin, Marian Anderson, Vladimir Horowitz, Leonard Bernstein, Mstislav Rostropovich and others too numerous to mention.

EARLY PRESIDENTS OF THE UNIVERSITY

The earliest presidents of the University were clergymen, graduates of seminary schools, some of whom taught courses in moral philosophy a subject that dealt with such psychological topics as “will,” “responsibility,” and “consciousness.” Although there was no attempt to shape the University in their own image, it may be of some interest that the first two University presidents (Henry Tappan & Erastus O. Haven) were “moral philosophers” a label sometimes given to psychologists prior to the 1890’s, when psychology was taught within departments of philosophy.

HENRY PHILIP TAPPAN

(UM’S FIRST PRESIDENT, 1852-1863)

Tappan was a native New Yorker, a graduate of Union College and a well-known “moral philosopher” whose books were highly respected (among philosophers) in this country and in Europe. The title of one of his books was The Doctrine of Will Determined by an Appeal to Consciousness (1840).

Before becoming President of Michigan, Tappan had visited European universities and he said that he was anxious to establish “an American university deserving of the name.” Under Tappan’s guidance, science courses were offered and Michigan became the second university in the country to grant a Bachelor of Science degree. Before the end of Tappan’s presidency, the University had a Law Department and a Medical Department in addition to the Literary Department---science courses being included in the latter. Tappan gradually replaced the clergymen instructors with young, intellectual leaders.

Apparently, Tappan managed to irritate a number of people while setting goals for the University. In 1863,

³ For anyone interested, the Alumni Association, which has a building on campus, can supply a list of prominent graduates of the university.

a conservative Board of Regents, none of whom had any higher education, summarily dismissed Tappan the day following the convocation, putting the campus into an uproar. Embittered, Tappan spent his remaining years in Europe.

ERASTUS O. HAVEN

(2ND UM PRESIDENT, 1863-1869)

Haven was also a “moral philosopher” and a clergyman. One of his lectures was entitled: Increased mental activity of the age: Its causes and demands.

During Haven’s administration a controversy arose about admitting women. Haven’s original position on this issue can be inferred from his statement:

Youth is a transitional period when passion is strong and restraint is feeble, and if, just at this period, multitudes of both sexes are massed together, not in families and not restrained by the discipline of the home circle, consequences anomalous and not to be cultivated by an Institution supported by the State are likely to occur.

Nevertheless, Haven changed his views and in 1867 when the legislature approved admitting women, he supported this action. The first woman was admitted in 1870 and five years later there were about 100 women enrolled in the University. By 1882, President Angell could proudly point out that six members of the Wellesley faculty, including the President (Alice Freeman) were women graduates of Michigan.

In 1869, Haven resigned unexpectedly in order to take the position of President of Northwestern University.

HENRY SIMMONS FRIEZE

(ACTING UM PRESIDENT, 1869-1871 after Haven resigned)

Frieze had been a Professor of Latin, an inspiring teacher and a gifted musician, but he was not considered to be the right man for the Presidency of the University. During his tenure as acting president, Frieze founded the University Musical Society, an organization that has remained active ever since.

JAMES BURRILL ANGELL

(3RD UM PRESIDENT, 1871-1909)

When Angell became President of the University in 1871, there were three colleges (Literary, Medicine and Law), nine buildings (including four professor’s houses), a faculty of thirty-five, and more than 1,200 students, half of whom were in Law or Medicine.⁴ Although this does not seem

⁴ Snapshot of the University: The freshman class of the college (the “literary and scientific departments”) had 176 students (16 were women) in 1870. In 1871, Angell commented on how he seemed to have succeeded in stopping the freshman and sophomores from throwing “missiles” (usually nuts) at each other during chapel services. At this time, there were over 300 students

like much by today's standards, Michigan was the largest university in the United States at the time, with a yearly budget of \$100,000.

Under Angell, the University grew enormously. Angell helped to establish the College of Dental Surgery, the Homeopathic Medical School (1875), a Department of Pharmacy (1876), the first Professorship of Science in the U.S. (1879), a College of Education, a separate Engineering College, and a Department of Forestry. Fifty University buildings were constructed during Angell's presidency. Angell took leave from the University on a few occasions in order to accept diplomatic assignments to China and to Turkey. Subsequently, Angell spent a year and one-half in Peking as "Envoy Extraordinary" (appointed by President Rutherford Hayes) negotiating an agreement that would establish barriers against "coolie labor" coming to the U.S. and against traffic in opium. Angell encouraged wealthy Chinese families to send their sons and daughters to the University of Michigan and since then there have been a succession of Chinese studying in Ann Arbor.⁵ There exists today (1993), a Barbour Fellowship for Asian women, a fine oriental art collection in our Art Museum, and eminent Asian Study Program, and an exchange program for students and faculty between the Psychology Departments of the University of Michigan and Peking University.

Although he attempted to resign several times, Angell was persuaded to remain as president of the University until 1909. By that time Angell was 81 years old. By the end of Angell's thirty-eight year presidency, the University of Michigan was well on the way to becoming the world class university that it is today ---- a university that the football team can be proud of...

When Angell died in 1916, there was a huge crowd of University and Ann Arbor people lining State Street for his funeral procession.⁶

enrolled in the Law School (only one was a woman). The women in the medical school were taught anatomy separately from the men and the staff involved received an extra \$500 for the additional work. A large and influential group of state citizens were responsible for a homeopathic medical school to be created at Michigan (the current ROTC building once housed part of the homeopathic school), which co-existed for decades with the traditional allopathic medical school before disappearing around 1900.

5 Before the Second World War, there were branches of the University of Michigan Alumni Association in China. Starting in the 1980's, a number of Psychology faculty members have given a series of lectures at Peking University and Chinese students (including Ph.D. candidates) and faculty have spent varying lengths of time at this University.

6 James Burrill Angell was the father of the psychologist, James Rowland Angell, who later became president of Yale University. The younger Angell grew up living in the U-M Presi-

Following Angell there were a succession of Presidents of the University many of whose names now appear on campus buildings: Harry Hutchins, Marion Burton, Clarence Little, Alexander Ruthven, Harlan Hatcher, Roffen Fleming, Harold Shapiro, James Duderstadt and Lee Bollinger. The President since 2002 is Mary Sue Coleman.

SOME OF UNIVERSITY OF MICHIGAN "FIRSTS"⁷

1. The University of Michigan's Medical School was one of the first medical schools to consider research to be an essential part of their mandate. In 1865, UM's Medical School was the largest in the U.S.

dent's house on South University Street. Apparently southern Michigan had Malaria during those years as the son had several bouts of that disease. He attended the U-M as an undergraduate where his first class in psychology used Dewey's recently published book, Psychology. Angell had interests in philosophy, medicine, physiology, and evolution, but decided in the end to pursue an academic career in psychology. He stayed an extra year after graduation to work for an M.A. under Dewey, went to Harvard to continue graduate work in psychology under William James, and then to Leipzig to pursue his Ph.D. under Wilhelm Wundt.

Angell finished a draft dissertation for his Ph.D. but before he could rewrite the final version, he received an offer of an assistant professorship in psychology at the University of Minnesota – for which he would have to leave Germany in two weeks! He took the job, and never completed his dissertation. Thus he never received an official Ph.D. despite his many achievements in academia though he later received several honorary doctorates.

Angell did not remain long at Minnesota. John Dewey had recently moved from Michigan to the University of Chicago, where he was building a program in philosophy and psychology, and where others were building programs in zoology and physiology. Dewey invited Angell to move to Chicago, where Angell eventually became full professor and a leading figure in "functionalist psychology". He wrote an interesting Psychological Review paper on the relation of psychology to evolution, and was graduate advisor to young John B. Watson's Ph.D. dissertation at Chicago. Watson's dissertation was in biopsychology: the effects of sensory deafferentation on learning maze habits. Later Watson moved to Johns Hopkins, and became famous as the founder of behaviorism before his academic career was cut short by a scandal (Watson began an affair with a graduate student whom he later married -- but was then still married to his first wife. That situation was not acceptable to Baltimore mores at the time, and he was forced to retire). After about 12 years at Chicago, Angell began to shift some of his focus to administrative duties. He later wrote of this shift with some regret and explained it as due largely to the need to support his large family. Angell eventually became a full time administrator at Chicago, He left Chicago to take over the direction of the Carnegie foundation at New York, and then moved to Yale, where he remained president for many years.

7 These claims have appeared in various histories of the University of Michigan, but neither Kent nor Elliot have checked their historical accuracy (except for #5 & #6).

2. When established in 1854, the Detroit Observatory (still on campus, but no longer functional) had the largest telescope in the U.S. and the third largest in the world. The Observatory was given the name “Detroit” because the major contributors came from that city.

3. In 1856, UM’s Chemistry Building was the first separate building completely devoted to chemistry in the U.S.

4. In 1878, UM’s Laboratory of Electrotherapeutics was the first in the U.S.

5. In 1881, UM appointed the first professor of psychology, at least by one criterion (see note 8).

6. In 1884, UM offered the first course in comparative psychology taught in the U.S. (John Dewey’s “Special Topics in Physiological, Comparative, and Morbid Psychology”). This course might be argued to have been the first course in general biopsychology ever taught and a precursor of today’s Psychology 330 (Introduction to Biopsychology).

7. In 1903, UM’s Psychopathic Hospital was the first teaching hospital devoted to nervous and mental disorders. Albert Barrett, the Professor of Psychiatry, was the Director of the Psychopathic Hospital.

“PSYCHOLOGY AS PART OF PHILOSOPHY DEPARTMENT

As in most, if not all, universities in the United States, psychology at the University of Michigan was first taught within the Philosophy Department. In some schools, the Philosophy Department started to be called a “Philosophy and Psychology Department” during the early part of the 20th Century. Early “psychology” courses were usually considered to be courses in “mental or moral philosophy” and dealt with such subjects as morality, will, consciousness, and mental capacity. The period of the “mental philosophers” extended approximately from about the 1850’s to about 1895, at which time psychology laboratories were getting established at a number of universities.

Although experimental psychology began to be taught and studied at Michigan in the 1880s, psychology did not become a separate Department at Michigan until 1929. In 1959, the Department established the separate Area structure, which characterizes the large Michigan Psychology Department of today. Prior to 1959, graduate students were admitted by the Department as a whole and they, in time, affiliated with specific faculty members.

In addition to Presidents Tappan and Haven (first

two University presidents who taught courses in “moral philosophy”), several other members of the Philosophy Department taught “psychology” courses. A few of the more notable (or colorful) staff members are mentioned below.

Benjamin Franklin Cocker

The first Michigan professor to have the term Psychology in his title, Cocker had a colorful background. He was born in England – a Yorkshire man, who had immigrated to Australia because of poor health. He became prosperous in business, but lost much of his wealth in a financial panic (1856). Cocker had invested most of his remaining money in a trading ship, but was shipwrecked off the island of Tonga and lost most of his possessions and almost his life to the unfriendly “savages.” He managed to return to Australia, but his fortune was gone and he decided to immigrate to the United States. Cocker came to Adrian, Michigan because a Methodist clergyman, whom he had met in Australia, lived there and had promised to help him get settled. After Haven stopped teaching in 1869, Cocker was given the “Chair of Mental Philosophy.” He has been described as a friendly, supportive person whose lectures were more like inspirational sermons than academic lectures. He taught a psychology class every fall and a philosophy course that included some material on “rational psychology.” In 1881, Cocker became Professor of psychology, speculative Philosophy, and the History of Religion.⁸ After Cocker’s death in 1883, G. Howison taught Empirical Psychology (Philosophy 1).

George Morris

Morris was made head of the Philosophy Department at Michigan in the 1880’s, but he worked out an arrangement whereby he would continue to do some teaching at Johns Hopkins in Baltimore where he and G. Stanley Hall influenced each other. Hall had studied with Wilhelm Wundt in Germany and he was a strong advocate of psychology becoming an experimental, laboratory science.⁹ During the time that Morris continued to spend

8 It might be claimed that Cocker was the first “Professor of Psychology” in the country, despite the fact that Psychology was not yet a separate Department. However, James McKeen Cattell, who was appointed Professor of Psychology at the University of Pennsylvania in 1889, has also been claimed to have been the first professor of psychology in the U.S. Others consider that Joseph Jastrow, a John Hopkins Ph.D. who was appointed Professor of Psychology at the University of Wisconsin in 1887, to have been the first professor of psychology. The choice of who receives the laurels depends partly on the criteria used by the judge.

9 During the last decade of the 19th century, many Americans studied with Wilhelm Wundt in Leipzig. G. Stanley Hall may have been the first, but James McKeen Cattell was the first American to get a Ph. D. under Wundt. In addition, there

some time teaching at Hopkins where he had among his students Joseph Jastrow, James McKeen Cattell, H.H. Donaldson, E.C. Sanford, and John Dewey—all important figures in the development of psychology in the United States. Morris was responsible for bringing the young John Dewey (age 26) to Michigan in 1884 at a starting salary of \$900 for the year.

John Dewey

(UM from 1884-1888; 1889-1894)

John Dewey was hired at Michigan as an instructor in order to teach courses in psychology. His first years were spent primarily teaching introductory psychology and advanced experimental and biopsychology courses. After spending about 10 years at this University (with one year away at the University of Minnesota) he left as a Professor to join the staff at the University of Chicago. Dewey had received his Ph.D. under G. Stanley Hall at Johns Hopkins University where he became indoctrinated in the experimental approach to psychology. At Michigan, Dewey first taught “Empirical Psychology” in 1884 and “Special Topics: Physiological, Comparative, and Morbid Psychology (Philosophy 12). “Morbid” psychology referred to abnormal or clinical psychology. In 1885, Dewey taught Experimental Psychology (Philosophy 12) and in 1887 he published the influential text PSYCHOLOGY, which gives a reader a good idea of the content of these early courses. Dewey left Michigan in 1888 with a wife (an undergraduate he met at the rooming house where he lived) to take the Chair of the very small philosophy department at the University of Minnesota. The next year (1889), after Morris (Chairman of Philosophy at Michigan) died of pneumonia, Dewey returned to Michigan as Chairman of Philosophy¹⁰. Dewey became increasingly

were others such as Edward Titchener and Hugo Munsterberg who were foreign nationals but settled in the U.S. after getting a Wundt Ph.D. It has been said that the early U.S. psychology laboratories were almost duplicates of Wundt’s lab. However, while Wundt used reaction time and other sensory-motor tasks to learn about the mind, most American (Titchener was an exception) used the same equipment to study individual differences. Wundt regarded this as “typically American.” By 1908, there were a number of universities offering training in psychology and Americans no longer went to Leipzig to study with Wundt. The titles of two of Wundt’s enormous number of publications are Lectures on Animal and Human Psychology (1863) and Principles of Physiology (1873). Wundt’s greatest influence, however, was through the people he trained in psychology as a laboratory science.

10 Herbert Spencer Jennings (1868-1947) was an undergraduate student at Michigan who took Dewey’s Intro Philosophy course around 1890. Jennings later became well known for his studies of behavior of protozoa as an influential professor of zoology at Johns Hopkins University. A favorite quote from Jennings: “The writer is absolutely convinced, after a long

interested in education, publishing an article on “language development in children” based on observations made on his own children. Dewey remained at Michigan for five more years and while he did not teach any more psychology courses, he was responsible for changing psychology from the “moral philosophy” stage to that of a natural science with an emphasis on evolution, physiology, and function. Although their views were not identical, John Dewey, Wm. James, and Charles Pierce are considered functionalists and pragmatists in the history of psychology. These three rejected the European tradition of examining the faculties of the mind and emphasized, instead, that the mind evolved for practical reasons and that it is best studied in action, during learning and problem solving.

Dewey hired James H. Tufts to teach general psychology and physiological psychology during the 1889-90 academic year. With Dewey’s encouragement, Tufts established a psychology lab (reaction time, color perception, psychophysics, and brain anatomy) on the top floor of the old Medical Building. This marks the beginning of experimental work in Psychology at Michigan. At the time, there were only about eight psychology laboratories in the whole country.

When James Tufts accepted a position at the University of Chicago in 1891, Dewey recruited Alfred H. Lloyd and George H. Mead to teach psychology courses. Both Lloyd and Mead had studied at Harvard under Wm. James. Mead had also studied in Germany where he knew G. Stanley Hall. For three years, Mead taught a psychology laboratory course that emphasized physiology (dissecting frogs, studying brains). In 1894, Dewey accepted a position at the University of Chicago and Mead went with him. The following year, Dewey recruited James R. Angell, whom he had known as a promising undergraduate at Michigan. Thus, former Michigan staff and students (Tufts, Dewey, Mead, and Angell) formed the nucleus of what was to become the “Chicago School of Psychology and Philosophy.”

Dewey later went to Columbia University

study of this organism, that if Amoeba were a large animal, so as to come within the everyday experience of human beings, its behavior would at once call forth the attribution to it of stages of pleasure and pain, of hunger, desire, and the like, on precisely the same basis as we attribute these things to a dog. ... In conducting objective investigations we train ourselves to suppress this impression, but thorough investigation tends to restore it stronger than at first.” (p. 336, *Behavior of Lower Organisms*, 1931). Jennings was named by his father after the English natural philosopher, Herbert Spencer. But Jennings views diverged from his namesake’s partly as a result of Dewey’s teaching, which sparked Jennings’ intellectual enthusiasm. Jennings later wrote “Professor Dewey’s attacks on Herbert Spencer’s philosophy and on Materialism showed that they had no monopoly on rigid logical thinking...I was left again in the condition of suspense of judgment; the great questions were entirely reopened.”

where he became best known and most influential for his contributions to educational psychology. Dewey's empirical (pragmatic) approach to education through action ("learning by doing") provided the intellectual foundation for the so-called "progressive education" movement.

Other Notes on the Teaching of Psychology:

First UM course entitled: PHYSIOLOGICAL PSYCHOLOGY (listed as Philosophy 12) was taught by W. Hough during the FALL of 1888.

In the FALL of 1890, Psychology courses were consolidated into a series. General Psychology was taught by J. Tufts using Dewey's Psychology as the text.

After Tufts, Dewey, Mead, and Angell left Michigan, psychology courses were taught by Alfred Lloyd, and for short periods by John Bigham and Edgar Pierce, both of whom had trained under Hugo Münsterberg at Harvard. Robert Mark Wenley became chairman of Philosophy and was responsible for recruiting staff to teach psychology courses. In 1897, Wenley recruited Walter Pillsbury to supervise the teaching of psychology.

ESTABLISHMENT OF THE PSYCHOLOGY DEPARTMENT

Although psychology was emerging as a distinct field during the early part of the 20th century, it remained a "sub-department" within the Philosophy Department at Michigan until Wenley, the Chairman of Philosophy died. In 1929, Psychology became a separate department and Walter Pillsbury was appointed the first Chairman of Psychology.

THE WALTER PILLSBURY YEARS (1897-1942)

As an undergraduate, Walter Bowers Pillsbury studied at Nebraska under H. K. Wolfe, the second American to get a degree under Wilhelm Wundt. Afterwards, Pillsbury was a Ph.D. student of Edward Titchener at Cornell University. Titchener had also studied with Wundt. At Cornell, Pillsbury translated (with Titchener) several German texts. Thus, Pillsbury had a thorough exposure to the German experimental approach to psychology. Pillsbury's dissertation was on attention. Among several of Pillsbury's later books were: Essentials of Psychology; History of Psychology; and Attention. Pillsbury was not an experimentalist, but he was reputed to be a superb teacher in his younger days and his introductory psychology became one of the most popular courses on campus.¹¹ Pillsbury introduced Titchener's experimental

11 It was at the University of Michigan that Clark Hull first studied psychology, first carried out a laboratory research project,

manuals and his text, Essentials of Psychology, to replace Dewey's text, and he initiated classes in animal, child, and systematic psychology. Pillsbury started the practice of teaching a one-semester parallel course emphasizing physiological and experimental psychology for majors. Pillsbury collaborated with physiologists and the medical staff and encouraged psychology graduate students to take "medical" courses.

Pillsbury became influential nationally within psychology circles and he was on the editorial board of journals. Pillsbury and Donald Marquis, who succeeded him as Chairman, were amongst the youngest Presidents of the American Psychological Association. Later, E. Lowell Kelly, Theodore M. Newcomb and Wilbert McKeachie, both Chairman of Psychology at Michigan, were also APA Presidents.

Pillsbury was responsible for bringing the American Psychological Association 43rd meeting to Ann Arbor in 1935. Among the biopsychology, cognition, and and first began to study learning as a topic. Thus, it might be claimed that Hullian learning theory began at Michigan. Hull grew up on a farm in Michigan and attended High School in what today might be called a "junior college" at Alma College (where he survived a bout of typhoid). He studied mathematics, physics, and chemistry at Alma, and intended to be a mining engineer. Soon after he began his first job as an engineer, he was struck with polio. He required a year of convalescence and then spent two years teaching high school in Alma.

At age 27, Hull entered the University of Michigan as a junior around 1911. He later wrote the "free system operating there permitted me to concentrate largely on psychology during my first two years in residence, similar to that of a graduate student... The outstanding course in psychology at that time, and of my life for that matter, was a year of experimental psychology, Professor W.B. Pillsbury giving the lectures and Professor J.F. Shepard in charge of the laboratory. They made a great combination, and each was incomparable in his own way. During my senior year I carried out a small experimental project involving learning under Shepard's direction." (According to McKeachie, Hull used Shepard's "Chinese figures" for his dissertation on learning.)

After receiving his A.B. from Michigan, Hull applied to graduate school but was turned down for a fellowship by Yale and Cornell. Through his intercession of Walter Pillsbury, a graduate fellowship was obtained at Wisconsin with Joseph Jastrow (at \$50 per month). Hull later wrote regarding Jastrow's teaching ability: "He would sometimes lecture for five minutes at a time in perfectly good sentences, yet hardly say a thing." While at Wisconsin, Hull began projects on statistics, hypnosis, smoking (the subjective effects of nicotine-free versus ordinary smoking, using a specially designed pipe), and learning. After completing his Ph.D., he moved to the Institute of Human Relations at Yale University (organized under the direction of James R. Angell, described in a footnote above). There he began to pursue the formal "drive reduction learning theory" that dominated much of American psychological research in the 1940's and 1950's and which is "immortalized" in the equation, $sE_r = sH_r \times D$.

perception presentations at the 1935 APA meeting in Ann Arbor were the following:

Ivan Kreshevsky

(a.k.a. David Krech)

“Brain mechanisms and brightness discrimination.”

Harry Harlow

“The effect of local cortical anaesthetization on motor and sensory functions.”

Norman R. F. Maier

“Some new tests of reasoning in rats.”

Leonard Carmichael

“The onset and primary development of reflexes in the Field Guinea Pig.”

REORGANIZATION OF DEPARTMENT IN 1936

The Executive Committee of LSA proposed a democratization of departments in 1933. Although most departmental chairmen at the time were autocratic by today’s standards, Pillsbury has been reported to have produced an especially great amount of resentment among some of the staff. It is said that all manuscripts by any faculty member had to be approved by him before being submitted for publication and that he made arbitrary, unilateral decisions in allocating resources.

Pillsbury resisted the proposed democratization of the Department but the Dean insisted, and in 1936 the Psychology Department adopted the “Department Articles of Organization” (a Constitution). The Department Constitution established the power of a democratically elected Executive Committee. This constitution (with some amendments) remains in effect to date and the Executive Committee makes decisions about appointments, applying to the Administration for new positions, promotions, salary increases, and curriculum matters for both undergraduate and graduate students. There are also committees established by the Executive Committee such as the Undergraduate and Graduate Committees that are responsible for much of the micromanagement of the Department.

Pillsbury retired at 70 in 1942 after 45 years at Michigan. After his retirement, a Pillsbury Prize was established to be awarded annually to the undergraduate completing the best research project.

JOHN SHEPARD

Pillsbury had encouraged John Shepard to switch his graduate studies from the University of Chicago to the University of Michigan in 1903. Shepard’s Ph.D. in 1906 was Michigan’s first Psychology doctorate¹² and
12 There were many Michigan Ph.D.s before Shepard. The

after graduation, he was given a staff appointment. Over the years, Shepard became a major figure in the training graduate students in biopsychology during the Pillsbury era. Although he published little, Shepard was a dedicated teacher and researcher. His research interests, which were primarily in the area of comparative learning capacities, influenced the research interests of his students.

Shepard’s thesis entitled: Organic Changes and Feeling was published in the American Journal of Psychology (1906, vol. 17). Shepard attempted to test Wundt’s “tridimensional theory of feelings” by measuring autonomic changes produced by exposure to various pleasant and unpleasant (including painful hand shock) stimuli. Most of the physiological measures were peripheral autonomic indices such as circulatory changes in the hand and breathing rate and depth. However, one of the subjects was an Ann Arbor laborer who had a part of his skull removed as a result of an earlier accident. Shepard attached a motion sensitive device over the subject’s scalp at the site of the missing skull making it possible to record changes in brain pulsations (caused by cerebral circulatory changes) produced by exposure to the stimuli. Although it is doubtful that the thesis really tested Wundt’s theory, it is worth noting that Michigan’s first Psychology Ph.D. thesis was a physiological study.

After completing his Ph.D., Shepard was given a faculty appointment and he later became Director of the Psychology Laboratory. Shepard also headed a University Committee on New Buildings and under his leadership the Natural Science building was designed to house the Departments of Botany, Forestry, Geology, Mineralogy, Zoology, and Psychology. Today only the Biology Department and its subdivisions occupy the Natural Science Building. Much of Psychology’s activities (teaching and research) were in the Natural Science building from its completion in 1915 until 1952, but various members of the Department were located in other buildings around the campus after World War II and in some cases at Willow Run Airport.

Shepard taught introductory psychology (often with Pillsbury) and also advanced experimental psychology, psychophysical methods, systematic psychology, genetic psychology, and comparative psychology. Shepard’s major area of interest was comparative psychology. He studied the role of early experience on pecking in chicks and he attempted to compare the mental processes of many different animals e.g., ants finding sugar and comparable learning tasks in fish, rats, cats, and humans. Shepard constructed a human maze under Hill Auditorium where subjects could be observed as they tried to find their way through the maze. He also had a rat maze in the Natural Science Building where the animals could be observed
first two Michigan PhD’s were awarded in 1876 and between 1876 and 1900 there were 63 Ph.D.’s awarded by the University.

through “peek holes” from the floor above.

Shepard’s two most famous students were N. R. F. Maier & T.C. Schneirla, but among other notable Shepard Ph.D.s were Ernest B. Skaggs, Norman Cameron, and Burton D. Thuma.

Late in his career, Shepard is said to have become involved in politics that may have resulted in conflict with the administration. In any case, although, Shepard seemed to be the natural successor to Pillsbury, he does not appear to have been seriously considered as a candidate for the position as Chairman of the Department. Shepard retired in 1950.

The Shepard Lecture Series was established by his son in 1960. Funds are still available and periodically a distinguished psychologist is invited to give the Shepard Lecture.

Shepard died in 1965.

SELECTION OF A SUCCESSOR TO PILLSBURY

Ernest Hilgard, who came to Michigan for an interview, is said to have been the first choice to replace Pillsbury, but during the interim surrounding the deliberations, he accepted a position at Stanford. Skinner (Minnesota), Weaver (Princeton), and Donald Marquis were also on the short list to succeed Pillsbury. Marquis was selected.

DONALD MARQUIS CHAIRMAN (1942-1957)

Marquis had been a Carnegie Fellow, spending a year in Pavlov’s laboratory, and later wrote *Conditioning and Learning* with Hilgard. Marquis had also been Chairman at Yale.

Bill McKeachie reports that when he returned from military service to start graduate school in 1945, there were only six active faculty members in the Department and (because of the draft) most of the graduate students were women. During Marquis’ first five years at Michigan, the Psychology Department staff increased from 8 to 50. Among the more notable senior staff recruited by Marquis were Daniel Katz (Ph.D. Syracuse), E. Lowell Kelly (Ph.D. Stanford), Rensis Likert (Ph.D. Columbia), Angus Campbell (Ph.D. Stanford), Urie Bronfenbrenner (Ph.D. Michigan), Leon Festinger (Ph.D. Iowa), Dorwin Cartwright (Ph.D. Harvard), John B. French (Ph.D. Harvard), John Atkinson (Ph.D. Michigan), Clyde Coombs (Ph.D. Chicago), and Wilber McKeachie (Ph.D. Michigan). Theodore Newcomb (Ph.D. Columbia) was already in the Sociology Department at Michigan, but Marquis arranged for him to have an appointment in Psychology and to help create a joint Ph.D. program in Social Psychology. Marquis also helped start the Mental Health Research Institute (MHRI; renamed the Molecular and Behavioral

Neuroscience Institute or MBNI around 2005) and the Institute of Social Research (ISR). Both MHRI, which administratively is under the Psychiatry Department, and ISR employ a number of psychologists, including, in the case of MBNI, several biological psychologists.

Marquis left the University in 1957 and went first to the Social Science Research Council and later to MIT after he and his wife were divorced (Dorothy Marquis was also a psychologist with a Departmental affiliation).

E. LOWELL KELLY CHAIRMAN (1957-1961)

Kelley studied with Terman and was well known for his work in the field of testing, pilot selection, etc. Kelly was awarded the APA Gold Medal. In 1959, during Kelly’s chairmanship, the Area organization was created. The original Areas were Experimental, Physiological, Clinical, Mathematical, Developmental, Personality and Social, Industrial, and General. For several years after the establishment of the Area structure, the final selection of graduate students was done by a committee of the whole Department. It is now done within Areas

WILBERT (“Bill”) McKEACHIE CHAIRMAN (1961-1971)

Under McKeachie, the Department continued to expand at a remarkable rate. By 1965, the Psychology Department had joint staff appointments with 25 other University units. Counting all the different types of appointments (including joint appointments) the University of Michigan had (and still has) more psychologists on campus than any other University in the country.

KEITH SMITH CHAIRMAN (1971-1976)

WARREN NORMAN CHAIRMAN (1976-1981)

During Warren Norman’s tenure as Chairman, the Biopsychology Area added Terry Robinson and Warren Holmes to our faculty.

ALBERT CAIN CHAIRMAN (1981-1991)

During Al Cain’s tenure as Chairman of the Department, a number of Biopsychology positions were approved and the Area successively recruited Barbara Smuts (half time), Kent Berridge, Jill Becker and Theresa Lee. Also, during Al Cain’s tenure, approval for a Psychology Building (the renovation of East Engineering) was obtained, although construction did not occur until the early mid-1990s.

PATRICIA GURIN
CHAIR (1992 -2002)

During Pat Gurin's tenure as Chair of the Psychology Department, East Hall was completed and occupied by the Department in 1995. During these years, the biopsychology area was joined by Steve Maren, Seema Bhatnagar, Jeff Hutsler, and Tim Schallert. In addition, Wayne Aldridge and Geoff Gerstner joined the area as adjunct faculty. The Cognition and Perception area hired Patricia Reuter-Lorenz, Bill Gehring, Thad Polk, and Jun Zhang. In addition, a number of senior C&P faculty extended their cognitive research into cognitive neuroscience, including John Jonides, Patricia Reuter-Lorenz and Ed Smith. The second half of Pat Gurin's tenure was marked by increasing emphasis on interactions across areas of Psychology, leading to interdisciplinary hiring, courses, and research collaboration.

RICHARD GONZALES
CHAIRMAN (2002- 2007)

Among the changes that occurred during "Rich" Gonzalez' tenure as Chair of the Department was the more efficient use of research and community space in East Hall, a new undergraduate neuroscience concentration in conjunction with the Biology Department, and the expansion of the undergraduate "psychology majors" to over 2,000 students. Martin Sarter was recruited during Richard Gozalez' tenure as Chairman.

Theresa ("Terri") Lee
CHAIR 2007--Present

OTHER UM PSYCHOLOGY FACTS:

Departmental ranking. From about 1960, the UM Psychology Department has been consistently rated as one of the strongest in the country. While ratings are somewhat arbitrary and they certainly differ between subspecialties, UM's Psychology Department has been consistently judged to be one of the five strongest psychology departments in the country. The National Research Committee rated UM No. 2 in 1995 (Michigan tied for second place with Yale, UCLA, and U. Illinois; Stanford was in first place, and Harvard was in third place behind these others). In 2005, *US News and World Report* ranked the whole department as tied for second place and ranked the Biopsychology Program as #1 (under the category 'Behavioral Neuroscience').

Faculty Honors. Four members of our department were APA Presidents (Walter Pillsbury, Donald Marquis, E. Lowell Kelly, and William McKeachie). A full 14% of those who have received the APA Award for Distinguished Scientific Contributions taught at Michigan or received their Ph.D. here (Leon Festinger, James Olds, Stanley

Schachter, Roger Brown, Angus Campbell, Theodore Newcomb, Russell De Valois, Robert Zajonc, John Atkinson, Michael Posner, Amos Tversky, Clyde Coombs, Eleanor Macoby, John Swets, Robert Wurtz, Edward Smith, Richard Nisbett, David Meyer, Wilbert McKeachie), Elliot Valenstein received the lifetime career award from the International Behavioral Neuroscience Society. David Meyer and Ed Smith received the APS William James career award. Ed Smith and Dick Nisbett were both elected to the National Academy of Sciences.

MOVE TO EAST HALL

Up until 1995, the Psychology Department was spread around the campus, with no building holding the majority of the staff. The Biopsychology Area was located principally in the Neuroscience Building at Huron & Glen streets (torn down in 2002 and replaced by a new medical research building) and its administration, but not all of its affiliated faculty were located in that building. After seemingly endless meetings with architects, the renovation of East Hall (formerly East Engineering) was completed and most of the Psychology Department staff had moved into this building by September 1995.

INTERACTION BETWEEN BIOPSYCHOLOGY AND COGNITION & PERCEPTION AREAS

After the move to East Hall in 1995, the relationship has grown increasingly close between the Biopsychology area and the Cognition & Perception area. The two areas occupy space close together on the 4th & 3rd floors, and share secretarial and office services. A new undergraduate concentration was formed in "Biopsychology and Cognitive Science" by expanding the earlier "Psychology as Natural Science" degree (which had been primarily biopsychology) to include C&P faculty and more C&P courses. This concentration currently attracts more students than do the majority of departments in the University. More recently a third undergraduate concentration was added in Neuroscience which involves a collaboration between the Biosychology Area and the Biology Department.

Interactions between the Biosychology and the C&P Areas were strengthened by increasing convergence of faculty and graduate student interests. A number of new C&P junior faculty were hired in the 1990s whose research was oriented toward the brain, and at the same time several senior C&P faculty also began cognitive neuroscience research using brain imaging techniques. Thus the brain became a shared topic of interest for the two areas. At the same time, an increasing number of biopsychology faculty have developed interest in processes related to cognition. In recent years, there have been faculty hired with split (inter-areas appointments, and the two areas now routinely

confer together on admission of particular graduate students.¹³

NOTABLE HISTORICAL FIGURES

Although there were no separate Areas in the department prior to the 1960s, the most notable early biopsychologists at Michigan (besides John Shepard) were Norman R. F. Maier and Theodore C. Schneirla. A brief sketch of these two eminent Michigan Ph.D.'s follows.

Norman R. F. Maier

(1900-1977; Ph.D. 1928)¹⁴

For a number of years, Norman Maier (1900-1977) was one of the most active, well-funded and visible

13 *A brief description of the development of interest in cognitive neuroscience written by John Jonides follows:*

There were two important developments within the C&P Area that led to its closer connection to Biopsychology. One is that several faculty became interested in pathologies of the brain and their consequence for cognitive and affective functioning. For example, one member of the C&P faculty has extensive experience studying patients with callostomies, using them as important models for functioning of the two hemispheres separately. Another has focused on Parkinson's patients, studying how their dysfunction in motor skills might relate to other cognitive dysfunctions. Yet another has focused on obsessive-compulsive disorder and the underlying brain mechanisms involved. Further, more than one C&P faculty member is involved in studying Major Depressive Disorder and its underlying brain correlates and cognitive presentations. These and other examples of studies of brain dysfunction have become more and more central to the mission of the C&P Area.

The second development that has focused many C&P faculty on brain mechanisms has been the emergence of neuroimaging tools that can be used not only to localize brain processes, but also to test and enhance psychological theory. The main tools in use among C&P faculty are event-related potentials (ERPs) and functional magnetic resonance imaging (fMRI). ERP has long been an important tool to localize the temporal course of processing, and several C&P Area faculty have made productive use of it. Functional MRI has become an important tool to understand the spatial localization of processing in the brain, growing out of the earlier use of positron emission tomography. There are now several faculty in the C&P Area who have active laboratories collecting both behavioral and fMRI data as a way to converge on theoretical developments. This has been made possible by the founding of an fMRI Laboratory on North Campus that is jointly administered by a neuroscientist and a biomedical engineer. This joint core facility makes possible the acquisition and analysis of large bodies of imaging data by all users, and it also serves as a nexus of educational activities to promote the development and dissemination of fMRI techniques.

14 Those interested in learning more about Maier might read the obituary: Solem, A & McKeachie, W.J. Norman R. F. Maier (1900 – 1977), *American Psychologist*, 1979, 34, 266-267 and also Dewsbury, D.A. On publishing controversy. Norman R.F. Maier and the genesis of seizures. *American Psychologists*. 1993, 48, 869-877.

psychologists in the Department. Maier received his Ph.D. at Michigan in 1928 under John Shepard).

Maier was born in Michigan and received a BA degree from UM in 1923. After spending one year at the University of Berlin with Gestalt psychologists (Kohler, Wertheimer & Lewin), Maier returned to Michigan and completed the Ph.D. degree in 1928 (Thesis: "Reasoning in the White Rat"). He then spent two years at the University of Chicago with Karl Lashley before becoming a member of the Michigan Psychology Department in 1931. Maier received a \$3500 Rackham grant to study "Effects of brain damage in rats," but much of his subsequent animal research was devoted to animal models of abnormal behavior in the tradition of Pavlov and Howard Liddell and based on that work he wrote a monograph in 1938 entitled: *Experimentally Induced Abnormal Behavior in the Rat*. The monograph described the abnormal behavior produced by unresolvable conflict situations and frustrations. A presentation at the 1938 AAAS meeting won the "AAAS Thousand Dollar Prize" (now called the "Newcomb Cleveland Prize"). Maier was the first psychologist to have won that prize (Neil Miller and James Olds, who shared the prize in 1956 are the only other psychologists so honored). Maier's work was described in many newspaper and magazines (including LIFE) and the work was incorporated into a *New Yorker* story by E. B. White. Maier had his lab in the old Pharmacology building, while most of Department staff were in the Natural Science Building, except for a few housed in the West Medical Building.

Maier wrote 12 books, including "Frustration: the Study of Behavior Without a Goal" a topic which had some similarities with the concept of "learned helplessness" and "Principles of Animal Behavior" (with Schneirla).

Later, Maier became interested in behavior of humans in industry, organizations, and government and wrote Principles of Human Relations (1952).

Theodore Schneirla

1902-1968; Ph.D. 1927)

Although Schneirla was never a regular staff member of the Department, he received a Ph.D. from Michigan under John Shepard and went on to have a major influence on the field of comparative psychology.

Schneirla was born in Michigan, where his family had a small celery farm. He received a BS degree from UM, where he played the trumpet in the band and married a student at the University.

In Shepard's class, Schneirla kept a 400 page, beautifully illustrated notebook (Elliot Valenstein has seen this notebook in Schneirla's office at The American Museum of Natural History. It had Shepard's "A" grade on the front cover). Schneirla's Ph.D. thesis ("Learning and Orientation in Ants," 1927) was completed while he

was teaching at New York University. In 1943, Frank Beach appointed him Associate Curator at the Museum of Natural History and around this time he became an Adjunct Professor at New York University.

Schneirla spent a year with Karl Lashley in Chicago, where he and N. R. F. Maier, who knew each other at Michigan, became good friends. Schneirla's reputation was first based on his field experiments on ants in Panama and Mexico.

Schneirla was offered a Michigan staff position, but he turned down the offer as he was entrenched in New York by this time. The offer was made to Maier and accepted.

Schneirla became a major theoretician in comparative psychology and Maier and Schneirla's book "PRINCIPLES OF ANIMAL PSYCHOLOGY" (1935) remains a classic in comparative psychology.

BIOPSYCHOLOGY AREA CHAIRS:

James Papsdorf (1968-1970)
William Stebbins (1970-1977)
Elliot Valenstein (1977-1990)
Valenstein & Robinson, Co-Chairs (1990-1991)
Terrence ("Terry") E. Robinson (1991-1996)
Barbara Smuts (1996)
Warren Holmes (1997-2000)
Theresa Lee (2000-2005)
Jill Becker (2005-2007)
Terry Robinson (2007-2008)
Martin Sarter (2008-present)

BIOPSYCHOLOGY FACULTY IN 1940-50's

John Shepard
Norman Maier
Carl Brown
H. Richard Blackwell (1947 Ph.D.)
Burton Thuma
Edward Walker (1947-1980)
Robert McCleary (1953-1957)
James Olds (1957-1968)
Robert Isaacson (Ph.D. 1958)
Mathew Alpern (1955-1996)
C. J. Smith (Ph.D. 1952)
Russel DeValois (Ph.D. 1952; Postdoc.
1953 -)
Paul Ellen (Ph.D. 1954)

BIOPSYCHOLOGY DURING 1960'S

In 1959, the Psychology Department established eight separate Areas: Physiological, Experimental, Clinical, Developmental, Personality and Social, Industrial,

Mathematical, and General. Despite the Area structure, there was no clearly established Biopsychology Area Chair until 1968. Prior to that time, members had only loose affiliations with the Physiological Area. Jim Olds, who clearly was the most prominent physiological psychologist at Michigan during much of the 1960's, refused to become Area Chair and for the most part the staff recruited their own students and set the requirements for them. Whenever an issue arose that concerned the training of biopsychology students or other issues that had to be resolved, the staff got together (usually in someone's house) for a discussion. Bob Isaacson often took the initiative in calling meetings when there seemed to be a need. In 1968 (or 1969), James Papsdorf served as the Chair until 1971 at which time Bill Stebbins became the Area Chair.

Despite the lack of an Area Chair, there was some cohesiveness among those interested in biopsychology. For example, for several years there existed a lively "journal club" led by Jim Olds and Bob Doty (Physiology) that met regularly in the old Medical Library Building. Charles Votaw, an anatomist and protégé of Elizabeth Crosby held a rat brain anatomy seminar that met Saturday mornings for several semesters. Bernard Agranoff, Samuel Hicks (Neuropathology and Brain Development), Ed Domino (Pharmacology), Lester Rutledge, and Bob Doty (both in Physiology) helped to establish a collegiality among those interested in the brain and behavior. Around 1969, the Area changed its name from Physiological Psychology to Psychobiology. This was in response to a suggestion by Bill Stebbins and Ed Walker who felt that those doing animal studies, but not involving physiological interventions or measurements, would feel more comfortable under the umbrella of the broader title. Ed Walker had been in the Experimental Area, but he withdrew because he did not feel accepted by his chair, Arthur Melton, who (it is said) had little interest in (or respect for) "animal psychology." Ed Walker, however, never was actively involved with the Psychobiology Area, but he was the chairman (sometimes, really the nominal chairman) of a great many Ph.D. thesis committees that involved biopsychology research.

For several years, beginning around 1965, there was a loosely affiliated "operant conditioning" group that met informally and taught some courses in operant conditioning. This group included (over different periods of time) William Stebbins, Harlan Lane, Charles Schuster (his main appointment was in Pharmacology), George Geis (interested in "self instruction" had an appointment in Center for Research on Learning and Teaching), John Falk (his main appointment was in Pathology where he was recruited by Sam Hicks) and Daryl Bem. Later affiliates of the operant group were Jim Woods (recruited by Schuster) and Dave Moody (recruited by Stebbins).

Staff affiliated with the Psychobiology Area during the 1960's:

Mathew Alpern (Ophthalmology and Psychology)
H. Richard Blackwell (Applied Vision Research)
Carl Brown (Vision Research)
Charles Butter
Steve Fox (main appointment in MHRI)
Stephen Glickman
Daniel Green (main appointment in Ophthalmology)
Robert Isaacson
Harlan Lane (in Experimental Area, but part of the "operant group")
Olney Thomas Law
Robert McCleary
James McConnell (MHRI and Biopsychology)
David Moody (Kresge Hearing Institute)
James Olds
Charles R. Schuster (Pharmacology)
C. J. Smith
William Stebbins
William Uttal
Edward Walker
James Woods (Pharmacology)

BIOPSYCHOLOGY IN THE 1970s

During the 5 years prior to 1970, the Area had lost many of its most prominent members. For various personal reasons (better offers, better climate, etc.) Stephen Glickman went to UC, Berkeley, Jim Olds went to Cal Tech, and Bob Isaacson went to the University of Florida in Gainesville. These departures took place within a three-year period from about 1965-1968. Charles Schuster, whose main appointment was in Pharmacology, left for the University of Chicago during the 1967-1968 academic year. Robert McCleary had left for the University of Chicago earlier in the 1960's. The last Psychobiology staff member hired was James Papsdorf, who came in 1965. Papsdorf came from Iowa, trained by I. Gormazano in rabbit eyelid conditioning. Charles Butter had joined the staff in 1962. Butter & Papsdorf were 100% on the Department budget. Stebbins, who joined the University in 1963, had his appointment in The Kresge Hearing Research Institute and the Department of Otorhinolaryngology. Stebbins became the Area Chair in 1971 and gradually he switched to an increasing fraction on the Psychology Department budget. Elliot Valenstein, who arrived in 1970, was less than 50% on the Department budget (the remainder of his appointment was in the newly established Neuroscience Program). Mathew Alpern's main appointment was in

Ophthalmology, but he participated in the activities of both the Experimental and Psychobiology Areas. Dan Green, who had been brought to the University by Mathew Alpern, had an appointment in Ophthalmology, but when he started to teach psychology (perception) courses, he was given a 33% appointment in Psychology, which over the years was reduced to zero. William Uttal, who was in MHRI (later ISR) was initially affiliated with the "Experimental Area," but later he participated as an active member of the Biopsychology group. (Bill Uttal left the University in 1985). James Woods' (Pharmacology), Dave Moody (Kresge Hearing Institute) and Roger Davis (MHRI) also became increasingly active participants in the Biopsychology Area during the 1960's.

For a number of years the Area was only a loose federation. With only a few of the staff on the Department budget only a bare minimum of undergraduate Psychobiology courses could be offered. Graduate students were admitted mainly if they "fit into" a laboratory of an affiliated staff member. The supervision of graduate students and the requirements for a degree (other than those set by the Department and University) were for the most part determined by a student's faculty mentor.

BIOPSYCHOLOGY IN THE 1980s and 1990s

Elliot S. Valenstein became the Area Chair in 1977. The Area was able to get two positions approved and Terry Robinson arrived in 1978 and Warren Holmes, who was completing his Ph.D., arrived in 1979. In 1985, another position was approved and Kent Berridge was hired. Two years later, an additional single Psychobiology position was approved, but when two women surfaced to the top during the selection process, the University responded to the "target of opportunity" by allowing the area to create two positions, and so both Jill Becker and Theresa ("Terri") Lee were recruited. Jill Becker, who originally came to the Department as a postdoctoral fellow with Terry Robinson, had been appointed a Research Scientist before getting the staff position in 1987. Theresa ("Terri") Lee, who was finishing "postdoctoral" research at Berkeley, arrived in 1988. In 1984, Barbara Smuts was given an appointment half in Psychology and half in Anthropology and she affiliated with the Psychobiology Area (Barbara's appointment is now completely in the Psychology Dept.). Henry Augustus ("Gus") Buchtel came to Ann Arbor with a VA appointment in 1980 and a Psychology appointment was officially awarded in 1981. Gus soon became an active participant in both the Biopsychology and the Clinical Areas. All of this staff recruitment made it possible to introduce a wide range of "popular" undergraduate psychology courses and to increase the number of Teaching Assistant positions available for graduate students.

In 1981, Biopsychology created two different

streams for graduate students to pursue: the “Brain-Behavior” and the “Evolution” stream.

In 1988, the Area name was officially changed from “Psychobiology” to “Biopsychology.” The prompting idea was that the area was primarily a field of psychology, so “psychology” should be the noun and “bio” the modifier rather than vice versa.

In 1991, psychology approved a new undergraduate major in psychology called: “BS Degree in Psychology as a Natural Science (now called “Concentration Program in Biopsychology and Cognitive Science”). This has proven to be an enormously popular major and most of the undergraduates electing this concentration pursue a sequence of biopsychology and C&P courses.

A new biopsychology faculty position was approved in 1996, and Steve Maren joined us. In 1998 several additional interdisciplinary positions were created, and Seema Bhatnagar and Jeff Hutsler joined the area. In addition, Wayne Aldridge and Geoff Gerstner both joined the biopsychology area as adjunct faculty in the mid 1990s. Several members of the cognition & perception area became increasingly interactive with biopsychology over this time, and one C&P faculty, John Jonides, has a biography included in this history.

During the period prior to 1971, the staff affiliated with the Biopsychology Area were spread all over the University, including Mason Hall (Warren Holmes), the Kresge Hearing Institute (David Moody & Bill Stebbins), and in space belonging to other Departments such as Pharmacology (Jim Woods). In 1971, the renovation of Neuroscience Laboratory Building (formerly the University Food Service Building) was completed and the laboratories of Elliot Valenstein, Charles Butter, Daniel Green, and Roger Davis were moved there. The Neuroscience Laboratory Building (NSL) housed, in addition to the above, the laboratories of Terry Robinson, Jill Becker, Kent Berridge, Theresa Lee, and Wayne Aldridge. The Cognition & Perception area was primarily located in the Perry Building (originally an elementary school). Most (though not all) members of both areas moved to East Hall in 1995. Since 2000, the Area was joined by Tim Schallert, Seema Bhatnagar, Jeff Hutsler, Martin Sarter, Jacinta Beehner and Thore Bergman. Schallert, Bhatnagar and Hutsler later left the area before 2008.

AREA FACULTY 2008

As most of the Cognition and Perception Area staff are now doing research on the brain, the primary faculty affiliated with that Area are also listed below.

Biopsychology primary faculty:

J. Wayne Aldridge
Brandon Aragona
Jill Becker Luma
Jacinta Beehner
Josh Berke
Kent Berridge
Thore Bergman
Theresa (“Terri”) Lee
Stephen Maren
Terrence (“Terry”) Robinson
Martin Sarter
Barbara Smuts
Sari van Anders

Affiliated Biopsychology faculty:

Henry (“Gus”) Buchtel (VA Hospital)
Geoffrey Gerstner (Dentistry)
Randy Nesse (Psychiatry + ISR)
Bryan Pfungst (Kresge Hearing)
Rachael Seidler (Kinesiology + Psychology)
James Woods (Pharmacology)

Emeritus Biopsychology Faculty:

Mathew Alpern (Emeritus, deceased)
Charles Butter (Emeritus)
Daniel Green (Emeritus)
William Stebbins (Emeritus)
Elliot Valenstein (Emeritus)

Recent Former and Visiting Biopsychology Faculty:

Jaak Panksepp (Visiting Professor 2000-2001)
Roger Davis (Retired)
Michael Gorman (Michigan Fellow 1997-1999)
Warren Holmes (Moved to U. Oregon)
Tim Schallert (2000-2001; returned to U. Texas)
Seema Bhatnagar
Jeff Hutsler

Primary Cognition & Perception Faculty

Julie Boland
William Gehring
Richard Gonzalez
John Jonides
Rick Lewis
Cindy Lustig
David Meyer
Robert Pachella
Thad Polk
Patricia Reuter-Lorenz
Rachael Seidler
Colleen Seifert
Daniel Weissman
J. Frank Yates
Jun Zhang

BIOPSYCHOLOGY GRADUATE STUDENTS: PH.D. THESIS

One of the best ways to learn about the work going on in biopsychology throughout the years is through the topics chosen for Ph.D. dissertations. The following list includes a list of most of the dissertations that deal with topics of interest to biopsychologists. The list includes many people who have gone on to have highly productive careers in biopsychology and in other areas. While any list of the more notable biopsychology or cognition/perception graduates of Michigan is somewhat subjective, besides Norman Maier and Theodore Schneirla, the more familiar names would probably include Robert Wurtz (Past President of the Society for Neuroscience, and National Academy of Science), Eleanor Macoby (National Academy of Science), Russel DeValois (National Academy of Science member), Seymour Wapner, John Swets (APA Distinguished Scientific Career Award, Warren Medal), David Meyer (APA Distinguished Scientific Career Award), Edward Smith (National Academy of Science member and APA Distinguished Scientific Career Award), Robert Isaacson (Named Chair at SUNY, Binghamton), Michael Posner (National Academy of Science), David Olton, Daniel Kimble, Marvin Lickey, Aryeh Routtenberg, Ed Pugh, Nancy Wexler, Amos Tversky, Donald Dewsbury, John Liebeskind (National Academy of Science), Jackson Beaty, and Larry Butcher.

1906

John Frederick Shepard "Organic changes and feeling."

1913

Harry W. Crane "Association reactions and reaction time."

1914

Joseph E. DeCamp "A study of retroactive inhibition."

1915

William H. Batson "Acquisition of skill."

Floyd C. Dockeray "The effect of physical fatigue upon mental efficiency."

1919

Charles H. Griffitts "Individual differences in imagery."

1922

Martha Guernsey Colby "A study of liminal intensities and the application of Weber's Law to tones of different pitch."

1923

Ernest Burton Skaggs "Further studies of retroactive inhibition."

1925

Milfred F. Baxter "An experimental study of the differentiations of temperaments on the basis of rate and strength."

Nellie L. Perkins "Human reaction in a maze of fixed orientations."

1926

Adelbert Ford "Attention – automatization: An investigation of the transition nature of mind."

1927

Norman Cameron "Effects of cerebral injury on the maze learning of the Albino rat."

1928

Carl R. Brown "A quantitative study of achromatic visual contrast."

John a. Glaze "Psychological effects of fasting."

Norman R. F. Maier "Reasoning in white rats."

Theodore C. Schneirla "The maze-learning and orientation of ants."

1929

Ella May Hanawalt "Whole and part methods in trial and error learning."

1930

Sinforoso G. Padilla "Further studies on the delayed pecking of chicks."

Burton D. Thuma "A contribution to the study of the auditory sensitivity of the white rat."

Doris F. Twitchell "An investigation of higher thought processes."

1931

Sugi Mibai "An experimental study of apparent movement."

1932

Lloyd S. Woodburne "The effect of a constant visual angle upon the binocular discrimination of depth differences."

1933

Dji-Lih Bao "Plateaus and the curve of learning in motor skills."

Mary C. Van Tuyl "Studies in the monocular perception of distance."

1934

Nathana Turk "The effect of cerebral destruction on the performance of the white rat in various maze situations."

1935

Siao-sung Djang "The role of past experience in the visual apprehension of the masked form." (later Chairman of Psychology at the National University of Taiwan)

Samuel A. Kirk "The effect of unilateral cerebral lesions on the handedness, pattern vision, and reasoning in the Albino rat."

- 1936**
Quin F. Curtis “The effect of floor cues upon the mastery of the unit-alike maze.”
- William L. Jenkins “Adaptation in isolated cold spots.”
- 1937**
Wilma T. Donahue “Psychological and physiological effects of noise.” (later founder of the Institute of Gerontology)
- Charles C. Irwin “A study of differential pitch sensitivity relative of avoidance theory.”
- Margaret V. Sabon “The effect of electrical stimulation of the thalamus on the performance of rats during visual discrimination learning.”
- 1938**
Margaret Ives “The flight of colors following intense brief stimulation of the eye.”
- J. Wallace Nygard “Cerebral circulation prevailing during sleep and hypnosis.”
- 1941**
Tooi Xoomsai “Measurement of emotional reactions.”
- 1942**
Irvin A. Berg “Development in behavior: The micturition pattern of the dog.”
- 1944**
Seymour Wapner “The differential effects of cortical injury and retesting of equivalence reactions in the rat.”
- 1947**
Harold Richard Blackwell “The inter-relations of contrast, area, and adaptation brightness in human binocular vision.”
- 1948**
Harold Guetzkow “An analysis of the operation of set in problem-solving behavior.”
- Joan H. U. Longhurst “Effect of brain injury to the rat on seizures produced during auditory stimulation.”
- Robert S. Waldrop “A statistical examination of Sheldon’s concept of primary components of morphology.”
- 1949**
Mortimer H. Applezweiz (AKA Appley) “The role of effort in learning and extinction.”
- Joan Morton “Human performance in a walk-through maze.”
- 1950**
Richard J. Anderson “Taste threshold in stimulus mixtures.”
- Stewart g. Armitage “The effects of barbiturates on the behavior of rat offspring in learning and reasoning situations.”
- John A. Swets “An experimental comparison of two theories of visual detection.”
- 1951**
Albert Eglash “Abnormal fixations.”
- Robert S. Feldman “The relationship between guidance and the specificity of the fixated response in the rat.”
- Eleanor E. Macoby “Acquisition and extinction of a conditioned response under three different patterns of partial reinforcement.”
- Daniel E. Scheer “The effect of frontal lobe operations on the attention process.”
- 1952**
Russel L. DeValois “The relation of different levels and kinds of motivation to variability of behavior.”
- Murray A. Glanzer “Stimulus satiation as an explanation of spontaneous alternation in rats.”
- John H. Taylor “Variations in spectral sensitivity within the human fovea.”
- 1953**
Olney Thomas Law, JR “The effect of background luminance on brightness discrimination.”
- 1954**
Paul Ellen “The compulsive nature of abnormal fixation.”
- 1955**
William N. Dember “Decision-time and psychological distance.” (later Dean at the University of Cincinnati)
- 1957**
Dorothy Foster “A comparison of the prairie and forest races of the deer-mouse, *Peromyscus maniculatus*, with respect to certain measures of behavior and temperament.”
- Ephraim Peretz “The effect of cingulectomy on fear.”
- 1958**
Bert Forrin “Affect conditioning associated with the onset and termination of electrical shock.”
- Andrew J. Karoly “Behavioral tests of rats under chronic reserpine.”
- 1959**
Robert L. Isaacson “An electrographic study of the dog during avoidance learning.”
- Berne L. Jacobs, Jr. “Some determinants of the rates of acquisition of avoidance and escape responses.”

Wilson P. Tanner, Jr. "Application of the theory of signal detectability to amplitude discrimination."

1960

Stephen S. Fox "Sensory deprivation and maintained sensory input in monkey: A behavioral and neuropharmacological study."

Sylvan Kornblum "Reaction time to sequential stimulus presentations."

Ausma Rabe "The effect of subcortical stimulation on memory and responsiveness in rats."

James T. Tedeschi, Jr. "Infantile stimulation in rats and the genesis of disposition to emotionality."

1961

Carleton D. Creelman "Human discrimination of auditory duration."

James R. Ison "Changes in instrumental response speed following rewarded end box placement." (later Chairman at the University of Rochester)

Daniel P. Kimble "The effect of bilateral hippocampal damage on cognitive and emotional behavior in the rat."

Richard T. Louttit "Effect of Phenylamine and Marplan feeding on brain serotonin and learning behavior in the rat." (later headed several important neuroscience and behavioral programs at NIH, HIMH, NSF)

1962

Jay S. Caldwell "One trial backward avoidance conditioning."

Salvatore N. Cinch "The effects of intra-cranial electrical stimulation on the delayed response test in monkeys."

Margaret L. Clay "Conditions effecting voluntary alcohol consumption in laboratory animals."

Fred Horvath "Subcortical mechanisms in behavior: The effects of basolateral amygdectomy on three types of avoidance behavior in cats."

David L. Margules "Motivational producing properties of the feeding system of the rat hypothalamus."

Michael I. Posner "An informational approach to thinking."

Sol Schwartz "The effect of neo-natal brain damage and early environment on adult behavior in the hooded rat."

Paul G. Shinkman "Visual depth discrimination in day old chicks."

Robert H. Wurtz "Self-stimulation and escape behavior in response to stimulation of the rat amygdala."

1963

Gordon G. Bechtel "Emotionally based drive and autonomic patterning."

Allan L. Jacobsen "An attempt to demonstrate transfer of a maze habit by ingestion in planarians."

Rachel Kaplan "A new measure of motivation in rats."

Stephen Kaplan "Arousal and preservation: A theoretical model."

Leonard Lash "Response discriminability and the hippocampus."

John C. Liebeskind "The effect of cingulate cortex lesions on the development of resistance to stress."

Barton P. Myers "The importance of the response for discrimination learning and interocular transfer in visually deprived cats."

1964

Judith M. Bardwick "Uterine contractions as a function of anxiety, sexual arousal, and menstrual cycle." (wrote first book on psychology of women).

Paul R. Cornwell "The behavioral effects of lesions of the orbital and preoreal gyri in cats."

Robert J. Douglas "An analysis of spontaneous alterations."

1965

Donald A. Dewsbury "Some correlates of electric discharge frequency in three species of electric fish."

Richard R. Johnson "Hippocampal lesions and distractions."

Marvin E. Lickey "Slow potential (DC) response in cat interaction and conditioning."

Aryeh S. Routtenberg "Certain effects of stimulation in septal area and hypothalamus."

John W. Scott "Brain stimulation reinforcement in the runway: Effect of intertrial interval."

Amos N. Tversky "Additive choice structures."

1966

James H. O'Brien "Single cell activity in cat somatomotor cortex during classical-sensory conditioning."

Robert H. Thalman "The effect of intracranial stimulation on subsequent self-stimulation."

1967

Fred P. Valle "Effect of exposure to feeding-related stimuli on eating behavior in rats."

1968

Larry L. Bucher "Dopaminergic correlates of lever-positioning in the rat."

Gary A. Davis "The behavior of multiple molecular forms of cholinesterase."

James W. Baerwaldt "Aftereffects, partial reinforcement effect, and extinction."

Jackson T. Beatty "Visual evoked potentials: Changes induced by stimulus grouping, metacontrast, and apparent movement."

Fred. I. Leavitt "Drug-induced modifications in the sexual behavior of male rats."

Leonard W. Schmaltz "The hippocampus and recent memory loss."

David C. Wood "Behavioral and electrophysiological studies of the response decrement produced by repeated mechanical stimulation in the protozoan, Stentor coeruleus."

1969

Fred Altman "Lever positioning strategies in normal and striatum lesioned rats."

Paul K. Brandon "The effects of error-contingent time out on counting behavior of rats."

Martin D. Hamburg "Hypothalamic unit activity and eating."

Neil M. Kettlewell "Corneal deafferentiation and the development of the classically conditioned nictitating membrane."

Fredric J. Mortenson "Determinants of electric discharge rate in Gymnotus carapo, the banded knife fish."

Ralph E. Norgren "Diencephalic systems related to gustatory reinforcement."

David S. Olton "Penicillin and the hippocampus."

1970

Daniel Baran "Responses of the male Mongolian gerbil to biological odors deposited in gerbil-inhabited areas."

Daniel R. Snyder "Social and emotional behavior in monkeys following orbital frontal ablations."

William D. Timberlake "Continuous coding of general activity in the rat during repeated exposure to a constant environment and to stimulus change."

Gail D. Winger "Intravenous self-administration of alcohol by Rhesus monkeys."

1971

Charles F. Levinthal "The CS-US interval function in Pavlovian conditioning: An orienting response interpretation."

Maxime L. Stitzer "Rate-dependent drug effects on punished and unpunished responding in pigeons."

Swayzer Green, Jr. "Two behavioral methods for measuring auditory sensitivity and loudness in the squirrel monkey."

1972

Ramon C. Blatt "Septal lesion effects on avoidance behavior in rats: Task contingencies and pituitary involvement."

Carol Iglauer "Concurrent performances: Reinforcement by different doses of intravenous cocaine in Rhesus monkeys."

David H. Malin "Behavioral effects of molecules occurring in the brains of trained rats."

Edward N. Pugh "The paradoxical elevation of rod threshold after flash bleaching of the human retina."

Donald W. Rajecki "effect of prenatal exposure to auditory and visual stimuli on social responses in chicks."

1973

Mary Lou Cheal "Reproductive behavior: Regulation in trichogaster tichopterus, an Anabantid fish."

1974

Carol M. Cicerone "Impulse response and frequency response of the color mechanisms of the eye."

Patricia C. Fox "Fronto-reticular interactions in classical conditioning and evoked potentials in rabbits."

Nancy Sabin Wexler "Perceptual-motor, cognitive, and emotional characteristics of persons at-risk for Huntington's disease."

1975

Howard B. Eichenbaum "Localization of memory by regional brain protein inhibition."

1976

Bruce L. Bastain "Individual differences among the photopigments of protan observers."

James E. Leri "Neuropsychologic test performance of psychiatric and neurologic groups."

David M. Marques "Determinants of maternal behavior and cannibalism in the female golden hamster."

1977

Jeffrey J. Kassel "Behavioral functions of the telencephalon in the teleost, Macropodus opercularis."

Daniel Kurtz "Eye movement of monkeys with superior colliculus lesions during visual discrimination performance."

Maureen K. Powers "Visual sensitivity of the goldfish."

Lilian Tong "Contrast sensitivity and color opponent optic tract fibers in the Mexican ground squirrel."

Trudy A. Villars "Castration and reproductive behavior in the Paradise fish, Macropodus opercularis."

1978

Richard L. Doyle "The role of biogenic amines in an audiogenic seizure-prone genotype of Peromyscus maniculatus....."

Marilyn Kay Malott "Exposure-noise level and band width: Effects on temporary threshold shift in the Macaque."

Michael R. Petersen "The perception of species-specific vocalizations by old world monkeys."

1979

John L. Orr "Behavioral toxicology of Kanamycin."

Patricia L. Schwagmeyer "The function of alarm calling behavior in Spermophilus tridecemlineatus, the thirteen-lined ground squirrel."

Stephen R. Zubrick "Neurophysiological analysis of interhemispheric and intrahemispheric differences in men with surgically verified cerebral lesions."

1980

Seymour Herling "An analysis of specificity of drug-induced changes in drug-reinforced responding."

Judith D. K. Moreines "Ovarian estradiol and progesterone regulation of sexual receptivity of female rats."

Cynthia A. Prosen "Absolute and intensity difference threshold in the guinea pig before and after aminoglycoside-treatment and cochlear injury."

John V. Serafin "Frequency selectivity of the monkey's auditory system: Psychophysical tuning curves."

Mary P. White "The effects of melatonin on retinal light damage and photoreceptor outer segment shedding."

1981

Angela M. Brown "Dark adaptation of the long-wave length sensitive cones."

1983

Maria A. G. Feitosa "Changes in perception of loudness and absolute threshold associated with hair cell loss in the Patas monkey."

Adrienne L. Graves "Behavioral studies of spatial vision in normal and amblyopic rats."

Lynne J. Friedman "Cone antagonism along visual pathways of red/green dichromats."

1984

Alfred Mansour "Opiate responsiveness and seizure proneness induced by amygdala kindled, metrazol, or electric shock."

1985

Guy Mittleman "A psychobiological analysis on non-ingestive eating."

Barbara E. Schlumpf "Axonal competition in the goldfish visual system following optic tectum ablation."

1986

Dianne M. Camp "Enduring changes in brain and behavior produced by repeated intermittent amphetamine administration: The influence of sex and gonadal hormone."

Sandra Nagel-Leiby "Cerebral control of voluntary visual attention, directed with or without orienting saccades."

Jill M. Roberts-Lewis "Modulation of the dopamine and calmodulin-sensitive second messenger system in rat forebrain by amphetamine administration."

1987

Susan E. Bachus "Parallels in individual responsivity to electrical and chemical brain stimulation."

Edward Castaneda "The long-term consequences of amphetamine treatment on chemical and electrical stimulation-evoked striatal dopamine release in vitro."

Brad May "Significant factors and perceptual categories in the vocal communication of Japanese Macaques."

1988

Albert J. Bertalmia "Characterization of the Mu and Kappa receptors mediating the reinforcement and Discriminative effects of opioids: Apparent pA2 analysis."

1989

William D. Essman "Behavioral characterization of Sigma site compounds."

1990

Karen E. Luh "Right hemisphere dominance in the metacontrol of cerebral arousal."

Mitchell S. Sommers "Formant frequency discrimination by Japanese monkeys."

Lisa M. Kyl-Heku "Effect of context and sex on hierarchy negotiations tactics."

- 1991**
Ellen A. Walker “Apparent affinity and efficacy estimates for opioids in Rhesus monkeys.”
- Janet Mann Friedman “Home observations of high risk premature infants and their mothers during the first year of life: A micro analytic behavioral study.”
- 1992**
Xiaojuan Xu “Amnesic effects of NMDA receptor antagonist MK-801 in goldfish.”
- Sandra Comer “BWB373U86: Behavioral pharmacology of a non-peptide, systemically-active delta opioid agonist.”
- 1993**
Scott Baron “Excitatory amino-acid-induced drinking in pigeons: Pharmacological and physiological aspects.”
- Rachel Smolker “Acoustic communication in Bottlenose dolphins.”
- Howard Casey Cromwell “Functional heterogeneity of striatopallidal subregions: A lesion mapping study.”
- Bruno Laeng “Complimentary cerebral lateralization of perceptual systems underlying categorical and coordinate spatial relations.”
- 1994**
Leslie Meek “Seasonal variation in fertility in female Meadow voles.”
- Gretchen Reeves “Influences of photoperiod and ambient temperature on maternal-pup interactions in the Meadow vole.”
- Stacy Castner “Effects of amphetamine on C-FOS expression and calbindin-D28K expression in the rat dorsal striatum and associated amphetamine-induced behaviors: Sex Differences and Hormonal Influences.
- 1995**
Pamela Paulson “Time courses of changes in behavior and mesostriatal dopamine during amphetamine withdrawal.”
- Jill Mateo “The development of alarm-call responses in free-living and captive Belding’s ground squirrels, Spermophilus Beldingi.”
- 1996**
Lisa Barnes “Shared mechanisms of object-based attention and object working memory.”
- Namni Goel “Nonphotic entrainment of circadian rhythms in the diurnal rodent, octodon degus: Behavioral and neuroanatomical integration
- 1997**
Rowena Johnston “Transplantation of fetal ventral mesencephalon in a rat model of Parkinson’s Disease.”
- Li (Linda) Xiao “Estrogen modulation of the forebrain dopamine system: Neurochemical and behavioral studies.”
- 1998**
Colleen Garbe LePrell “Significance of frequency and amplitude-based temporal cues in Japanese Macaque coo vocalizations.”
- Kaitlin Browman “Psychomotor sensitization: Modulation by circumstances surrounding drug administration.”
- Rebeca Dowhan-Schneider “Female social preferences and mating behavior in captive group-living baboons (papio cynocephalus anubis): An experimental study.”
- Mei-Chuan Ko “Pharmacological assessment of thermal antinociception mediated by Kappa opioid receptors in Rhesus monkeys.”
- Susana Peciña “Contrasting roles of mesostriatal dopamine and opioid systems in food ‘wanting’ and ‘liking.’”
- Keith Williams “The naltrexone-ethanol interaction on oral-reinforced responding in Rhesus monkeys.”
- 1999**
Jillian Broadbear “Cocaine self administration and hypothalamic-pituitary-adrenal (HPA) axis activity in Rhesus monkeys.”
- Hans Crombag “Modulation of the acute psychomotor response and psychomotor sensitization by environmental context.”
- Mary Heitzeg “Egocentric body-centered coordinates modulate visuomotor performance.”
- 2000**
Karen Parker “The behavioral neurobiology of affiliation and paternal care in Microtus Pennsylvanicus (Meadow Voles)”
- 2001**
Elizabeth L. Stallman “Social behavior, foraging, and reproduction in female yellow-bellied marmots. (Marmota flaviventris)”
- 2002**
Cindy L. Wyvell “Identification of conditioned incentive salience: Mesoaccumbens substrate and incentive-sensitization.”
- William E. Fantegrossi “Behavioral and pharmacological characteristics of 3, 4-Methylenedioxymethamphetamine, its enantiomers, and related compounds.”
- Ki Ann Goosens “Conditional plasticity in the amygdala: Substrates, molecular mechanisms and the relationship to fear behavior.”

2003

Sheila Marie Reynolds “Accumbens shell organization of positive and negative motivational states: GABA and glutamate rostrocaudal gradients.”

William James Jenkins “Paced copulatory behavior in the female rat: Behavioral and neurochemical studies.”

Jason Uslander “The Influence of Context, Dose and Drug History on the Neurobiological Effects of Amphetamine and Cocaine”

2004

Lisa R. Jackson “The effects of ovarian hormones on responses to cocaine in Sprague Dawley rats.”

Kevin Corcoran “Participation of the Dorsal Hippocampus in the Acquisition, Expression, and Context-Dependency of Extinction of Learned Fear”

Stephanie Jesseau “Kin Discrimination and Social Behavior in Community-Nesting Degus (*Octodon Degus*)”

Anne-Noel Samaha “Investigating the Effects of Rate of Drug Delivery on Brain and Behaviour: Implications for Addiction”

2005

Erika Bauer “Dyadic Play Interactions and Social Relationships in Three Species of Complex Social Mammals: Domestic Dogs (*Canis familiaris*), Bonobos (*Pan paniscus*) and Chimpanzees (*Pan troglodytes*)”

Graham Flory “Factors Affecting the Regulation of Intravenous Drug Self-Administration by the Rhesus Monkey”

Jennifer Hobin “Neural Circuits for Context-Specific Expression of Pavlovian Fear Memory After Extinction”

Daniel Hummer “Development and Sexual Differentiation of the Circadian System of *Octodegus*”

Amy Tindell “Coding of Rewarded Tasks by Ventral Pallidum Neurons: Learning, Hedoni and Incentive Value”

2006

Azra Jaferi “Habituation of Hypothalamic-Pituitary-Adrenal Responses to Repeated Stress: Role of the Posterior Paraventricular Thalamus and Medial Prefrontal Cortex”

Jennifer Mohawk “Interactions Between Glucocorticoids and the Desynchronized Circadian System”

Michelle Wirth “Steroid Hormones and Social Motivation in Humans”

2007

Ziva Cooper “Effects of Repeated Morphine Deprivation on Operant Behavior in Rats”

Kyle Smith “Brain Mechanisms for Food Reward: Liking, Wanting and the Ventral Striatopallidal System”

Camille Ward “Cognition and the Development of Social Cognition in the Domestic Dog (*Colopus Familiaris*)”

2008

Vicente Martinez “A Repeated Amphetamine Model of Impaired Attention in Schizophrenia”

NEUROSCIENCE GRADUATE PROGRAM

This is an interdisciplinary Ph.D. program, which involves staff from a large number of LSA and Medical School departments. The first students were admitted in 1971 at which time the renovation of the Neuroscience Laboratory Building (NLB) (formerly the “Food Services Building” which stored food for the dormitories) had been completed. Several of the Biopsychology faculty and Cognition & Perception faculty are also members of the Neuroscience Ph.D. Program. The NLB was torn down in 2002, when the Neuroscience program offices moved into medical campus buildings. In 2007, the Neuroscience Graduate Program office moved into the newly constructed Undergraduate Science Building. The program is now directed by Steve Maren, a Biopsychology faculty member.

NEUROSCIENCE GRADUATE**STUDENTS: PH.D. THESIS**

Sponsored by Biopsychology faculty

1990

Robert N.S. Sachdev “Effect of Excitotoxic Striatal Lesions on the Discharge Pattern in Globus Pallidus and Entopeduncular Nucleus of the Cat”

Dieter Jaeger “Primate Basal Ganglia Single Unit Activity Reflects an Active Internal Model of Learned Sensory-Motor Associations”

1992

Eileen Curran “Adrenal Medulla Grafts in an Animal Model of Parkinson’s Disease: Mechanisms Associated with Recovery of Function”

1996

Paul Mermelstein “Rapid Effects of Estrogens Within the Striatum With a Possible Role in Female Sexual Behavior”

2001

Michelle M. Ostrander “Environmental Context and Past Drug History Interact to Modulate Amphetamine-Stimulated c-FOS mRNA Expression”

2003

Shelly B. Flagel “Postnatal Manipulations and Vulnerability to Drug Use”

2004

Susan Ferguson "On the Mechanism Underlying Drug-Evoked Gene Expression in Striatopallidal Neurons: Implications For Psychomotor Sensitization"

2005

Mary Torregrossa "Regulation of Brain-Derived Neurotrophic Factor mRNA Expression by Delta Opioid Receptor Agonist and Antidepressant Drugs"

2006

Carrie Ferrario "Behavioral and Neural Alterations Associated with Extended vs Limited Access to Cocaine Self-Administration"

2008

Lisa Briand "Effects of drug abuse on experience dependent plasticity"

BRIEF HISTORY OF SOME OTHER RELEVANT GROUPS ON CAMPUS

Molecular and Behavioral Neuroscience Institute (MBNI) *formerly the Mental Health Research Institute*

In 1955, Raymond Waggoner, the head of Psychiatry, (together with Donald Marquis) established the Institute and recruited James Miller as Director, Ralph Gerard, a renowned neuroscientist, and Anatol Rapoport, a mathematician with an interest in game theory. All three were from the University of Chicago. The Institute is under the administrative wing of the Psychiatry Department, but it receives separate support from the State of Michigan. In January of 1960, the MHRI building was dedicated at a ceremony that included among others G. Mennen Williams, the Governor of Michigan, Ralph W. Tyler, a distinguished behavioral scientist, and Detlev W. Bronk, President of the Rockefeller Institute and the National Academy of Science. Over the years, MHRI has acquired more wet lab space and its activities have become more biologically oriented. MBNI now focuses mostly on biochemistry and cellular/molecular neurobiology. Students should be aware of the weekly MBNI colloquia as the program often includes reports from leading neuroscientists and behavioral scientists. The Institute has been led by Stan Watson then Huda Akil since 1990 and was renamed the MBNI around 2005.

Kresge Hearing Research Institute (KHRI)

KHRI was established in 1961. The Institute includes laboratories studying vestibular neurophysiology, human and animal psychoacoustics, auditory prosthetic devices, and more. Several of the Biopsychology staff are active members of KHRI.

Reproductive Science Program (RSP)

Formerly Reproductive Endocrinology Program

This program trains graduate students and postdoctoral fellows interested in various aspects of endocrine function, reproductive physiology, and behavior. Financial support is available for some graduate students and postdoctoral fellows

pursuing research careers on relevant topics. A weekly colloquia program of lectures is held in the North Ingalls Building. Biopsychology students interested in this program should talk to Drs. Jill Becker or Theresa ("Terri") Lee, both of whom are affiliated with this group. At the time of this revision, the administrative unit for this program is under discussion.

Evolution of Human Behavior Program

After several years of informal existence, the EHB Program was established in 1986 with five years of seed money from the Rackham Graduate School. Faculty members from five different departments (or schools) and about two dozen graduate students meet regularly to discuss theory and research relating evolutionary theory to human and animal behavior. Many more staff and students affiliated with various units on campus attend the colloquia programs of lectures that have been offered over the years.

Vision Science Training Program

This is an interdisciplinary program that has a Training Grant, which provides some funding for graduate students doing research on vision. There is also a weekly colloquia program of lectures sponsored by this group.

The Hearing and Chemical Senses Program

This program has a training grant that supports students doing relevant research and it also supports a colloquia program of speakers.

AUTOBIOGRAPHIES OF SOME PRESENT FACULTY

In order to provide examples of the diverse paths that can lead to a career in biopsychology, some biographical sketches of current, former, and retired staff are presented below.

J. Wayne Aldridge

My research focuses on neuronal mechanisms of behavior with a special concentration on a region of the brain called the basal ganglia. My experimental approach is to record the activity of individual neurons during behavior with the goal of decoding the neural mechanisms. In this academic history I will relate how the inspiration and help of many individuals brought me to the Biopsychological Area of the Department of Psychology at the University of Michigan whose faculty and students are a current important source of encouragement and support.

My academic interest in behavior began as an undergraduate at the University of Toronto. Barnard (Barney) Gilmore was my first inspiring teacher. His lessons were vivid and some, such as his descriptions of operant condition, helped me in my laboratory work years later. I learned to appreciate the formalities of measuring behavior and it inspired me to pursue Psychology as my undergraduate major. Courses in perception, development, research and personality as well as an introduction to physiological psychology formed the core of my studies in Psychology. I still use my notes from Martin Wall's statistics course and occasionally I copy one of his handouts for students today. My wife, Liz, was also a psychology major and we shared many interests and courses together.

A physiological psychology led me to other courses in physiology at the University of Toronto. One was a class and laboratory on animal behavior and neurobiology in the Department of Zoology. David Dunham (ethology), Nicholas Mrosovsky (physiological psychology), and Bruce Pomeranz (neurophysiology) were the instructors. I was so captivated by the concepts of neurobiology and Bruce Pomeranz in particular that I asked him if I could do research in his lab for one year. He agreed. If I had to give only one reason in this history for why I am here today it would be my association with Bruce Pomeranz.

Bruce Pomeranz was an engaging teacher admired by his students. His joy of brain research and neurophysiology was infectious. He had a medical degree from McGill University and he did postdoctoral research at the Massachusetts Institute of Technology prior to joining the Department of Zoology at the University of Toronto. He listed Jerry Lettvin and Pat Wall among his influences. Thus in his lab, we studied pain and vision. Papers central to our discussions included those of Pat Wall on the Gate Theory of pain and David Hubel's and Torsten Wiesel's neurophysiological studies of vision. As part of his teaching, we read and discussed many classic research papers together. I was rewarded immeasurably by this personal attention.

As part of my laboratory work during this internship, I helped to design and construct multiple barrel glass electrodes and an electronic device for extracellular iontophoretic drug testing of synaptic physiology. Today, one can buy electrodes and iontophoretic instruments off the shelf, but we had to make them by hand. The electrode manufacturing process was complex and attention grabbing - even appearing on local television one evening!

The experimental work was tedious and difficult but also exciting and important. Jonathan Dostrovsky, a graduate student in Bruce Pomeranz's lab, conducted the project on which I spent most of my time. Dostrovsky and Pomeranz did a bold experiment to examine the effects of morphine on synaptic transmission in the spinal cord. At the time, opiates were not known to be neurotransmitters. I also learned a great deal from Rhanor Gillette (University of Illinois), Randolph Glickman (University of Texas, San Antonio) and John Kalaska (University of Montreal), who students in Bruce Pomeranz's lab. Harold Atwood, then a Professor of Zoology and later Professor and Chair of Physiology (University of Toronto), was another important influence. His research on synaptic transmission in crayfish was later to become important for my own Master's dissertation research.

By the end of my internship I knew that I would obtain my PhD degree and pursue a career in research. For the moment, I set aside behavior to learn neurophysiology and returned to regular classes to finish my BSc degree. The focal points of my last undergraduate year were Bruce Pomeranz's and Harold Atwood's lecture and lab courses in neurophysiology. John Kalaska and I worked together on a project studying synaptic transmission in the land snail, *Otala lactea*. The experiments were difficult and usually failed, but I learned more in that one course than I did in any other course in my career.

I entered the Master's degree program in the Department of Zoology in 1973 with Bruce Pomeranz as my supervisor. My project was to study the effects of $\Delta 1$ -tetrahydricannabinol, the

active ingredient in marihuana, on synaptic transmission. I began with the snail model to exploit its large and accessible cells with intracellular recordings. But the snails' inaccessible neuropil, which contains the majority of synaptic elements, was difficult to penetrate with drugs. After months of frustrating failures I had to abandon that and turned to the crayfish opener muscle model of Harold Atwood. In this model system, the synaptic elements are not only more accessible, but inhibitory and excitatory inputs can be stimulated independently. My last major obstacle was the highly lipophilic properties of $\Delta 1$ -tetrahydricannabinol. It was difficult to deliver in normal bathing solution. Fortunately, I was awarded a Government of Canada graduate scholarship to study the effects of drug abuse and their labs provided a water-soluble form of $\Delta 1$ -tetrahydricannabinol. I found that $\Delta 1$ -tetrahydricannabinol enhanced excitatory synaptic transmission at a glutamate synapse probably through a presynaptic mechanism. Since Bruce Pomeranz was on sabbatical, Harold Atwood supervised my thesis preparation and defense. He provided valuable lessons on writing and organizational styles.

My wife and I continued our studies at the University of Toronto. I moved to John T. Murphy's lab in the Department of Physiology for my PhD and my wife began her medical degree program. I was supported by a scholarship from the National Research Council of Canada and by teaching. John Murphy, who had trained with John Eccles lab at the University of New York in Buffalo, did neurophysiological studies of brain motor systems (cerebellum and motor cortex). At that time, Ed Evarts's (NIH) technique of recording single neurons in primates performing learned sensorimotor tasks were transforming neurophysiology and John Murphy's lab was in the process of implementing these new methods. It was an opportune time to join the lab. John Murphy's unique and interesting idea was to trace the computational transformations related to movement by comparing four different regions (motor cortex, thalamus, basal ganglia and cerebellum) with the same sensorimotor task.

The first step of exploring the motor cortex was well underway. Hon C. Kwan, a collaborator of John Murphy a great resource for me, and Y.C. Wong, his graduate student were conducting this study. Their findings produced one of the first and clearest maps of functional activation of the motor cortex. William MacKay, another colleague of John Murphy was studying the parietal cortex and cerebellum. Douglas Rasmusson, a postdoctoral fellow and Jane Macpherson, a graduate student, had also just joined the lab a few weeks earlier and they were embarked on an exploration of the motor thalamus. Rebecca Anderson, another postdoctoral fellow, had arrived shortly before me and we worked together on the basal ganglia stream of the project. The basal ganglia were the least understood of all motor regions and although we didn't appreciate it at the time it was both a stroke of good luck and a predictor of difficult times ahead.

My research on the basal ganglia became my most important and demanding teacher. Parkinson's disease, the disorder most associated with the basal ganglia, dominated theoretical discussions. Akinesia, which is the inability to initiate or maintain movement, is the major symptom of Parkinson's disease and it led to the dominant view that the basal ganglia initiated movement. Thus, we were perplexed to discover that most neuronal activity in the basal ganglia occurred after or at

the same time as the onset of movement – too late to initiate movement. Today, it is well accepted that basal ganglia do not initiate movements directly. We also found that neurons responded vigorously to salient sensory events seemingly without regard to movement direction or muscle activation patterns. Visual cues that signified the beginning of the motor task and the first or last movements in the motor task sequence were highly effective in activating basal ganglia neurons. We reported these findings as ‘complex abstractions of sensory events’, concepts that were difficult to reconcile with the mainstream of motor neurophysiology at that time. Rebecca Anderson and I also observed that basal ganglia activation seemed to be associated with the fruit juice rewards used to motivate the animals. Presenting treats (raisins) to the animals activated neurons even more effectively than limb manipulation. We submitted our findings to *Physiology and Behavior* but it was rejected and the source of considerable distress in the lab. Today reward activation in the basal ganglia is well-accepted theme but at that time I was even too intimidated to put the findings in my thesis. By listening to the data I came to realize that the basal ganglia were tuned to a much richer and broader aspect of movement than muscle activity alone. Although my findings were difficult to understand at the time, they foreshadowed my work with Kent Berridge at the University of Michigan on the role of the basal ganglia in movement sequences and motivational systems.

In 1980 I moved to the University of Alberta in Edmonton as a postdoctoral fellow in the lab of Richard (Dick) Stein. My wife began her internship and general practice residency at the Royal Alexandra Hospital in Edmonton. In Dick Stein’s lab one of my projects was to explore the variation of reflexes during the step cycle of locomotion. Monosynaptic reflexes might seem to be one nervous system feature that could be viewed as constant, but guided by Dick Stein’s clever analytical approach we showed this idea was incomplete. During normal stepping movements reflexes are modulated in a way that make biomechanical and functional sense i.e., the reflex properties are enhanced or diminished according to when the reflex might assist or impair the movement. The lessons I learned here were important to me. I learned that well focused analytical evaluation could reveal important information. The enormous power and flexibility of the brain to modulate its own circuits also impressed me. If the simpler circuits of the spinal cord can manipulate something as stereotyped as reflexes during locomotion, imagine the enormous power of cortical circuitry in modulating more complex behavior.

In 1982 I moved to the Department of Neurology at the University of Michigan to work with Sid Gilman, Professor and Chair of Neurology. Here I have pursued an understanding of clinical models of neurological disorders and more recently, returned to a study of behavioral correlates to explore the basal ganglia and its role in motor control. Two graduate students, Dieter Jaeger and Robert Sachdev worked closely with me and have made a significant impact on my progress. More recently, I have had the good fortune to join the Department of Psychology and work with Kent Berridge. Together we have shown clearly that neuronal activity in the basal ganglia depends not only on movement but the behavioral context in which it occurs. Our findings suggest that the basal ganglia may be playing an important role in the execution of behavioral sequences. Motor

behavior may be only one of the sequences in which the basal ganglia participate. Currently, I am working with my graduate student, Amy Tindell, and Kent Berridge on an exciting project to examine the role that ventral basal ganglia structures play in processing rewards. Andre Snellings, a graduate student in biomedical engineering is working on a collaborative project with David Anderson in my lab to record neural activity in different regions of the basal ganglia as part of a project to enhance the localization of targets for neurosurgical procedures in Parkinson’s patients.

Brandon Aragona

As an undergraduate at West Virginia University, I was primarily interested in what influenced behavior and therefore majored in psychology. I realized in my freshman year that I would need a Ph.D. to pursue my curiosities in depth and the goal of my undergraduate years would be to identify which field within psychology was right for me. Unfortunately, the WVU psychology department had no neuroscience labs, but there were many excellent behavioral labs and I was able to get some great experience.

I conducted behavioral pharmacology studies in rats and pigeons and learned a tremendous amount from Kent Parker. I also wanted to try out clinical research, so I joined Kevin Larkin’s lab and examined the relationship between family history of hypertension and cognitive performance. Kevin encouraged me toward a career in scholarship and recognized that I was not going to be happy unless I was doing more biologically based work. He recommended that I spend the next summer working with a clinical neuropsychologist, Marc Haut, administering tests to drug addicts and patients with dementia. Although I admire people that can handle the challenges associated with clinical work, this experience made it clear to me that I belonged in a lab. The following semester I took ‘Physiological Psychology’ and had many helpful discussions with the professor, Irv Goodman, and I had found the research field that was best for me. Even though I had never worked in a neuroscience lab, I was positive that I wanted to pursue a Ph.D. in behavioral neuroscience.

However, I had a weak science background and didn’t feel that I was quite ready for graduate study in neuroscience. So, I stayed at WVU for a 5th year and took several biology and chemistry courses. I was happy to do this because my partner, Michelle, had one year left at WVU and we didn’t want to separate. I applied to a handful of neuroscience programs that put a very strong emphasis on behavior and was lucky to be accepted by some of them... especially since I had never even seen a brain in person. While I was very excited about neuroscience, I had no idea what specific research I wanted to do. So, I was relieved to get a phone call from Mike Rashotte, who was recruiting for Florida State University. He said that FSU welcomes students with broad interests and to narrow one’s focus to behavioral neuroscience was actually pretty good. I felt an instant connection with Mike and the FSU program. I have no doubt that FSU was the right choice for me. The collaborative atmosphere, the talent of the faculty, and their dedication to training independent scientists makes FSU a top notch program.

While choosing FSU was easy, I had a lot of trouble choosing a lab. Given my lack of neuroscience experience, I would have benefited from a rotation system. At FSU, students

were accepted into specific labs, but were encouraged to work in other labs to broaden their experience and learn additional techniques. I narrowed my choice of mentor to Fred Stephan (who studies food entrained circadian rhythms in rats) and Zuoxin Wang (who studies pair bonding in prairie voles). I decided to officially join Fred's lab but also learn neuroanatomical and neurochemical techniques from Zuoxin (which I would apply to my research on circadian rhythms). Under Fred's mentorship, I received an NRSA to examine the expression of circadian 'clock' genes around an anticipated meal time.

Fred and I were also conducting a study on time-place learning and we collaborated with Zuoxin to measure tissue concentration of dopamine within the nucleus accumbens during this behavior. While working in Zuoxin's lab, I became even more interested in his research. I soon found prairie vole research much more interesting than the circadian rhythms work I was funded to do and I decided to talk to Fred and Zuoxin about switching labs. Fred encouraged me to pursue my interests and said that he would be available if I needed anything. I transferred to Zuoxin's lab and, since Fred continued to serve as my co-mentor, we were confident that I would be able to transfer my circadian rhythms NRSA. However, I was told that I would need to relinquish this grant and write a new one. So, I wrote an NRSA to examine dopamine regulation of pair bonding. Thankfully, this grant was also funded.

Zuoxin's lab was a perfect fit for me. While I was sure that I wanted a career in research, I wasn't sure that it was possible until I found a specific line of research about which I was very passionate. With extraordinary help from Yan Liu, Tom Curtis, and Christie Fowler, Zuoxin and I published a series of papers detailing the complex regulation of pair bonding by dopamine transmission within the nucleus accumbens. Additionally, I was significantly influenced by 'Psychoactive drug use in evolutionary perspective' by Randy Nesse and Kent Berridge. This increased my desire to examine drug reward in prairie voles and compare dopamine regulation of pair bonding and drug abuse. Zuoxin encouraged me not only to do the experiments, but also to apply for funding to support the projects. He allowed me to serve as a co-PI on two grants that we wrote to examine the effects of abused drugs on pair bonding. Both grants were funded and Zuoxin and I continue to collaborate on studies of the interactions between social and drug reward in prairie voles. Zuoxin prepared me for a career in science above and beyond anything I expected and my time in his lab was as enjoyable as it was productive.

When it was time for me to choose a post-doctoral mentor, I considered what I wanted my research program to be (when I had my own lab) and what scholarly and technical training I needed to achieve that goal. My dissertation was based on dopamine research but I had a very poor understanding about how in vivo dopamine transmission really worked. Also, I was very interested in the neural regulation of drug reward but had little experience in this field. I therefore chose to work with Mark Wightman and Gina Carelli at the University of North Carolina. Mark is an analytical chemist who established sub-second measurements of dopamine concentration (using fast-scan cyclic voltammetry) in freely moving rats. Gina's expertise is in freely-moving electrophysiology studies during cocaine-self administration. Mark and Gina had recently teamed up to

measure dopamine during cocaine seeking and their collaboration seemed like a perfect opportunity to learn about dopamine transmission and drugs of abuse.

At the time I started my post-doc, very few studies had measured sub-second dopamine fluctuations in behaving animals. This was therefore an extremely exciting time with surprising data and frequent discussion and debate within our group. In my first project, I showed that cocaine directly increased phasic dopamine release events in a sub-region specific manner within the nucleus accumbens, which revealed a novel consequence following cocaine intake. Additionally, I received an NRSA to examine the development of conditioned dopamine release to cues predictive of cocaine delivery. We showed that cue-evoked and cocaine-evoked dopamine release is also regionally specific within the nucleus accumbens which provided new insights into the role of dopamine during the acquisition of a learned drug association. I greatly appreciate Mark and Gina for allowing me to join the group as well as everything I learned from many other members of the group including: Mitch Roitman, Joe Cheer, Paul Phillips, Donita Robinson, Michael Heien, Bob Wheeler, Jeremy Day, and Josh Jones.

During the 3rd year of my post-doc, I received emails from both Zuoxin and Mark letting me know that there was a posting for a professorship in the psychology department at Michigan. I considered this a dream job but was worried that it had come one year too soon, since I was not nearly ready to leave UNC. Nevertheless, I was encouraged to apply and was honored to receive the offer. I deferred my start date for one year and this allowed me to finish my work at UNC and better prepare to start my own lab. During this process, I received invaluable help and mentoring from Jill Becker. I'm very grateful to her and everyone that helped me to become a member of the Biopsychology faculty at Michigan. I believe that this is the best possible opportunity for me to grow as an investigator.

Jill Becker Luma

I did my undergraduate work at the University of Kansas in Lawrence, Kansas. I majored in Human Development and Family Life. This was a unique department that had started as a Home Economics Department, but evolved into a department that studied development of behavior from every imaginable perspective. At the time there were developmental psychologists investigating cognitive development, social development, neural development and the use of Skinnerian behavioral modification techniques to enhance learning in preschool and school age children. One group of investigators, led by Dr. Don Bushell was implementing individualized instruction and the use of positive reinforcement techniques to enhance learning in K-3rd grades. This effort was part of the Follow Through Program, an attempt made in the early 1970's to continue the advantages begun with the Head Start Program. I had experience with programming lessons for use in K-3rd grades when I was in high school and throughout my undergraduate studies. I worked for Project Follow Through as a Research Assistant.

I had originally intended to get a Ph.D. in Developmental Psychology or possibly even Clinical Psychology. However, because of my work with the Follow Through Program, I became more and more intrigued by individual differences in behavior. Here we were, using techniques that some people

feared would turn people into robots who all behaved alike, and just the opposite was happening. When children were allowed to learn at their own rates, we found there was a larger range of abilities (not smaller). In one classroom where I was working, there were two boys in the Kindergarten class who were reading at the 6th grade level. In the same class, with the same teacher, there was a boy who still could not read one word. This observation had a profound effect on me. It said to me, “there are individual biological differences in the ability to learn.”

This event occurred during my senior year as an undergraduate. I had placed out of all science and math courses, so I had not taken any natural science courses during my undergraduate career. I was not academically prepared to begin a graduate program in the biological basis of behavior, nor did I even know at that time that the topic was of interest to me. I decided to take a year off, and work for Project Follow Through full time. After the first 4 months, I knew I was not going to be happy outside of an intellectually challenging environment. So, I realized that I needed to round out my scientific background, and started taking classes part-time. I took Biology, Chemistry, Organic Chemistry, Physics, and Physiology over the next 18 months. Then, I started graduate school in Human Development, with the idea that I would somehow be able to apply my interests in the biological basis of behavior.

Early in my first semester in graduate school at the University of Kansas, I went to talk with one of the professors in Human Development, Dr. Elias Michaelis. This was a pivotal even in my life. As Eli Michaelis began telling me about his research and studying the brain, I knew that this is what I wanted to study. I began working in his laboratory the next day. I was extremely fortunate to have the support of both Don Bushell (my former mentor) and Eli Michaelis in this move. While in Michaelis laboratory, I overlapped 1 year with William J. Freed who was just completing his Ph.D. with Michaelis. I completed my Master’s Thesis research, investigating the effects of protein synthesis inhibition on learning in rats, in less than 1 year. As it became clear that I was going to finish my M.A. soon, Michaelis recommended that I apply to a Neuroscience Program where I could get more thorough training. I applied to only one school: The University of Illinois at Urbana-Champaign, and was accepted.

I started graduate school in the Neural and Behavioral Biology Program (now known as the Neuroscience Program) at the University of Illinois in 1976. I began over the summer in the laboratory of Dr. V. D. Ramirez. Ramirez was investigating the control of luteinizing hormone releasing hormone (LHRH) by catecholamines in the medial basal hypothalamus (MBH). I began studies to investigate whether there was gonadal hormone modulation of catecholamine release from the MBH. For these initial experiments we used the striatum as a control tissue. To our surprise, we found that there was a dramatic effect of ovariectomy on striatal dopamine release, but no effect of ovariectomy on catecholamine release from the MBH. This result has led to many exciting experiments and is a line of investigation that I continue to pursue today.

During my stay at Illinois, I did a rotation in the laboratory of Dr. William T. Greenough in collaboration with Dr. C. Sue Carter investigating glucose utilization in the hamster brain during lordosis. While we failed to keep the spinal cord,

which turned out to be the active area to the CNS during lordosis in the hamster, I learned many valuable techniques during this rotation. I also learned to appreciate the field of Neuroscience from a broader perspective by attending Greenough’s lab meetings, which I continued to do the rest of the time I was at Illinois.

As I was finishing my dissertation research, I met Terry E. Robinson at the Society for Neuroscience meeting in St. Louis. We corresponded between Urbana-Champaign and Ann Arbor for the rest of the academic year, and then collaborated on a project looking at sex differences in striatal dopamine function in the rat over the summer of 1979. This article formed the background for a grant application by Robinson and a NRSA post-doctoral application by me. I received my Ph. D in 1980 and found out shortly thereafter that both the grant and the doctoral fellowship were funded.

I spent three years at UM as a Post-Doctoral Fellow. After that I was appointed Assistant Research Scientist in Psychology. I began submitting grant applications, for a federal funding on my own research. It was definitely a learning process (I mention this so as to encourage other young scientist not to quit when faced with rejection). I think I wrote a new grant for every deadline I was eligible to apply for over an 18 month period of time. A very small grant from the NIMH was funded to study the mechanisms of neurotransmitter release. Then, in 1984, I received a grant from the NSF to continue the research that I had begun with my dissertation, investigating gonadal hormone influences on striatal dopamine activity. This line of research has been funded by either the NSF or NIH since that time.

In 1985, Dr. William J. Freed visited the University of Michigan to speak to the Biopsychology Colloquium about his research investigating the use of dopamine producing cells to alleviate the effects of dopamine denervation in the animal model of Parkinson’s Disease. During his visit he asked whether I would like for him to demonstrate his grafting techniques. At first I could not think of any reason to learn how to do brain grafts, but as I thought about it I realized that the techniques that Terry Robinson and Freed, I wrote a grant proposal to study ‘Brain Tissue Transplantation: Neurochemical Indices of Recovery of Function’ which was funded by the NIH. In fact, I only asked for 2 years, and the study section recommended 3 years of funding (which I gladly accepted). This line of experimentation also served as the basis for a Research on the topic continues to be supported by NIH.

I joined the Biopsychology faculty at the University of Michigan as an Assistant Professor in 1987, and was promoted to Associate Professor in 1992. I continue to investigate both gonadal hormonal influences on striatal dopamine function and the neurochemical basis for recovery of function and the neurochemical function in an animal model of the Parkinson’s Disease. In addition, I have recently edited a book to be used for an upper level undergraduate course on Hormones and Behavior (J.B. Becker, S.M. Breedlove and D. Crews., Eds. Behavioral Endocrinology, MIT Press, 1992)

Jacinta Beehner

I did my undergraduate studies at Boston College as a biology major in the early 1990s. My training was primarily in molecular biology with a focus on plant genetics. My larger

academic goals were somewhat altered, however, following a semester spent abroad in Kenya. In Kenya, I was introduced to many of the large social mammals of the African savanna, and I came back to Boston College wondering how I might be able to incorporate animal social behavior – and more specifically, African fieldwork on animal social behavior – into my day-to-day research. After finishing up my honors thesis on plant genetics in 1994, I subsequently sought out graduate programs that might accomplish this.

I found a research project, directed by Dr. Clifford Jolly and Dr. Jane Phillips-Conroy, that focused on the evolution of social behavior of wild baboons. Much to my surprise, research on primates was rarely under the research umbrella of biology departments anymore, and I soon found myself in an Anthropology Ph.D. program at Washington University in St. Louis. With an evolutionary bend to my coursework, I developed a Ph.D. project that focused on female reproductive success in a population of hybrid baboons living in Ethiopia. To maximize my ability to measure female fitness in a short period of time, I sought out additional training in behavioral endocrinology, mainly from Dr. Patricia Whitten at Emory University (where I was trained in RIA procedures). At the time, non-invasive methods of measuring steroid hormones were just being developed, and I knew I was on the cutting edge of what would soon become routine for behavioral ecologists. For my Ph.D. thesis, I spent 1.5 years in the Rift Valley of Ethiopia following around a group of baboons - a location that I would later find out was only a few hundred kilometers from the “hottest place in the world” according to annual temperature records. When I wasn’t following baboons up and down a canyon or pulling acacia barbs from my skin, I was lying flat on the floor of my house, hoping the day’s heat would leave my body and enter the concrete. I finished up my fieldwork in 2000 and quickly headed to Emory University to complete my laboratory hormone work. I had intended to finish my Ph.D. within the year, but, as luck would have it, I was thrown my next opportunity for fieldwork.

Before I’d even recovered from the culture shock of re-entry to the U.S., Thore Bergman (my fellow graduate student and future husband), now finished with his Ph.D., was offered a postdoc position with Drs. Robert Seyfarth and Dorothy Cheney in the Psychology Department at the University of Pennsylvania. The postdoc involved at least two years of fieldwork living in a remote camp in the Okavango Delta of Botswana studying the communication and cognition of chacma baboons. It was truly the postdoc that anyone in our shoes would dream about. Without too much effort, Thore talked me into joining him in Botswana after I’d finished my labwork. Thankfully, I had a flexible advisor who saw the benefits of my association with this postdoc position (even though for me it would officially only be a “pre-doc”). She allowed me to put my thesis “on hold” until after we returned from Botswana. I’ll let Thore expand on the details of the research itself (see Thore Bergman’s entry), but I was able to continue my studies on baboon hormones and behavior – an extension of much of what I did for my Ph.D. thesis (or thesis-to-be, I should say). I began two projects, one investigating the relationship between dominance rank, aggression, and testosterone in male baboons and another investigating psychological stress experienced by female baboons when a new (potentially infanticidal) male joins the group. I believe I learned

more about animal behavior (and certainly animal cognition) in those two years than I had during all my other years of coursework. As a researcher conducting all her work on foot with no weapons, horns, teeth or claws, I also learned several survival skills as well (even now, I still look around for snakes when I hear bird alarm calls).

Thore and I returned to the University of Pennsylvania in 2003 to write up the results of this research. I also picked up the pieces of my thesis again, and seven months later I turned in the final draft of my Ph.D. thesis. During this time, I was offered a postdoc (this time I could actually call it a “post” doc) by Dr. Jeanne Altmann at Princeton University. It was perfect – if I could have chosen anyone in the country to work with, it would have been Jeanne Altmann, who worked on hormones and behavior in Kenyan yellow baboons. And the fact that Princeton was only an hour from the University of Pennsylvania was too good to be true. I spent the next two years mostly writing but also conducting more hormone research in the Altmann laboratory on yellow baboons. During my two years at Princeton, I finished two projects – one describing the fecal hormone profiles of pregnant female yellow baboons and the other evaluating female reproductive failures (i.e. anovulation, failure to conceive, or miscarriage) as potential reproductive strategies – rather than anomalies.

In the spring of 2005, I was offered a tenure-track position in the Psychology and Anthropology Departments at the University of Michigan (half-time appointment in each department). Thore was offered a position in Psychology at the same time. We accepted the positions, but delayed our starting dates by one year so we could get our next research project off the ground. With initial funding from the Wildlife Conservation Society, we spent the next year in Ethiopia (this time, high up in the mountains where nighttime temperatures frequently dip below freezing – a welcome relief from the Rift Valley). During this year, we established a fieldsite studying geladas (a close relative of baboons) in the Simien Mountains National Park. We always knew that geladas were the perfect study system for both of our research programs because (1) they have complex social groups and one of the most diverse vocal repertoires of all non-human primates (perfect for Thore’s work on cognition and communication) and (2) they also have sexually-selected signals (a red patch of skin on the chest and loud display vocalizations) that are almost certainly mediated by hormones.

Currently, I am involved with two projects – one investigating sexual signals in male geladas and the other investigating sexual signals in female geladas. Ultimately I am interested in larger phylogenetic comparisons of hormones and behavior across all Papionins (the taxa that includes both baboons and geladas, as well as several other primate species) and eventually across primates more generally. I feel fortunate at the University of Michigan to have several other colleagues conducting similar research to my own including Dr. Thore Bergman, Dr. Terri Lee, and Dr. Barb Smuts in Psychology, Dr. Liz Tibbetts in Ecology and Evolutionary Biology and Dr. John Mitani in Anthropology.

Thore Bergman

Both of my parents were teachers, so my summers growing up were spent camping and fishing. Probably because of

this, I have always enjoyed being outdoors and trying to figure out how the natural world works. It was not until I was finishing my undergraduate degree that I realized I could combine these interests into something that passed for a career. As I progressed through high school, I enjoyed science classes but I thought the only real scientific careers available were as medical doctors. So, that is what I thought I wanted to be. A year at Johns Hopkins as an undergrad surrounded by other pre-med students made me realize I didn't want to be a physician. I transferred to the University of Wisconsin at Madison and majored in Zoology and Conservation Biology. The courses (especially the hands-on upper level biology classes) were exciting but I still wasn't sure what to do after I graduated. It hadn't clicked that my professors that taught me about interesting behavioral research actually did interesting research as a viable profession. Before my senior year I participated in two summer research projects (on the ecology of aquatic plants and whale feeding behavior). It was only by seeing research in action that I realized it was possible to study the natural world as a career. I knew I wanted to do biological research, but I was still not sure what kind.

I went to graduate school in the Evolution and Population Biology Program at Washington University and started out doing a short project with Dr. Jane Phillips-Conroy who studies a baboon hybrid zone in Ethiopia. A semester of measuring baboon tooth-casts led to an invitation to spend a summer in Ethiopia trapping baboons. I knew immediately upon my arrival in Ethiopia that I wanted to continue working with the baboons. Here were animals that were constantly engaged in complex social interactions and I wanted to figure out why. For me, the most interesting aspect of this "why" question has always been adaptive. How do the behaviors help the animals survive and reproduce? The hybrid zone was a good place to study these questions, because in one group you could find a variety of baboons with very different behaviors. I picked the most varied group at the center of the hybrid zone and followed them for a year and a half. After returning, I spent most of my time in Dr. Clifford Jolly's genetics lab at New York University. Using a combination of behavioral and genetic data, I was able to document the behavioral variation, relate it to measures of hybridity and then look at how these different behaviors translated into reproductive success. I completed my Ph.D. on this project in 1999.

After habituating and observing a group of baboons, I learned a lot about them including their dominance relationships and their consistent social companions. I was even able to guess at who was kin – even before I had the genetics to back up these guesses. Just as I knew all these things about the baboons, I also came to realize that the baboons seemed to know many of these same things about each other. This realization got me interested in the cognitive challenges of living in complex social groups. How much information do animals keep track of? And, how might this information benefit them? I was fortunate enough to be able to pursue these questions through a post-doc at the University of Pennsylvania working with Drs. Dorothy Cheney and Robert Seyfarth. Cheney and Seyfarth pioneered much of the research on primate social cognition, particularly with their innovative use of playback experiments. The fieldwork for the post-doc took place in the Okavango delta of Botswana, one of the few truly wild places left on earth. In between staring

down lions and dodging elephants, I was able to do my own cognitive research on the baboons using playback experiments. The results of this research demonstrated that baboon's social knowledge is more sophisticated than people had previously realized. This experience was valuable in bringing a rigorous experimental approach to my behavioral questions, allowing me to address the cognitive aspects of social behavior. It also got me interested in communication. Conducting playback experiments requires recording lots of vocalizations, and paying attention to vocalizations makes very clear the importance of this (often overlooked) aspect of social behavior.

As I became more interested in social cognition, I began to read about the relationship between social complexity and cognitive evolution. Doing so, made me realize that there was species of primate that would be perfect for addressing my questions, the gelada (*Theropithecus gelada*). Geladas are close relatives of baboons, yet they have a different social structure and diet. Geladas live in multi-level societies and can be found in the largest naturally occurring groups of non-human primates in the world (>1,200 animals). Also, geladas have a much larger vocal repertoire than baboons. Furthermore, very little is known about geladas, while baboons are probably the most well-studied non-human primate in the world (perhaps only eclipsed by chimpanzees). Therefore, a comparison of geladas and baboons could yield valuable insights into the evolution of social cognition and communication. Even better, geladas are found only in Ethiopia, a country I was already very familiar with. After finishing my post-doc at Penn in 2005, I established a field site in the Simien Mountains of Ethiopia, along with my wife, Jacinta Beehner. We continue to study the geladas full time.

At the same time we were establishing the field site in Ethiopia we were fortunate enough to be offered jobs at the University of Michigan in Biopsychology. We came in 2006 and have been very happy with both the intellectual and social aspects of Michigan Biopsychology. I still feel like I just arrived, but so far, so good!

Josh Berke

I was exposed to ideas about the mind and psychopathology from early on - my father is a psychotherapist who moved to the UK in the 60s to study with R.D.Laing and the "anti-psychiatry" movement. Growing up I assumed I would be an academic, but I was most interested in theoretical physics, and that is what I started to study as an undergraduate at Cambridge University. As it turned out Cambridge was one of the very few places in the UK where budding physicists were expected to study other sciences as well, and I progressively became more interested in biology and experimental psychology, ending up focusing on neuroscience (my senior thesis equivalent was on nerve growth factors and the survival of cholinergic neurons). Towards the end of Cambridge I felt I wasn't ready for a Ph.D, especially as the 3-year programs in the UK involved no course work, just working in a fixed lab, and I hadn't identified a lab yet. I applied for a new 2-year scholarship that had just been set up by the Daiwa Anglo-Japanese Foundation to get potential future leaders in various fields to learn Japanese and go work in Japan for a while. I ended up at the University of Tokyo in the neurophysiology lab of Yasushi Miyashita, a prominent memory researcher. However I didn't get that much done, largely because

every morning was still devoted to Japanese language instruction, and the afternoons I would often spend in seedy Go clubs filled with retired men who smoked too much. I was not a very good student, especially by Japanese standards. I did become quite fluent in Japanese, but I have a terrible memory (for all kinds of things) and I no longer remember much.

While in Japan I applied to various graduate schools in the US. I was quite clueless both about the reputations of specific schools, and about matching my interests with those of specific faculty. I ended up going to the Neuroscience Program at Harvard, even though Harvard was strongest at molecular biology stuff that I wasn't especially into at the time (MIT might have been better). Harvard did have the considerable side-benefit that I met my future wife there (Anna Grzymala-Busse, now also on the UM faculty, in Political Science).

I did a couple of lab rotations in the Psychology Dept before ending up working with Steven Hyman at Mass General Hospital. Although his lab was doing molecular biology and nothing very behavioral, he had impressed me in the graduate core Neuroscience course with an engaging, open response to questioning and discussion of mental illness. Also, MGH had just recently pioneered the functional MRI technique and Steve leveraged this into a large NIDA grant to study human brain activation following cocaine infusion. This was a fairly irresistible project to a young graduate student, as it involved legal intravenous cocaine infusions into human patients and fancy color images of their brains. One of my more memorable roles was to be the guinea pig for the crash test procedures, designed to see how fast they could yank someone out of the scanner if the cocaine caused a heart attack (fast enough to be quite painful). However, since fMRI was new, and this was a large team project, there was concern that it wasn't clear how I could carve out my own distinct thesis, and so I began also working on molecular biology projects in the lab. A fairly recent development was the ability to monitor changes in neuronal gene expression produced by drugs or other manipulations, and this was a major focus of the Hyman lab. Along with Eric Nestler he had been promoting the idea that persistent changes in gene expression could underlie the behavioral changes of drug addiction (and other disorders). Although they had been quite successful looking at known genes in key brain structures such as the striatum, it was easy to imagine that they were also lots of important unknown genes. Steve developed a collaboration with Chip Gerfen at NIH, to use a molecular technique called differential display PCR (DDPCR) to identify such novel genes, and that became the heart of my thesis project. In particular we were aiming to look at the gene expression consequences of removing striatal dopamine (a model of Parkinson's Disease) because in this condition the striatum was known to become super-sensitive to dopamine, and this effect did not seem to involve alterations in dopamine receptor numbers. Shortly after I began this project Steve was appointed head of the National Institute of Mental Health, and started the process of moving his lab down to Bethesda. Since I was already collaborating with Chip, I moved down first and worked in the Gerfen lab while also getting things set up in the new Hyman Laboratory of Molecular Plasticity (a terrible name, I thought). DDPCR is a finicky technique that involves the careful extraction and amplification of mRNA from brain tissue following by running very large gels that were always setting too fast or not

at all. Since this was before any mammalian genomes were sequenced, this identification of mRNA fragments was followed by an even more finicky PCR-based procedure for sequencing the full-length mRNA, called RNA-ligation mediated RACE. By the end I could design PCR primer sequences in my sleep (a skill I have now also completely forgotten). Though very supportive, Steve's mind was clearly far more on administration (a few years later he left NIMH to become Provost at Harvard, and now doesn't maintain a lab).

The gene expression changes following dopamine removal were fairly minimal, and we didn't discover the basis of dopamine supersensitivity. But I had decided to include as a form of positive control a one-hour timepoint following dopamine agonist administration, and we found lots and lots of rapidly induced genes in this condition. Somewhat to our surprise they pretty much all were back to baseline within 24 hours. They were all also switched on by acute cocaine, and in addition, there was a great deal of overlap with genes that were being identified in screens for possible synaptic plasticity - related factors. Largely this is because various ways of increasing neuronal activity tend to produce a similar set of induced genes, but it did get me thinking. What if the relevant gene expression changes in drug addiction (or L-DOPA-induced dyskinesias in Parkinson's Disease) were transient - leaving their mark in the form of synaptic rewiring? In this way the persistence of drug-induced behavioral change would simply mirror the persistence of normal learning, because they both relied on the same molecular mechanisms. I ended up writing a review on this subject that Steve and I were able to get published in *Neuron*, and was quite influential.

My interest in learning processes had been rekindled, and I was also now convinced that one could spend many lifetimes studying the nitty-gritty of specific genes without getting a handle on how addiction worked. It was time to move to a different level of analysis, and I wanted to study how the brain processes information differently as a result of synaptic rewiring. I had seen some cool presentations of single-unit recording in rats, and so behavioral electrophysiology looked like the way to go, despite meaning a change in subfield. Anna had just secured a tenure-track job at Yale, but was also very keen to defer this to spend another year at Harvard under a prestigious post-doc. So even though I would have preferred to move out West, I was looking at East Coast post-docs. After checking out various labs, I went to work with Howard Eichenbaum at Boston University. The core of our project was to study the formation of "habits" - highly practiced routines for performing actions without needing to think about them. I had been thinking (I was not the first to do so) that over-strong drug-induced habits might explain persistent "compulsion" in drug addiction. We set out to compare neural activity in different memory systems that control behavior, specifically the more "cognitive" hippocampus-based system and a more "habit" striatum-based system. Largely on the basis of my addiction review, Howard's reputation, and it being the "fat" NIH years at the end of the Clinton-era expansion, I was able to rapidly get first a post-doc NRSA grant, then my own RO1 to become quasi-independent. Howard was a fantastic mentor, and to this day continues to be generous and supportive. The down side is that (like Steve) he was no longer a bench scientist actually doing experiments, and

just as I arrived at BU the only people in Howard's lab who were successfully doing electrophysiology left for jobs in Scotland. So I spent a couple of years doing things completely wrong and learning from my mistakes before generating quality data. Partly because I didn't know just what I was supposed to be looking for and analyzing, I became interested in some of the striking electrophysiological phenomena that are present in the raw brain signals. These include various forms of oscillation, and the fact that there seemed to be different kinds of striatal neuron with different spike shapes and firing patterns. I ended up publishing some papers on these subjects, and as it turns out the larger electrophysiology community is increasingly interested in how different types of interneurons regulate the timing of projection cells and their participation in oscillations, so this is becoming an ever more mainstream interest.

Anna and I were very fortunate to get several joint offers at major universities. Although Anna was happy at Yale and had just received tenure there, after considerable discussion we decided to move to Ann Arbor. This was largely because of the intellectual climate for me - in particular the presence of Kent Berridge and Terry Robinson. Kent had previously written me a kind letter about another addiction review I had written - despite the fact that it argued strongly against their incentive-sensitization theory - and I was impressed by such intellectual generosity and openness.

I came to UM at the end of 2004. I was determined to "do things right" so we spent a lot of time (probably too much) designing our own electrophysiology rigs and writing software. Now the lab is finally running well and we have been generating interesting data and papers. I've also been busy with all the usual stuff that junior professors do - teaching, committees, grant-writing, and Anna and I now have two young sons, Conrad and Casimir. My main intellectual focus now is trying to determine the underlying computations that enable behavior, especially learning and decision-making. Specifically, how do particular components of neural circuits contribute to the overall algorithms performed by those circuits? This is a level "below" most behavioral electrophysiology or imaging studies, that have been (mainly) concerned with determining what representations are present in the brain, and where. But it's a level "above" most electrophysiology studies in brain slices or anesthetized animals, since it's become obvious to me how radically different the dynamics of neural activity are in awake behaving animals. In fact, a lot of what we've discovered so far is that seemingly robust in vitro phenomena often don't happen when alert brains are examined. My interest in brain dynamics has led to collaborations with faculty in UM Physics, and I am now myself a (barely deserved) member of the UM Center for Theoretical Physics, which in a sense is coming full-circle intellectually. After spending so much of my adult life trying to master one difficult technique after another, I was certain that I wouldn't want to bring other techniques into my lab for a long, long time. But after a lot of hesitation I've now realized that some new optical techniques for manipulating neurons in vivo are too powerful, and mesh too well with our existing questions and techniques, to be avoided. So now we're embarking on bringing those on board too - in competition with a lot of bigger labs. Now if I only had time to really think...

Kent Berridge

I decided to become a biopsychologist during my first weeks as a undergraduate at the University of California at Davis in Fall 1975, in my first course in introductory psychology. I had chosen Davis because I was interested in behavior, physiology, and evolution (but did not know that psychology contained an experimental branch that addressed those topics), and UC Davis had a veterinary school and a curriculum that seemed appropriate to such topics. The first term of introductory psychology was focused on biological psychology, and was team taught by 2 comparative psychologists and 2 physiological psychologists. Within weeks of beginning the course, I discovered that the material and approach was exactly what I was looking for. By mid-semester I had declared my major as psychology with a biology minor (the UC Davis equivalent of a Biopsychology & Cognitive Science concentration), and tentatively decided to pursue it as a career. Since then, I've always felt it was the right decision.

Though focused at that point on biological psychology, I was yet undecided whether to specialize in physiological or in comparative psychology. I was interested in both. While at Davis, I worked in William A. Mason's large comparative primate group on a project on social and cognitive development in rhesus monkeys raised that had been with dogs as surrogate mothers, and on another project on species differences in social behavior among new world monkeys. I also worked in Leo Chalupa's lab on visual electrophysiology of the superior colliculus.

As a sophomore, I applied to the university's "education abroad" program to spend my junior year in England. My advisor, Don Owings, who is a comparative psychologist studying the evolution of communication and social behavior in ground squirrels, recommended I apply to Sussex University (among the 9 British universities that had relations with the University of California) because Sussex had at that time strong biopsychology and neuroethology programs. The year at Sussex University was excellent in every way, filled with new experiences. Intellectually, there were 4 courses that stand out. One was "Instinctive Behavior", an advanced course organized by the Ethology and Neurobiology faculty (including Richard Andrew, Peter Slater, and Tim Roper), which combined classical ethology with a strong physiological emphasis. Another was "Animal Learning" taught by Nicholas Mackintosh, an excellent introduction to what is perhaps the most analytical of psychological topics, associative learning. General laws of learning had been somewhat frowned on at Davis, as old fashioned and not sufficiently evolutionary; Mackintosh's course showed me that learning theory approaches could be quite powerful and exciting. A third formative course was "Philosophy of Psychology" taught by Margaret Boden, a philosopher of mind who had an artful way of guiding a small-group discussion to force students to discover new points on their own. I had long held a minor interest in philosophy and had taken several courses at Davis -- but not enough, perhaps, because I remember Boden's final evaluation of me as "tries hard, though philosophically naive"). The fourth important experience was a biopsychology research project in Richard Andrew's lab on hemisphere lateralization and visual learning by chicks. That research experience cemented my aspiration for a research career in biological psychology.

Upon return to Davis for last undergraduate year, I had one more chance to accompany Don Owings for several days to do field research on ground squirrel social behavior in the Sierra foothills. Although fun in many ways, and although I found the topic fascinating, I was struck by the difference in my reaction to field work (which left me often bored) compared to laboratory work (which I enjoyed). I just didn't seem to have the right sort of patience for field observation. The long hours of watching, waiting for the minutes in which something important would occur, struck me as tedious to experience, though interesting to think and read about. I was still interested in field issues, but decided I should focus on laboratory research topics, where data could be collected more easily and efficiently.

I was not pulled strongly to any single research topic for graduate study, but thought I would focus on physiological psychology of motivation. A visiting instructor at Davis, Alan Gibson, who studied sensory and motor physiology, recommended that I consider studying with Harvey Grill, who worked on the physiology of hunger but used "neuroethological" techniques, and who had recently become an assistant professor at the University of Pennsylvania. It was fortunate advice. The scientific apprenticeship with Harvey and the general milieu of Penn's department of psychology both turned out to be excellent and formative experiences.

At Penn in the early 1980s was an excellent interdisciplinary group of faculty focused on the biopsychology of motivation (e.g., Randy Gallistel, Paul Rozin, Richard Solomon, Norman Adler, Elliot Stellar and Alan Epstein). In addition, the department of Psychology at Penn was an extraordinarily stimulating environment. Students there were encouraged to achieve in their chosen research topic, but also to broaden their familiarity with other fields and to think critically in general. The general intellectual climate at Penn seemed to me to be enthusiastic and supportive, and I remain grateful for the experience.

During my first semester there, I did a short side-project with Richard Solomon on opponent processes and imprinting in chicks (it never amounted to much, but was exciting to interact with Dick Solomon on his famous theory). Then I returned to working with Harvey Grill using the taste reactivity technique and intravenous sampling technique to measure behavioral aversion and to show that neuroendocrine suppression of insulin secretion to the taste of food was caused by taste aversion conditioning (my first year graduate project, the equivalent of a 619). Gradually, my interests began to focus specifically on affective reaction as opposed to hunger per se, and I focused on taste reactivity as a naturalistic affective reaction. My eventual dissertation dealt with whether positive versus negative affect evoked by a stimulus (taste in my case) were processed separately by the brain (which was my conclusion), or conversely, were two poles of the same affective dimension (the more traditional view since Wundt). This is a question still of interest, which pops up in other areas of psychology too (for example, in studies by Cacioppo and colleagues). Also, as a consequence of working on affective expressions, I began to be interested in how states like affect were translated into patterns of behavior via motor "control rules". This too remains a question of interest.

To pursue that question after finishing my Ph.D. in 1983 I went to do postdoctoral work with John Fentress, a

Cambridge-trained ethologist with strong roots in psychology and neurobiology, at Dalhousie University in Halifax on the coast of Nova Scotia in Canada. I spent 2 years working with John on motivation and sensorimotor integration topics at Dalhousie. This began the projects on action patterns which continue now almost two decades later in collaborations with Wayne Aldridge and others.

John Fentress also kept a colony of wolves in a semi-natural compound out in the forest in the middle of Nova Scotia. I enjoyed many visits there but didn't pursue any projects with John's wolves, somewhat to my regret now. (A related regret: just before leaving Penn for Dalhousie, David Premack began a project with a new group of infant chimpanzees at his facility in the Amish country near Philadelphia. I went out to visit one day with a friend who was a postdoc with Premack. I watched as the infant chimps greeted her with apparent delight, jumping on her and hugging with absolute absorption. It was a touching sight and cute beyond description. I toyed with the idea of staying on an extra year to work with Premack on chimp cognition, but dismissed it as impracticable and too much of a divergence. Now, if all would have turned out the same, I wish I had done it. In general, graduate students and postdocs always face a choice between expanding their range of projects versus focusing and achieving progress in depth. There is no single correct way; each choice involves trade-offs, and perhaps there is no way to avoid all regrets.

In 1985, Michigan offered an assistant professorship position in biopsychology, which I was delighted to receive. The job market had been poor in the early 1980s, and I hadn't really expected to succeed in finding a job without several more years of postdoc experience. As a general rule, in your own early career, I think it is best to be flexible as you make plans: try for jobs early, and be prepared for a range of outcomes.

Over the years at Michigan, first in the Neuroscience building and then at East Hall, I've enjoyed working with a series of fine graduate students. Each student has followed her or his own unique trajectory, and it has been a great pleasure to watch their development.

Another source of satisfaction within the area has come from interactions with fellow Michigan faculty. Several collaborations in particular have stretched into what are becoming decades. One has been with Terry Robinson, which began with an initial research project on mesolimbic dopamine's role in "wanting" versus "liking", and which evolved into the incentive-sensitization theory of addiction. It occasionally continues to spur new collaborative research or theoretical papers. Another long-standing research collaboration has been with J. Wayne Aldridge on action syntax, especially on the electrophysiological coding in striatum and related brain systems of behavioral patterns. That collaboration has recently extended to studies of limbic coding of incentive motivation too. Interactions, conversations, & lunch discussions with other faculty both in Psychology and other departments have also been a tremendously important. The high quality of collegueship and intellectual exchange is one of the high points of life at Michigan.

Henry Augustus ("Gus") Buchtel

My introduction to science came in high school as a research assistant in an electro microscopy lab at one of the

hospitals in Denver, where I grew up. I think that the attention to detail and the every-day experience of seeing things that were hidden far below the surface attracted me to studying the brain and behavior. I did my undergraduate work at Dartmouth College in Hanover, NH. At Dartmouth, I was a research assistant for Tom Landauer, who piqued my interest physiological psychology through his course "Physiological Psychology" (using Hebb's "Organization of Behavior"), and an article he wrote that year for Psychological Review on the possibility that memories were coded in RNA. The research I carried out for him was on learning abilities of hamsters that had been affected by a virus resulting in atrophy of one layer of the cerebellum. In the department at the time were Wolfgang Koehler, Bill Smith (brother of the more-famous K. U. Smith), Roger Elliott, Tom Tighe and others, all of whom shaped my interests toward physiology and visual perception.

Mainly because of Rogers Elliott's influence (he had been at McGill as a post-doc), I went to McGill University in Montreal for graduate school. My first year project was on the role of the cerebellum on learning in rats, stimulated by first year seminars taught by Don Hebb and Ron Melzack (of Melzack/Wall gate control of pain fame). I continued to work on the cerebellum for my Ph.D., granted in 1969, under the mentorship of Bob Malmo (a 1950's activation theorist, more recently known for work on osmoreceptors in the hypothalamus.) My lab was in the research wing of the Allen Memorial Institute on Mount Royal, overlooking downtown Montreal. This part of McGill gained a bad reputation for trying some of the unsuccessful "cures" for depression and schizophrenia in the 50's and 60's ("depatterning" for example), about which you will find references in Elliot's "Great and Desperate Cures." During those years at McGill, I was loosely associated with the psychologists at the Montreal Neurological Institute, mainly through my friendship with Phil Corsi, one of Brenda Milner's students and a fellow Dartmouth graduate. Phil's dissertation work on the effects of temporal lobectomies in epileptics undoubtedly helped to lay the groundwork for my current interest in this fascinating patient group.

After McGill I wanted to work in a physiology lab as a post-doc and I took Melzack's advice to go to Pisa to work with Giovanni Berlucchi in Moruzzi's Institute. We made single-cell recordings in cats with artificial strabismus, and studied their perceptual abilities with each eye alone. I climbed the you-know-what more times than any Pisano, and liked the setting and work so much that I eventually stayed three years, first supported by an IBRO/Unesco grant, then by teaching psychology to the medical students.

During this period I became acquainted with Elizabeth Warrington at the National Hospital for Nervous Diseases at Queen Square, London, and went to working her department for three years, where I learned human neuropsychology and tried (unsuccessfully) to do some work with humans with cerebellar lesion findings with the spreading depression technique in rats. In London, Tim Shallice's research assistant was my office mate, Margaret Evans. We were married at the end of my three years there and the two of us went for another three-year period to Italy, this time with Giacomo Rizzolatti at the Institute for Human Physiology in Parma. Because of my experiences in London, I increased the amount of work I was doing with

normal and brain damaged humans, though we also carried out many studies of single unit activity in the cortex and superior colliculus of monkeys and cats. I was again teaching psychology to the medical students to supplement my post-doc stipend. An opportunity to return to Montreal and work in Brenda Milner's group led to us moving again, this time with a 2 year-old daughter in tow. At this point I began to work on patients with surgical removals for the control of epilepsy, finding that the right hemisphere plays a role in recognizing the face, regardless which cerebral hemisphere receives a face stimulus. I also started studying the effects of frontal lobe lesions on attention, and discovered that frontal lesions interfere with the ability to look away from an interesting stimulus.

Two and a half years later, after almost ten years of post-doctoral training (!), I saw an ad for a neuropsychologist to work at the VA Medical Center here in Ann Arbor, with faculty appointments in the Departments of Psychiatry and Psychology. I've continued the work on the frontal lobe influences on attention, but have branched out into positron emission tomography, consciousness during the Wada Test (in which one cerebral hemisphere is anesthetized temporarily), and the effect of depression on learning.

Geoffrey E. Gerstner

My interest in animal behavior dates back to my childhood, when I had the good fortune of being raised literally in the Wasatch National Forest of northern Utah. I turned this interest into a Zoology degree at BYU, where I was told that "you cannot do anything with a Zoology degree but become a doctor or dentist". Believing this, I assumed college would be my last time to learn about the natural world before I became part of the establishment as a professional. But the evolutionary biology classes I took presented me with existential crises that could not be resolved by graduation time.

Consequently, I decided to pursue a dental degree at UCLA because becoming a dentist was my original pre-crisis plan, and because UCLA would let me simultaneously pursue MS and DDS degrees. This would buy me time to resolve my crises without restricting my career choices. So I went off to UCLA in the hopes of studying bat echolocation, becoming a dentist, and perhaps gaining closure on my philosophical crises.

At UCLA, I worked with renowned and successful scientists. What caught my attention was that they were not only doing science, but they were actively dealing with my crisis issues in a scholarly way in the form of a study group. The leader of this group was A. Iberall, a brilliant physicist/engineer, who had strongly influenced the likes of Warren McCulloch, Rudolpho Llinas, Scott Kelso, Michael Turvey, Ralph Abraham, Richard Bellman, Arnold Scheibel, and others. The group consisted of biologists, psychologists, sociologists, engineers, physicists, mathematicians and others.

One of the most important things I learned from this group was that science was the best thing yet invented for understanding the natural world. I also learned that although scientists make great effort to understand their test subjects, to know the literature in their fields, to interpret experimental results objectively, and to calibrate their instruments, sensors, and probes, it was the human brain/mind that designed experiments, interpreted results, formulated hypotheses, filtered "good" from

“bad” data, and decided what were “hot” scientific topics and what ones were not. And science knew very little about this living brain/mind instrument and knew even less about its critical role in science or any other human endeavor for that matter.

I decided that one could not know the significance or meaning of anything without understanding the brain/mind. This meant to me that understanding the human brain was the only intellectually honest way out of my existential crises. Consequently, I gave up the idea of working on bat echolocation, and settled into Dr. Louis Goldberg’s laboratory, a neurophysiologist who trained with Clementi. In his lab, I worked with Dr. Kirstie Bellman, an ethologist who trained with Ted Bullock and Walter Heiligenberg and who was the daughter of the father of dynamic programming, Richard Bellman. The mix of physiology and ethology gave me exactly what I was after, and I graduated from dental school with a master’s degree in hand, having developed an animal behavior model of tardive dyskinesia.

I determined to remain in academics and to pursue a degree either in psychology or neuroscience. Fortunately, the NIH announced a special award whereby graduating dentists could be funded for five years and receive doctoral and dental-specialty training simultaneously. In other words, I could continue my mixed lifestyle for another five years. I applied for and received this award, and subsequently obtained a doctorate in neuroscience while specializing in human orofacial pain and temporomandibular disorder management. I was very much interested in the chemistry and physiology of brain function, but I also felt strongly that a real understanding of the human brain/mind would require a synthetic approach. By “synthetic”, I meant two things. First several scientific philosophies would have to play a role in my work. My philosophy has been strongly influenced by the neurophysiologist, L. Goldberg, by the classical ethologists (Lorenz and Tinbergen), by A. Iberall’s homeokinetics, by Thelen and Smith’s dynamic systems approach to studying cognition and action, by Gibsonian ecological psychology, among others. The second meaning of synthetic was that my research would strive to reassemble the “Cartesian clock” as opposed to disassemble it, as was traditional in neuroscience.

So, I studied free-roaming animals at the San Diego Wild Animal Park, San Diego Zoo, Los Angeles Zoo, and Phoenix Zoo in order to develop assays of motor behavior that provided insight into the physiological dynamics of mammalian brains. The major tenets of my work were: (1) an animal is a set of dynamic complex biochemical processes internally partitioned by semi-permeable cell membranes and mutually compartmentalized from the external environment by semi-permeable partitions (skin, lungs, gut) and inextricably connected to the environment by sensor, respiratory, ingestive/digestive, and reproductive systems, (2) the animal’s internal biochemistry settles into dominant activity regimes, i.e., when an animal is hungry little else matters, (3) the efferent limb of the nervous system is a probe of many of these dynamic processes, (4) motor behavior is the major manifestation of activity in the efferent limb, thus (5) motor behavior is a sensitive and useful assay of these internal processes, the challenge is to learn to read the assay in “meaningful” ways.

From a personal perspective, this project provided me the opportunity to reunite my interest in animal behavior with my

interest in brain-behavior issues. From a professional perspective, the project allowed me to develop methods for studying behavior. These included: (1) morphometric analyses of behavior kinematics (kind of a sophisticated hand-writing analysis that quantifies the shapes traced out by body segments as an animal moves in its environment), (2) methods of defining “unit” motor behaviors, and (3) frequency domain analysis of motor behavior streams.

After completing my doctorate in 1992, I moved to the University of Michigan as an assistant professor. Since being here, I have studied how orofacial pain affects oral behavior patterns in humans, how ambient temperature affects the motor behavior of poikilothermic vertebrates, and the nature and timing of intermittency in rodent mastication and locomotion.

My current studies include: (1) Human bruxism--we are interested in the neurophysiology and neuropsychological correlates of bruxism (night-time clenching and grinding) and the neuropsychological, (2) Chewing-rate jaw-mass correlates--there is a power law relating chewing rate to animal size in mammals. I have documented this power law for about 200 mammalian species. We are currently looking at how artificially increasing the mass of the rodent jaw influences chewing rate, muscle mass, and skeletal structure. (3) Development of high-tech means of measuring behavior in free-roaming subjects (human and animal). The goal is to obtain signals from free-roaming subjects using low-impact, unobtrusive sensors, and to use these signals to monitor behavioral and physiological parameters in test subjects. Currently, we are evaluating electromyography, and data obtained from position sensors and accelerometers. Data from these sources is fed into artificial neural networks and the goal is to have the neural networks correctly identify test parameters. We are using our bruxing and rodent chewing rate studies as test beds for developing these systems.

Theresa M. Lee

I received my undergraduate degree from Indiana University in Biological Sciences in 1975. As an undergraduate I was interested in animal physiology and behavior. When I found that I would not be one of the 7 women accepted at Purdue University’s Veterinary Program, I was at a loss for what to do next. I had been married for 2 years by this time, and my husband was ready to return to school and finish his undergraduate degree. I found a job as a pharmacology analyst working for the animal testing laboratory in a small pharmaceutical company outside Chicago, where I remained for the next three years.

My job introduced me to the possibility of research as a career. A couple of years later when my marriage was ending and the company I worked for was going out of business, I applied to the University of Chicago’s Biopsychology program because it allowed biologists to study behavior. In my application I said I was interested in studying toxicological effects of drugs on early development. Howard Moltz read my application, called and asked if I might be interested in studying normal development. It sounded interesting to me, so off I went in 1978 to spend 4 years determining how mother and rat young interact to time the production of the rat maternal pheromone, and what benefits are gained by pups responding to the odor.

To carry out my graduate research I was fortunate to work with several faculty in addition to Howard Moltz at the University of Chicago and Northwestern Medical School, including Martha McClintock, Jerre Levy, Allan Rechtschaffen, Chung Lee and Godfrey Getz. Going to the Conference on Reproductive Behavior each year introduced me to a wide group of researchers interested in reproductive and maternal behavior and physiology including Jay Rosenblatt, Frank Beach, Norm Adler, Donald Pfaff, Neena Schwartz, Bob Goy, John Vandenberg and on and on.

During graduate school I became increasingly interested in studying ecological variables that might influence reproduction and development. Martha McClintock introduced me to Irving Zucker and Fred Turek, both were studying the role of photoperiod on development at the time. One visit to Irv Zucker in California told me that 10 years in the Chicago area were enough. I successfully competed for a NIHCHD postdoctoral fellowship and went off to Berkeley, California in late 1982.

I remained in Berkeley as a postdoctoral fellow for 3 years and as a research associate with Irv Zucker until September of 1988 when I came to the University of Michigan as an Assistant Professor. While in Berkeley I developed three research interests: the role of seasonal environmental variables (maternal photoperiod history, current photoperiod, temperature and food) in controlling early growth patterns and the timing of puberty, development of circadian activity and temperature rhythms, and the mechanism underlying entrainment of circannual rhythms in golden-mantled ground squirrels. During this period in Berkeley, I was fortunate to interact with Frank Beach, Steve Glickman, Marc Breedlove and Paul Licht among others. Working with Irv Zucker also introduced me to the international community of researchers interested in biological rhythms, including Jurgen Aschoff, Colin Pittendrigh, Eberhard Gwinner, Bruce Goldman and many of Irv's former students that remain in the field of rhythms or reproduction.

During the years in Berkeley I also met my husband, Jack Love. Our first child, Shoshana, made a somewhat untimely arrival on Rosh Hashana in 1985 when Jack was supposed to be helping lead services at the Berkeley Hillel where he was Associate Director. Because Jack had lived in California for about 15 years and never wanted to leave, I tried valiantly to find a job in California, but eventually convinced Jack that the tornadoes of the Midwest couldn't be any more threatening than the earthquakes of California.

Since my arrival in Ann Arbor, my research has continued to explore various aspects of chronobiology and the intersection of that field with development and reproduction. Many undergraduates and three graduate students (Leslie Meek, Gretchen Reeves and Karen Parker) explored the interaction of photoperiod with a wide variety of behaviors in meadow voles. Other students have worked to develop the *Octodon degus* (degu) as a diurnal circadian model. Sue Labyak, Namni Goel and Tammy Jechura, along with many undergraduate assistants, have determined how degus use light, social odors and access to activity wheels as entraining agents. We find that how degus are synchronized with the external light: dark cycle, and how they recover from simulated jet-lag is highly sexually dimorphic, but only in adults. We are exploring the development and neural mechanisms of the circadian system of degus and believe

that they represent a reasonable model for the human diurnal circadian mechanism.

My years in Ann Arbor have been increasingly busy as I developed classes, gained students in the lab and became involved in other roles at the university and in the community. In 1991 Jack and I had our second child, Ephraim. We have definitely reached the limit of different things we can manage, but life is always exciting at work and at home.

Stephen Maren

I joined the Biopsychology area at the University of Michigan in the fall of 1996 as an Assistant Professor in the Department of Psychology. The general theme of my research relates to the neural substrates of mammalian learning and memory. I am particularly interested in the synaptic mechanisms underlying learning and memory for emotional events. I address these issues using a variety of neurobiological techniques in behaving animals, and fear conditioning in rats serves as my experimental model system. My current research interests, theoretical perspectives, and empirical models represent the convergence of several important training experiences.

I began my research career in 1987 as an undergraduate honors student and Edmund J. James scholar in Dr. Michael Gabriel's laboratory in the Department of Psychology at the University of Illinois. I met Mike as a student in his introductory biopsychology course. I took the course on a whim, primarily because I had started to lose my enthusiasm for the coursework I was taking as a bioengineering major. To my excitement, I was absolutely engaged by the biopsychology course, and in particular, the section Mike taught on the neural mechanisms of learning and memory. After the course, I approached Mike about the possibility of working in his lab and expressed my strong enthusiasm for the work he was doing -- he invited me join the lab. I jumped into the lab work enthusiastically. I started doing all of the usual tasks relegated to undergraduates, but I really had my eye on taking on my own project. I was reading the literature feverishly and cornering anyone in the lab that I could find to bounce around ideas, clarify things I had read, and discuss experiments. Fortunately, both Mike and his graduate students were receptive to this -- they were always ready to sit down and talk science. After a semester or so in lab I had already learned a number of procedures, and I was really itching to get involved in a project on the amygdala that I had been bouncing around with Mike. The main hurdle, though, was performing surgery. Mike used rabbits in his lab, and his research tended to be "K-selected". By this I mean that a great investment of energy and time was put into individual subjects with the hope that a single animal would yield an abundance of data. For this reason, only Mike and the graduate students performed rabbit surgeries. As a consequence, the undergraduates in the lab would only assist on projects that either Mike or one of the graduate students was running. But I really wanted to run my own project so I badgered Mike about learning surgery. Mike finally relented and he personally taught me how to perform stereotaxic surgical procedures for implanting multiple depth electrodes in the rabbit brain. After learning the surgical procedure I was off and running on my own project. My project examined the involvement of the amygdala, a temporal lobe structure known to be involved in emotion and learning, in the acquisition of instrumental avoidance learning in

rabbits. I found that neurons in the amygdala changed their firing properties during the course of learning, and acquired learning-related firing patterns much earlier than other brain structures previously implicated in avoidance learning. This work formed the basis for my honors thesis, and I later published the work in my first scientific paper. In retrospect, it is without question that this lab experience defined my career, because it was in Gabriel's lab that I fell in love with the neuroscience of memory.

After obtaining my bachelor's degree in 1989, I joined Dr. Richard F. Thompson's lab at the University of Southern California for my graduate studies. Dick had appointments in both the biology and psychology departments, and I was admitted to USC in the Section of Neurobiology in the Department of Biology. I chose to go to USC to work with Dick. He had an international reputation for his pioneering studies on the neural substrates of eyeblink conditioning in rabbits. Moreover, he was instrumental in founding the field of neurobiology of learning and memory. This alone would have been reason to join his lab, but I must admit that the lure of sunny southern California played a role in my decision to go to USC, too! My work at USC focused on a form of synaptic plasticity known as long-term potentiation (LTP), which is an enduring increase in synaptic efficacy that is thought to underlie learning and memory in mammals. The opportunity to do LTP work was not what I expected insofar as Dick was primarily involved in mapping learning circuits in the cerebellum. But it turned out that Dr. Michel Baudry had recently joined the USC faculty and he and Dick were collaborating on some basic projects examining LTP and its relationship to learning. Before I arrived, everyone doing LTP work was working in the hippocampal slice preparation. I was interested in determining to what extent LTP at synapses in the hippocampus was mediated by changes in postsynaptic receptors for the excitatory neurotransmitter, glutamate. For these experiments, we needed to induce LTP *in vivo*. This turned out to be an outstanding opportunity for me, because I was charged with setting up a technique that the lab had not previously used. I had to do substantial background reading, equipment shopping, and troubleshooting, and after about a year I finally had the *in vivo* electrophysiology suite up and running. I performed several experiments examining LTP induction *in vivo*, and the effect LTP induction had on glutamate receptor populations. The prevailing view at the time was that LTP was mediated by an increase in the presynaptic release of glutamate. I showed that LTP was also associated with an increase in the number of postsynaptic glutamate (AMPA) receptors. I also collaborated with Dr. Denis Mitchell on the involvement of hippocampal LTP in behavior. Denis was instrumental in getting me to think about behavior and the importance of nonassociative learning in behavior. The independence I had in Dick's lab was a real asset to my career development. It allowed me to establish myself as an independent investigator and thinker. It was also a great learning experience to do science in a lab that had 15 graduate students and several post-docs and visiting scientists. I should also note that my wife, Naomi, and I met at USC. She was also a graduate student in neurobiology and worked on synaptic physiology at frog neuromuscular junctions. She studied hormonal regulation of these synapses in *Xenopus* forearm muscles, because male *Xenopus* use their arms to clasp females for extended periods of time during mating. Before we started dating, I would often

joke with her that the NMJ wasn't interesting because it didn't show LTP. She would point out that grabbing and holding onto a female was probably a more important function for a synapse -- I guess she was right.

After graduating from USC in 1993, I joined Dr. Michael S. Fanselow's laboratory in the Department of Psychology at UCLA as a postdoctoral fellow. The years I spent in Michael's lab were extremely productive and rewarding. Together, Michael and I published over 18 papers in a three-year period. Michael's lab was involved in examining the neural substrates of fear conditioning in rats. I took his expertise in synaptic physiology and LTP and applied it to the neural circuitry underlying fear learning. I discovered that synapses between the hippocampus and amygdala exhibited LTP, and provided behavioral evidence that these synapses were important for fear conditioning to contextual cues (i.e., the place where fear is experienced). I also demonstrated the importance of the amygdala and amygdaloid LTP in fear conditioning in another series of experiments. In addition to the amygdala LTP work, I also explored the role of the hippocampus in the acquisition of fear memories. Using very selective neurotoxic lesions of the hippocampus, I confirmed earlier studies that hippocampal lesions disrupt fear conditioning to contextual cues. I also extended this work in an important and novel direction by showing that the timing of the lesions relative to training was critical for revealing behavioral deficits. Only lesions made after training produced behavioral deficits -- lesions made before training were without effect. This work made a critically important theoretical contribution to the field, which is that contextual learning can be mediated by different strategies, only one of which requires the hippocampal system. Michael's lab was just an outstanding place for me to do a postdoc. In addition to mastering fear conditioning, I learned a great deal about theory and experimental design. Michael was top-rate when it came to designing experiments to address complicated theoretical questions. I acquired a strong appreciation for the importance of learning theory in addressing empirical questions concerning neural mechanisms of learning and memory. Michael's lab was also a smaller and more tightly knit group. He had only three graduate students, including Stephan Anagnostaras who was an undergraduate in Terry Robinson's lab before going to UCLA. Moreover, the environment for learning and memory research in Southern California was outstanding. There were investigators interested in learning and memory at USC, UCLA, Cal Tech, UCSD, and UC Irvine. Both Fanselow and Thompson were members of UC Irvine's Center for the Neurobiology of Learning and Memory. So I routinely attended small Center meetings at Irvine and the larger Learning and Memory conferences. This was a great opportunity to interact with all of the prominent investigators in the field.

In the fall of 1995, I began to look for a faculty position and sent out around 10 applications to a variety of top-notch programs. I was invited for interviews by three psychology departments at Johns Hopkins University, the University of California-San Diego, and the University of Michigan. Terry Robinson chaired the search committee at Michigan, and they were hiring to fill a position vacated by Elliot Valenstein's retirement. The Michigan job was a perfect fit for me, and I was ecstatic when they offered me the position. Naomi and I

moved to Ann Arbor in July 1996, and I started to equip my newly renovated lab space in East Hall in the fall of 1996. I was running experiments early in 1997. My work at Michigan continues to address the synaptic mechanisms of learning, and I am now using single-unit recording techniques to examine the neural coding of stimulus representations during fear conditioning. I have also started a new line of research that examines the role of the hippocampus in contextual memory retrieval. I have been able to selectively target memory retrieval processes with reversible inactivation techniques. Several undergraduate and graduate students are involved in running these projects. In addition to my research activities, I teach courses in psychology and neuroscience including Introduction to Biopsychology and Neurobiology of Learning and Memory. I'm also an active member of the interdepartmental Neuroscience Program, and am teaching one module of the new neuroscience core course for incoming neuroscience graduate students. Outside the lab, our new baby girl, Emi, is keeping Naomi and I busy. I hope to have her working in the lab in the not too distant future!

Bryan E. Pfingst

My interest in biopsychology began when I was an undergraduate at the University of North Carolina and I came across a book of readings entitled Neurophysiological Psychology. I had long had an interest in the cases of human and animal behavior but little idea about how to approach the problem. After reading this book, I realized that this was the way I wanted to pursue my studies. Interestingly, when I was a senior, taking a course in Physiological Psychology I read another article which was later to have a strong influence on my career, though I didn't realize it at the time. This article, in *Science*, was by Blair Simmons at Stanford, who had implanted an array of electrodes in the auditory nerve of a deaf patient and produced hearing by electrical stimulation of the nerve. My research interest at that time was in the area of learning and memory, and it was not until many years later that I would recognize the relevance of Simmons work to my career in biopsychology.

I completed my undergraduate training at the University of North Carolina and then stayed there for graduate school. Many people feel it is advantageous to change schools at this juncture to gain a broader range of experience. In my case, I stayed at the same school, but changed faculty mentors. Kurt Schlesinger, who had been a strong influence in my undergraduate training moved to the University of Colorado about that time and I began working with Dick King, studying the effects of electro convulsive shock on memory. About the time I completed my Masters' thesis on that topic, Paul Shinkman arrived as a new faculty member in experimental psychology and interested me in pursuing learning mechanisms using single-unit recording techniques. Paul had received his Ph.D. at Michigan under James Olds. The goal of my work with Paul was to see if single neurons in the sensory cortex could be modified using operant conditioning techniques. Electrical stimulation of the brain "pleasure centers" was used as the reinforcer.

My doctoral research experience left me with some strong biases about how to study brain functions. First, I believed that it was important to study brain activity in as normal a preparation as possible; i.e. an awake subject, free of general anesthetics or other drugs that would affect the central nervous

system. Second, I felt that it was important to use a preparation that was as close as possible to humans. My goal was to obtain knowledge that would be of benefit to humans, particularly in a biomedical context. Third, I had a strong bias toward single-unit electro physiology. These biases led me to seek a postdoctoral training with someone doing single-unit recording in awake primates that were performing behavioral tasks. I applied to two programs on the west coast and ended up at the University of Washington where Joe Miller was beginning a program of single-unit work in auditory cortex at the Regional Primate Research Center.

The work in Seattle reinforced the view that the encoding of information in sensory cortex was strongly influenced not only by the anesthetic state of the subject, but by the behavioral and attentional states as well. I became interested in relating the activity in auditory cortex to the animal's hearing, including hearing and single neuron activity measured on a trial-by-trial basis. This led to an interest in the techniques for measuring sensory function behaviorally. The techniques developed by Bill Stebbins and David Moody at Michigan were a strong influence at this point, and I began to study these techniques in monkeys and in human subjects. It was this interest in the animal psychophysics that led me to the next and current phase of my career in biopsychology.

About the time we completed the studies on psychophysical procedures in monkeys, the National Institute of Health was attempting to foster research programs to examine the auditory prosthesis more carefully. The pioneering work of Blair Simmons at Stanford, William House in Los Angeles, and Robin Michelson in San Francisco had indicated that electrical stimulation of the auditory nerve might be a viable technique for the treatment of profound deafness, yet little was known about the long-term risks associated with the technique, or the details about the hearing produced by these prostheses. The psychophysically trained primate seemed an ideal preparation for answering some of these questions, and we were awarded a program project grant in 1976 to study the bionic ear.

I have continued on the work auditory prosthesis since that time. The primary focus of my work has been on psychophysics, but the theoretical basis for this work has been on the neural mechanisms underlying the observed behavior. I find this to be the most interesting and rewarding area of research I have ever done. Almost all of the profoundly deaf patients receiving these implants benefit from partial restoration of hearing, and some patients can hear so well with the device that is difficult for the casual observer to tell that they are profoundly deaf. Still, there is the challenge to better understand how information is coded in the electrically stimulated auditory system, to understand the reorganization of the deaf auditory system and how it affects hearing, and to make the device work better.

Work on the auditory prosthesis is truly an interdisciplinary effort. The international group of researchers working on this device comprises about 200 individuals, many of whom have been in this discipline almost from the beginning. It includes anatomists, physiologists, psychologists, engineers, speech scientists and there are strong ties with several high-tech businesses. We have maintained satisfying and productive collaborations with many individuals throughout the world during

this process.

In 1984, three of the Seattle research team, Joe Miller, Ben Clopton and I, moved to Michigan when Joe Miller, the PI on the cochlear prosthesis program project, became director of the Kresge Hearing Research Program Institute. A clinical program begun here in 1986 is now the largest in the United States and includes both an adult and a children's component. Many of the patients who received these implants are hired as and aids in further research. Thus each day we see the benefits of the science and the challenges for the future.

Terry E. Robinson

I took my first course in physiological psychology from Ian Whishaw in 1970 at the University of Lethbridge, as a junior. I transferred to Lethbridge from Concordia University in Montreal, mainly because I wanted to see Western Canada. The University of Lethbridge had been established just a couple of years earlier as a four year liberal arts college, and the brochure made it sound like an interesting place. It is in beautiful southern Alberta, about 45 miles from the Montana border, with a view of the Rocky Mountains to the west and the endless prairies to the east. At the time I really didn't know what I wanted to do with my life, and southern Alberta sounded like an attractive place to spend a year or two before having to work for a living (I didn't know what a graduate school was at this stage, and didn't realize it was possible to avoid 'work' even longer). The University was in the process of recruiting new faculty and Ian Whishaw arrived in 1970 as well, fresh from his Ph.D. program. I enrolled in his physiological psychology course just because it sounded interesting. I didn't know at the time that it was the first course he ever taught. It was a leaning experience for everyone. The class had about 20 students, and to survive we formed study groups, in which much of our time was spent trying to decipher what Whishaw was talking about in class (and doing his take-home exams that had lots of questions like, "what color is a red fish?; define 4", etc.). I don't think he had yet learned the difference between graduate and undergraduate students. But I found the material fascinating, and about half way through the term I approached Ian and asked him where one had to go to get involved in the kind of research he was talking about in class. I assumed he would tell me Toronto or Montreal, to the one institute in the country where brain research was done, but instead, he told me to come by and see him Saturday morning at 8:00. I learned stereotaxic surgery that Saturday, and hardly left the lab for the next two years. I now knew what I wanted to do when I grew up. Lethbridge had one of the experimental programs in vogue at the time, in which students didn't take classes, but 'contracted' to work towards their degree in a variety of ways. In my senior year, I did full time research, and left Lethbridge with three first-author publications. Most of the work involved the effects of posterior-lateral hypothalamic lesions on movement initiation and correlated changes in neocortical and hippocampal electrographic activity, although we also published a paper comparing the effects of electrolytic anodal and cathodal lesions and iron deposition on postoperative behavior.

For my Ph.D. I wanted to work with Ian's graduate mentor, and to continue to study the relationship between hippocampal and neocortical electrophysiology and behavior. But C. H. (Case) Vanderwolf (at the University of Western

Ontario) was not taking any new students the year I applied, and so I didn't get in. Instead, I went into a Master's program in physiological psychology at the University of Saskatchewan in Saskatoon and worked with Tom Wishart, who was a former student of Gordon Mogenson (from the University of Western Ontario). My most salient memories of Saskatoon are the weather (there was a period of over two weeks where the highest temperature was -20 degrees F), my statistics class, and having to test rats in the animal room. I had not taken statistics as an undergraduate and was thrown into a year-long class for all Psychology graduate students. All of the students had already had 2-3 undergraduate statistics courses. We covered nearly all of Winer's Statistical Principles in Experimental Design that year. After failing the first exam, I assumed my graduate career was over. But I worked like a dog for the rest of the year and ended up with an A. I am really proud of that A, although I must confess I never took another stats class. I also managed to develop a good allergy to rats that year, probably because I stood in the animal room for 5-6 hours every day watching rats with lateral hypothalamic lesions swim in a big pool of water.

I completed my Master's research project in 8 months, and went back to Lethbridge for the summer to continue what turned out to be a life-long collaboration with Ian Whishaw. I received a fellowship from the Canadian Non-Medical Use of Drugs Directorate for the summer to do experiments with the hallucinogen, LSD, although not much ever came of that project. In the fall I was off to the University of Western Ontario physiological psychology program to work with Case Vanderwolf, who was a Donald Hebb student (as are many Canadian physiological psychologists of that era). At Western, Case had begun a series of experiments that were redefining the nature of the reticular activating system; the ascending inputs responsible for electrographic activation of the neocortex and hippocampus. As part of that research program, I began a series of experiments involving simultaneous electrical stimulation and recording to study brainstem influences on neocortical and hippocampal electrical activity during both waking behavior and sleep, including the role of cholinergic and catecholaminergic inputs. My thesis committee, which was formed a few weeks before I was about to leave, included, in addition to Case: Gordon Mogenson, Mel Goodale, Nancy Innis and as an outside member, J. S. Schwartzbaum of the University of Rochester. While at Western I also spent one year testing neurological patients with focal brain lesions for Doreen Kimura, thinking I might want to do postdoctoral work in Human Neuropsychology. Bryan Kolb, who had been a postdoc with Case Vanderwolf, had left the year before to do a second postdoc with Brenda Milner in Montreal. Bryan papered his office wall at Western with job rejection letters, but after a year with Milner he got lots of offers. This may have also motivated me to give neuropsychology a try. But I didn't like testing people – so instead I went to work with Gary Lynch at the University of California at Irvine in 1977 with the intention of learning some neuroanatomical techniques and doing studies on lesion-induced sprouting and recovery of function. My postdoctoral career was cut short, however, with an offer of an Assistant Professorship in Biopsychology at Michigan, beginning in the fall of 1978.

When I arrived at Michigan the Biopsychology Area consisted of Matt Alpern, Charlie Butter, Roger Davis, Dan

Green, Dave Moody, Jim Papsdorf, Bill Stebbins, Jim Woods, and was chaired by Elliot Valenstein. Warren Holmes arrived at the end of the Fall Term, 1978. Bill Uttal switched his primary affiliation to Biopsychology from the Experimental Area a year or two after that, and Papsdorf left around this time. At that time there was much more space available in Neuroscience than there is now. The bottom floor had not yet been renovated, and we had animal space there, and the third floor was occupied by only 5 labs – Butter, Green, Oakley, Valenstein, and myself. My first few years were spent trying to get a lab established, writing grant applications and putting together courses for the first time. I taught the Introduction to Physiological Psychology course (then 431), and also started an undergraduate lab course in physiological, and a course in human neuropsychology (the latter patterned after the neuropsychology course I'd taken as a graduate student from Kimura).

I also met Jill Becker around this time, and was looking for an excuse to spend a few months in the summer of 1979 in Champaign-Urbana, where she was a graduate student with V. D. Ramirez at the University of Illinois. I thought a collaborative research project would make a good excuse. Jill's thesis research was on sex differences in striatal dopamine systems, and I had an interest in sex differences in brain asymmetries I picked up as a graduate student from Doreen Kimura, and her graduate student at the time, Jeanette McGlone. Jeanette's dissertation research was on sex differences in human cerebral asymmetry. I had been reading Glick's work on asymmetries in the nigrostriatal dopamine system. I had been thinking about getting out of the hippocampal electrophysiology 'business', mainly because Vanderwolf was the established leader in that area, and I didn't want to have 'compete' with him. The dopamine project was started in Illinois seemed promising, and so when Jill moved to Michigan we began a series of collaborative studies on sex differences, hormones, dopamine and lateralization.

In doing these studies we stumbled on the phenomenon of amphetamine-induced sensitization. We were using amphetamine-induced rotation as a behavioral 'assay' to study the effects of hormonal manipulations on dopamine activity, using a within-subjects design. But in our control animals a single pre-exposure to amphetamine caused an enormous increase in the effect of a subsequent drug challenge, given up to a month later. That totally screwed up our experimental design, but the phenomenon struck me as more interesting than the original project. A perusal of the literature soon established that we were not the first to discover this phenomenon of drug-induced sensitization, but there were not many people working on it at the time, and I became increasingly interested in the nature of the long-term neuroadaptations responsible for sensitization. When Jill got her own lab she continued the work on sex differences and hormones, which grew out of her dissertation research, and I became increasingly involved with the sensitization phenomenon, which I am still working on. It has now developed, however, into a very popular area in neuropsychopharmacology. In exploring the nature of sensitization-related changes in the nervous system I also became interested in how they might contribute to the development of addictive behavior. This recently culminated in a major collaborative effort with Kent Berridge, in which we combined his work on the nature of the psychological processes involved in incentive motivation, with the work on sensitization

and dopamine systems, into a new theory of drug addiction, the 'Incentive-Sensitization Theory'. This latter project provides an especially good example of how informal interactions with colleagues can lead to the realization that work on two seemingly quite different topics is actually intimately related, and how both areas are enriched by a collaborative effort.

Martin Sarter

I entered college in 1976 to study social psychology. At that time in Germany, enrollment as a psychology major was controlled by a federal agency which assigned students to a university based on the high school GPA. To my chagrin I was offered enrollment at a community college in a provincial town named Landau. After two years of study, I was thoroughly bored about psychology and thinking about doing something else. Biopsychology was not taught in that place, at least not really.

One day I bought a used textbook, and introduction into physiological psychology, by H. Rohracher. Although the textbook may have been outdated at the time, it was a real epiphany for me and I thought I'd love to do research on brain-behavior relationships. I began searching for a program in what was called back then "physiological psychology", and there were not many. The Psychology Department at the University of Dusseldorf caught my attention because they offered training in psychology as a natural science; in fact I believe that this was the only Psychology program in Germany that was not situated in the humanities or social sciences but was part of the mathematical/natural science college of this university. Luckily they accepted me as a transfer and I moved to Dusseldorf in 1979.

By coincidence, John P. Huston had just been hired to lead the Institute of Physiological Psychology and I made contact with a new Assistant Professor, Dr. Monika Pritzel. Initially I was involved in work involving decerebrate animals and residual learning without a cortex and a striatum (that sounds really old now). Later I worked in Monika's lab on plasticity in the nigro-striatal system, the functional significance of collateral sprouting after unilateral dopaminergic deafferentation and other aspects of models of hemi-parkinsonism. I also took classes from an eminent psychopharmacologist, Prof. W. Janke. His classes were uniquely informative and evoked my interest in psychopharmacology as a scientific approach for research on psychological constructs (as opposed to a discipline that would merely studies drug effects). I also elected to study Pharmacology as a minor. I received my Diploma in Psychology/Pharmacology in 1982. (This degree typically completes the University education in Germany (perhaps comparable to a Masters degree in the US).

Thereafter I moved to the University of Konstanz to study with an up-and-coming behavioral neuroscientist, Hans Markowitsch. Hans is now very well known for his human neuropsychology and imaging work on memory and amnesia. In fact, in Germany he is presently something of a media darling, to be seen on TV and heard on the radio whenever issues involving brain and memory are discussed. Back in the 1980s, Hans focused on comparative neuroanatomy of limbic circuits, prefrontal cortex and learning and memory. Retrograde tracers for anatomical studies and excitotoxic amino acids for lesioning had just become available and thus these were the days of easy experiments and easy productivity. We were a rather small group

of perhaps four graduate students, but sometimes it felt like we produced a weekly publication. I did research on the connections between the prefrontal cortex and the amygdala and the role of these connections in learning and memory. A review I wrote as a student with Hans, on the implications of amygdaloid anatomy on its role in learning and memory, still is among my most cited papers. I received my PhD in 1984.

At the time (and perhaps still today) you needed good connections in Germany to get a job as an assistant professor (though they did not have a tenure-track system as we know it in the US); merit did not seem to play that much of a role. Hans was still a relatively junior person himself and I did not have those connections. I had a pretty good CV at the time I received my PhD but the established "Gods" in academic psychology did not seem to care. Thus, I accepted a research position at Schering AG in West Berlin. At Schering, neuropsychopharmacological research bloomed and life in West Berlin, surrounded by a wall and the communist GDR, was uniquely interesting.

At Schering I worked on negative GABA modulators as cognition enhancers. This was very new and productive research, cumulating in the proposal that some of these drugs might be useful to treat the cognitive impairments associated with age-related cognitive disorders. We also figured out that these drugs somehow worked by modulating the cortical cholinergic input system. The regulation of cholinergic activity and the functions of this system have remained my primary research interests ever since.

After a couple of years at Schering, I wanted to return to an academic research setting and as all the action was in the US, I applied for US tenure-track positions. In 1988, I was offered and accepted such a position at the Ohio State University in Columbus (OH). I think it took me just about two weeks of living in the US to realize that I felt at home in the US.

At OSU, my research focused primarily on the role of the cholinergic system in attention and animal models of cognitive disorders. We developed tasks for the test of attention in laboratory animals and new approaches to measure and manipulate cholinergic neurotransmission in task-performing animals. I began a collaboration with John Bruno that is still ongoing today. We managed to measure acetylcholine release in animals performing tasks requiring attention and impairment of attention following selective removal of the cortical cholinergic input system. We have also investigated the limbic control of the basal forebrain cholinergic system and also studied the role of cholinergic systems in animal models of age-related cognitive disorders.

In 2004 I moved to University of Michigan. In recent years, my research interests have spread out quite a bit. Based on collaborations with my colleagues and our superb students and postdocs, my research now includes problems from human neuropsychology, brain imaging, and molecular techniques. Most recently, much of our research has focused on understanding the neuronal mechanisms responsible for the cognitive symptoms of schizophrenia and the development of new treatment strategies for these symptoms, nicotinic receptor neuropsychopharmacology, choline transporter regulation and function, and human brain systems in attention. (Please check my website for more details).

Barbara Boardman Smuts

My interest in animal behavior began with a childhood enchantment with animals of all kinds. At the age of 13, I read the first of Jane Goodall's National Geographic Magazine reports about her research on wild chimpanzees of the Gombe Stream Reserve, Tanzania. As a result, I decided that I wanted to devote my life to the study of wild primates.

I went to Harvard in 1968 with plans to major in Biology. I was discouraged by a freshman advisor who thought that I, as a woman, probably couldn't handle the math, the labs, and, most of all, the pressure to excel. This sad tale has a happy ending, because I ended up in Anthropology where I received tremendous support from my mentor, Irvan DeVore, whose success at integrating biological and cultural approaches to behavior remains a permanent influence on my work. I majored in social anthropology, which allowed me to read dozens of ethnographies. Their documentation of the day-to-day details of community life inspired my approach to primate fieldwork several years later.

At Harvard, I was fortunate to work with Biology graduate student Robert Trivers, who shared his ideas about sexual selection, parent offspring conflict, and reciprocal altruism while he was thinking them up. These ideas grew into three of the most important papers in modern evolutionary biology, profoundly influenced my thinking about animal behavior.

Encouraged by Trivers, DeVore, and graduate students Peter Rodman and Peter Warshall, I spent the summer of 1970 in Puerto Rico studying free-ranging rhesus monkeys, where I discovered I loved watching animals even more than I thought I would. Upon my return to Harvard, I found a deep rapport with another dedicated field worker, Sarah Blaffer Hardy, who was embarking on her highly influential studies of female lungur reproductive strategies. Sarah remains a source of support and inspiration to this day. I went to graduate school at Stanford University Medical School, where Jane Goodall and my advisor, David Hamburg, M. D., had created opportunities for students to conduct field research on chimpanzees at Gombe. Hamburg's insightful work on the evolution of emotions stimulated my interest in animal emotions, and Goodall's fabulous, slide-filled lectures about her chimpanzee research fueled my eagerness to go to the field. Before leaving for Gombe, I read Richard Wrangham's 1975 thesis chimpanzee behavioral ecology, which influenced me more than anything else I have ever read about primates.

In 1975, 12 years after first reading about Goodall's work, I set foot on the shore of Lake Tanganyika, caught a glimpse of a dark, graceful form knuckle-walking through the forest and I felt I was in heaven. But my euphoria was short-lived. Two months later, a dozen armed guerrillas from Zaire kidnapped three other students and myself. After 7 days in a remote jungle camp, I was released to bring the ransom demands back across the lake. Weeks later, the other three were free unharmed. Gombe, however, was closed to outside researchers (except for Goodall), ending my plans to conduct dissertation research on chimpanzees.

Two years later, I at last found my niche studying wild olive baboons in Gilgil, Kenya. Contrary to theoretical expectations, I found that although baboons fought a lot, they also devoted enormous effort to cultivating intimate relationships

with a handful of other troop members. Abandoning theoretical preconceptions, I asked myself, “What is most important to the baboons themselves?” After many weeks of immersion in their daily lives, the baboons revealed the answer. It determined the direction my research would take and culminated in my first book, SEX AND FRIENDSHIP IN BABOONS.

After completing my Ph. D in Neuro- and Bio-Behavioral Sciences, I taught at Harvard for two years and then spent a year at the Center for Advanced Studies in the Behavioral Sciences at Stanford, where four other primatologists and I edited *Primate Societies*, a volume synthesizing primate field research. In 1984, I joined the Department of Psychology and Anthropology at the University of Michigan, where I have remained happily ever since, with occasional visits to West Australia to conduct fieldwork on social relationships in wild bottlenose dolphins, considered honorary primates because of their large brains and complex social dynamics. In the near future, I plan to resume fieldwork at Gombe, which has once again opened to foreign researchers. This time, however, I will be studying olive baboons, rather than chimpanzees.

This narrative, inevitably, let’s the cat out of the bag, since my history reveals a single absence of formal training in Psychology. I am grateful that, despite the gap in my education, psychologists at the University of Michigan and elsewhere have embraced me as one of their own. Since a fascination with mind, brain and behavior is where I began, finding a home in Psychology is not so strange after all.

Sari van Anders

I grew up in Toronto, and did all my schooling in Canada. I suppose my university story could start on my birthday in 1997, when the President of the University of Western Ontario (UWO) called to offer me a full scholarship. I felt an incredibly pure sense of joy and wellbeing that I actually find hard to describe. The news meant, among other wonderful things, that I could afford to go away to university! This urgent need of mine was a bit silly, given that the University of Toronto and other excellent schools were local, but moving away seemed necessary in that way things do. In high school, I was interested in gender, sex, and evolution, as well as art, so I planned to study biological anthropology and art criticism. I realized early on that I wasn’t overly fond of my art history classes, and - much to my surprise - that I was more interested in my psychology courses (I even enjoyed statistics) than my anthropology courses. I majored in psychology and Scholar’s Electives, and surprised myself again when I found I really clicked with my biopsychology courses.

I decided in, I think, second year that I wanted to go to graduate school, so I focused on gaining research experience. At this stage, I wasn’t sure whether I was more interested in social or biological approaches, though I had a pretty good idea that I would have to pick. I volunteered in an undergraduate research program with Vicki Esses, who worked on issues around immigration. I conducted three independent research courses: one with Tony Vernon on social intelligence, one with Douglas St. Christian (in anthro) on cultural embodiment, and one with Peter Ossenkopp on fear responses, sex, and reproductive status in voles. I then continued with Peter and the voles, supported by an award from the Natural Sciences and Engineering Research Council of Canada (NSERC). To this day, however, the thought

of working with voles still brings an icy fear to my heart, as they were wild-bred and, well, fear-inducing. I realized that my original hunch – wanting to focus on humans – had been a good one. I conducted my honors project with Elizabeth Hampson on gender and evolved responses to facial affect, and graduated with my hons BA, with honors, in 2001. At this point, I still wasn’t sure about social or biological perspectives, even though I was pretty committed to hormones.

Only a few researchers work on hormones in humans with a non-clinical and evolutionary approach, so I decided to stay at UWO with Elizabeth for my MA in behavioral neuroscience (2001-2003), where I was supported by another President’s Scholarship (still exciting though a smaller award) and an NSERC scholarship. Interested in healthy individuals, and thinking about how our knowledge about hormone-behavior associations in humans generally stems from clinical populations or non-human animals, I studied how circulating testosterone was associated with sexual desire and waist-to-hip ratio in women. Serendipitously, Bill Fisher, a social psychologist sex researcher at UWO, was conducting research on testosterone administration and sexual desire in women, and I became involved in that as a side project. While I was waiting to defend my M.A., I conducted a study on whether, in graduate school, men perceive fewer barriers to becoming professors than women do (answer = yes).

I moved to do my PhD in behavioral neuroscience (2003-2007) at Simon Fraser University with Neil Watson, as I wanted to work with someone who focused more on social behaviors, but definitely including hormones. My partner Greg and I moved together, and he worked on his Ph.D. in string theory at nearby UBC. Both he and I were supported by NSERC and some other awards, which helped us focus on research. Being somewhat academically isolated, I made independent connections with colleagues in North America and Europe that were and continue to be very important to me. In my Ph.D., I focused on associations between social behaviors and testosterone in humans, looking at sexuality, partnering, and competition, attending to gender. I recruited enthusiastic students to work in what my supervisor Neil called my ‘mini lab’, and worked very independently to develop a programmatic line of research that I love. My mini lab met in a small windowless room akin in size to a walk-in closet (and we’re not talking mansions) that was my office, lab space, and storage. We took turns sitting on filing cabinets and desks as there were not chairs enough for everyone and the freezer top was too high to sit on.

Along the way, there were many setbacks that – of course – don’t find their way onto vitas, but despite these my work progressed quite well and I began to wonder whether I could skip a post doc. Having worked so independently and having developed a whole research program in social neuroendocrinology (where I was finally able to make the social vs. biology question redundant) made me somewhat loathe to take ‘time out’ to work under/with someone else, despite all the wonderful benefits of post docs. I applied to faculty positions and post doc awards, since I knew a faculty position was a long shot without being in a post doc position or even having my Ph.D. in hand. I was really fortunate to receive the awards and then be able to turn them down, because I was offered a wonderful faculty position to start August 2007! Since I was defending in May 2007, the timing was perfect. I even had time to go to Spain

and Portugal (where my external advisor Rui Oliveira invited me to talk). If you know me, you probably know I love custard tarts: we had a good time .

So, my first faculty position was a joint position in the area of sex and neuroscience at Indiana University in Psychology and the Kinsey Institute. It was a great fit for my own interests, and there were wonderful colleagues in Psychology, Kinsey, Biology, and Gender Studies. It was with dismay that I had to hit the panic button early in my appointment when we were told the unexpected and certainly unwelcome news that the university would never hire in Greg's field of string theory, contrary to some info that was made available prior to our move. I had definitely noticed the University of Michigan ad for the psychology of sexuality joint position in Women's Studies and Psychology, and given the excellent fit - I applied! Since my undergrad, I had wanted to be on faculty in both departments given my interests in critical approaches to gender. I originally planned to keep my feminist interests hush hush until tenure, so of course a position like this was intensely attractive. Additionally attractive was the presence of behavioral neuroendocrinologists in UM Psychology and the program's strengths, the joint program in Women's Studies and Psychology, the remarkable interdisciplinarity of UM Women's Studies, and the excellent physics and string theory program for Greg. So, snippety snap, we moved to Ann Arbor in July 2008. I am enthusiastic about more explicitly incorporating feminist science and inclusive research practices in my research here at UM. And, I am excited to start up my social neuroendocrinology lab here and work on social modulation of testosterone via sexuality and partnering, attending to gender and evolved physiology-behavior links. Who says the social and biological have to sit in opposite corners?! As I'm just starting at UM, writing this has been an interesting opportunity to reflect on what feels like a very long and rich short amount of time. Drop by and say hello!

James H. Woods

Soon after the Soviets launched sputnik, I had the opportunity to begin graduate study in Experimental Psychology at the University of Virginia. Much of my undergraduate career was spent enjoying social activities and playing football. Nevertheless, during that time, I learned about operant conditioning, sensation, and perception – enough to know that I wanted to learn more.

Mr. Jefferson's University had a delightful, albeit peculiar, Department of Psychology. It was staffed exclusively by experimental psychologists. The Chair, Frank Geldard, had convinced the University administration and the state that his Department should be devoted to pure science. My, I had a good time! The faculty, all devotee's of Socrates, taught exclusively in seminars. The students led discussions and taught each other. It was not efficient, nor was it passive, and I put in a lot of 16 hour-a-day exposures to different fields of endeavor. The faculty's interest was in human learning and memory, behavior, sensory mechanisms, and conditioning theory. My interest in conditioning, mainly operant, persisted, but I encountered some problems formulating a thesis project that the faculty approved of. I made a compromise of it, and in the course of solving the dilemma for myself, decided that I wanted to do something, which at the time I considered practical.

I had read of interesting research being done at Michigan on addiction using drug self administration procedures in animals (actually, in Up John and in Pharmacology at the University of Michigan). An undergraduate friend had started graduate studies in psychology at Michigan when I started in Virginia, and so I asked him to inquire about the positions. Bob Schuster had just been hired by the Department of Pharmacology and had a position to offer me. I was so pleased that I left the day after I collected my last bit of information for my thesis.

I became immersed completely in this new endeavor. The only drugs I had ever administered were antibiotics for rodent ear infections. There was (and is) a lot to learn in this field of behavioral pharmacology. Bob Schuster was a psychologist by training, but he knew a lot about drugs; indeed, he and Travis Thompson were writing the first textbook in the field when I arrived. In addition, there was a host of pharmacologists around with much knowledge and expertise. The Department had some of the world's best experts in narcotic pharmacology though I did not know it until after I arrived. It was truly a grand piece of good luck for me to find these opportunities to learn by doing. Quite by coincidence, John Falk had a nearby lab in Pathology that was turning out very interesting findings, and we spent a considerable amount of time together. I didn't know many of the other Psychology faculty except by reputation.

One of the pharmacologists, Ed Domino, had secured a training grant in psychopharmacology, and there were a number of bright graduate students, some of whom found their way to Schuster's lab. I helped with the training of some of them, and when it became time to renew the training grant, we had a site visit, headed up by Fred Elmadjian, an administrator at NIMH. Though I had helped with some laboratory teaching with Harlan Lane in Psychology, I did not have a professional appointment. When Elmadjian heard of this, he said he would have a talk with the "Department" about it. A couple of days after his visit, I got a call from Bill McKeachie regarding an appointment. I continue to give thanks to Elmadjian from time to time.

Soon afterward, Schuster left and my boss in Pharmacology became Maurice Seevers, the Chairman—who was one of the great experts mentioned above. He was nearing retirement and had a grant entitled, The Psychopharmacology of Drug Dependence. He gave me the grant, and told me I was to renew it within a month or two—oh and by the way, if I was to plan and administer the grant, I should be an Assistant Professor in Pharmacology. I stayed up late that night! The grant ran for more than 20 years, and I continue to thank him, too.

During these many years, there have been a lot of psychologists and pharmacologists trained in the lab. Being family-oriented, the most important person in this crew is my wife, Gail Winger, with whom I maintain an active collaboration at home and at work. The postdoctoral fellows have been especially entertaining; I've learned and continue to gain from my collegial relations with them. From the first to the present they are D. Downs, A. Young, K. Takada, W. Kiek, P. Skjoldager, E. Butelman, M. Takasuna, K. Schwarz-Stevens, and D. Jewett. The graduate students have taught me that, while they share some common intellectual interests, they have very distinct attributes. In order of seniority they are, G. Winger, M. Stitzer, H. Salive, C. Iglauer, S. Herling, C. France, A. Bertalmio, W. Essman, E. Walker, S. Comer, and S. Baron.

I have gotten an enormous sense of satisfaction from my association with the “Area” every since becoming affiliated. Training undergraduate students is fun, graduate students are even more fun, and I’m awfully proud of every one of them, though I try my best to express this through the few behavioral principles I have learned.

Being a behavioral pharmacologist involves a difficult act of balance—sitting on an interdisciplinary fence—without falling into either. One relevant device that I’ve found usual over the years is to ask of each experiment, does it involve something of interest about both the drug and the behavior being studied? Not surprisingly, the vast majority of experiments have dealt with opioids (some more recently, with the endogenous variety), their receptors, and the major behaviors they bring about, *e.g.*, analgesia, dependence, discriminative and reinforcing effects, and respiratory depression. Partly as a deviation and partly an effort toward expansion, we have also studied phencyclidine-like drugs and their related excitatory amino acid receptors. Still more recently, the experiments have taken on a more theoretical objective of relating the tenets of receptor theory to behavior as directly as possible.

Those who appreciate the work of the laboratory know well that a few good experiments have been performed, and there are many more to be done...

Current Cognition & Perception Faculty

Bill Gehring

Our research focuses on brain processes related to detecting and correcting performance errors. To investigate these processes we use a measure of scalp-recorded brain electrical activity known as the error-related negativity (ERN), which occurs when people make errors. The ERN is thought to arise from the medial frontal cortex, probably the anterior cingulate or pre-supplementary motor area, and thus much of our research is concerned with testing theories that account for activity in these areas. Prominent theories suggest that the ERN reflects activity involved in detecting errors or response conflict, and that the activity may be involved in overriding erroneous responses. We are also trying to understand how the ERN is disrupted in adults and children with obsessive-compulsive disorder, and how the activity develops through childhood.

John Jonides

Throughout high school and throughout college, I was certain that I would be attending medical school and becoming a practicing physician. Accordingly, I followed a pre-med curriculum at Johns Hopkins, applied to medical school as a senior, and was accepted to several schools. It was in the spring of my senior year that I developed cold feet about the impending experience, largely because I had lost interest during my school years in providing therapy, while at the same time developing interests in basic science. In response, I took a year off, taught junior high school in Baltimore, and worked as a research assistant before and after school hours in the Department of Psychology, largely to earn extra money and to figure out how I was going to spend the rest of my career.

It quickly became apparent that I was very interested in the intellectual problems of cognition, and that I had some talent

in conducting programs of behavioral research on human subjects dealing with various issues in cognition. In particular, the research groups that employed me were conducting fascinating studies on early processes in visual perception including masking and selective attention, and it was an eye opener for me to see just how complex and intricate the visual system and perceptual processes were. Beyond the topics, the mentorship of Bill Bevan and Howard Egeth during that year gave me many opportunities of pursue my interests via several research projects concerned with visual perception. I was also well-tutored by my office-mates also, graduate students Stan Collyer and Howard Hock, both of whom taught me a good deal about how to design and conduct experiments.

With all of this under my belt, I took yet another year off and worked as a full-time research assistant in Psychology. During that time, I began a program of research on perception and attention that occupied me during my graduate school years and beyond. The issue that captured my fancy and that kept my interest for several years was how top-down knowledge of category membership influenced simple visual tasks such as search.

Having found an intellectual interest, I then struggled with a choice of graduate program to complete my Ph.D. I chose to attend the University of Pennsylvania, not so much because there was great expertise there concerning my developing research interests (in fact, there wasn’t), but because the array of faculty there at the time struck me as a fabulous resource for being trained broadly in psychology. I was correct. From Henry Gleitman, I learned how to think as a scientist, how to appreciate what problems are important and what are not, how to teach, and how to write. From Paul Rozin I learned that science should be fun, exciting, and clever; and I learned that educating students in all these things was a critical part of the enterprise. From Justin Aronfreed and Rochel Gelman, I learned about the developing child. And from Lila Gleitman, I learned enough about the rudiments of linguistics and psycholinguists to allow me to appreciate a large body of research that had not influenced me before. Beyond that, I spent significant time with Norm Adler, Randy Gallistel, and Phil Teitelbaum, all of whom reinforced my already keen interests in biological mechanisms. Add to this mix of faculty that my student cohort was unprecedented at Penn and is widely recognized as among the most talented student cohorts of any graduate school period, and it is clear that graduate school was a seminal experience for me. Some of my contemporaries at Penn included Susan Goldin-Meadow, Martha McClintock, Jay McClelland, Heidi Feldman, Jim Johnston, Dan Osherson, Elissa Newport, Peter Schizgal, and Ellen Markman. There were others as well who developed distinguished careers from this cohort. Believe me, there was never a day in graduate school that didn’t have at least one knock-down drag-out battle about some psychological phenomenon or theory. Between the faculty and the students, I learned how to think about data and theory in a way that pushed me to my limit constantly.

After completing my graduate work, I was fortunate to be offered a position at Michigan, among other schools, and I jumped at the chance to be a faculty member as a top-five ranked department. That also was a wise decision: witness that I have spent the remainder of my career at Michigan, with short leaves elsewhere. One of those short leaves was to spend a tour of

duty as an associate dean. The end of that term led to a year's sabbatical leave that was critical for my intellectual development.

I had been observing from afar the early beginnings of cognitive neuroscience, especially the very early work by Raichle, Posner and others using positron emission tomography (PET) to study cognitive processes. This was especially exciting for me because it opened up a vista heretofore untapped: studying the biological basis of cognition in normal adult humans. So, I spent the year of leave studying neuroanatomy, studying the use of imaging techniques, and developing an early research program to study working memory. This all occurred around 1990, and that year of work has become the basis for all of my work since. In the ensuing 10 years, largely in collaboration with my close colleague Ed Smith, I developed a program of research to study the architecture of working memory in humans, guided by earlier behavioral work that I had done, by invasive work on animals done by others, and by work on brain-injured humans published widely in the literature. Since then, many students, other faculty, and I have used both PET and functional MRI to continue to study this architecture, and to expand our studies to include mechanisms of change in working memory as a function of aging, and as a function of pathology. We have most recently concentrated our energy on the study of executive mechanisms of processing that make use of the stored information in working memory. Executive mechanisms such as selectively attending to one source of information rather than others, inhibiting prepotent responses, scheduling sequential processes, and managing multiple task are central to many complex cognitive tasks and so they are critical to understand if one is to have a deep understanding of human cognition. It is for this reason that we have focused on them.

One of the great joys of academic life in science is not only making and pursuing discoveries in the lab, but working with talented students who themselves develop independent programs of research via that work. I have been fortunate over the years to have worked with some very talented students including Dave Baum, Steve Hirtle, John Palmer, Dave Irwin, Steve Yantis, Rich Abrams, Ed Awh, Andrea Patalano, Eric Schumacher, Christy Marshuetz, Ellen Levy, David Badre, and many others too numerous to name. They have been enormous influences in my own work, and judging by the listings in their own resumes, I have been of some influence on their work as well. This sort of interchange is at the very heart of academic life, and I value it enormously.

Beyond interactions with students, of course, I hold the interactions with my faculty colleagues in high regard. For many years, these interactions occurred largely with my colleagues in the Cognition and Perception Area of the Department. But with the development of my interests in biological mechanisms of cognition, I have increasingly spent more and more time working with colleagues in Biopsychology and the Neuroscience Program. This is certainly the major trend of work in cognition, and so it is inevitable that faculty in the C and P and the Biopsychology Areas will develop closer and closer relationships in the coming years.

Cindy Lustig

The work in my lab focuses on the interactions

between attention and memory. Current questions concern the separation of different types of controlled attention, the role of the cholinergic system in attention (in collaboration with Martin Sarter), whether some types of attentional control show greater aging effects than others, and the degree to which age-related attentional problems underlie age-related differences in the "default network" as measured using fMRI. Other studies in the lab take a more translational approach, using the knowledge gained from our basic research to improve cognitive training programs for older adults. We use a variety of methods including behavioral testing, neuroimaging (BOLD and ASL fMRI), cross-species research, drug manipulations, and the examination of group and individual differences (older adults, schizophrenics, and cognitively normal individuals with specific genotypes).

Patricia Reuter-Lorenz

I have to admit that my interest in psychology arose from high school angst. In addition to being introspective and soul-searching, I was very taken by books such as *Sybil*, *The Bell Jar*, and another less well-known book: *I Never Promised You a Rose a Garden*. But it wasn't until the end of my sophomore year of college at SUNY Purchase, that I discovered how deeply that interest in psychology could consume me. Purchase, at that time, was a small liberal arts college where I went to study creative writing. The college was a very funky place for a state school, and most of the students were in the arts. There were no letter grades or required courses, classes were small, and everyone was required to do a junior project, and a senior thesis. Our semesters were divided into a 12-week "long term", and a 4-week "short term." Short terms were intended for periods of intensive, focused study, independently or in a single course that met for several hours a day, four times a week. The one that changed my life was the term I took the psychology of consciousness, taught by Richie Davidson in 1977 (what on earth was he doing at SUNY Purchase?). The course was essentially a study of mind and brain, focusing on altered states, meditation, left brain-right brain, electrophysiology, neurosurgery, and many of the most amazing things that had been done in human brain science to that date. That course changed my life -- radically. It got me hooked on psychology, and I would never look back. I had not gone to college with the intention of going on to graduate school—although neither of my parents had gone to college, they had always assumed that my sisters and I would. But getting a Ph.D.? It hadn't crossed my mind until my course work and senior thesis with Richie Davidson. Never in my life had I wanted anything more than to get graduate training to study the "neural mind."

My undergraduate thesis investigated how the left and right sides of the brain differed in their ability to perceive emotional faces, and it was my first publication. I am proud to say that it is still cited to this day! I have studied a broad spectrum of problems since then: the mechanisms of attention and eye movements, reading, verbal, spatial and emotional working memory, executive functions, neurocognitive aging. Yet the issues I first grappled with in that thesis, laterality, consciousness, and emotion-cognition interactions, frame much of what I do today. But getting my present position in Cognition and Perception at the University of Michigan was a challenging journey with many influential mentors and scholarly experiences along the way.

I did my graduate training at the University of Toronto. When people hear that, they ask if I'm Canadian—my Long Island accent can be hard to place sometimes. Here's why I went to Toronto. The first night of my visit there, the last stop on my tour of graduate programs to decide where to attend, it so happened there was a meeting taking place at Endel Tulving's home (the "episodic/semantic memory" guy). My advisors to be, Morris Moscovitch and Marcel Kinsbourne were both there, along with Gus Craig, Bob Lockhart (the "depth of processing" guys) and a senior graduate student, who did a lot of the talking: Dan Schacter. The meeting was about amnesia, and the group was discussing the dissociations between procedural and declarative memory that were being newly documented by Larry Squires and others. It was 1980, and cognitive psychologists were meeting brain sciences before my eyes in that very room: they pondered what could be learned about how memory worked by studying how it broke down. Imagine how lucky I was to get to eavesdrop on a parlor conversation between the most important memory scientists of the 20th century? This was a student's dream of academia: amazing minds meeting to discuss groundbreaking discoveries and new ideas about how the mind worked. Yet, as any graduate student will tell you... most of graduate school is not like that. And this was surely true for me.

I went to U of T intending to study emotion and the brain, because that was my focus as an undergrad and a topic I deeply adored. But I ended up getting interested in spatial attention because of the great fortune I had, thanks to Marcel Kinsbourne (who moved to Boston upon my arrival in Toronto), to work with stroke patients who had unilateral neglect. So, here I was in the Mecca of memory research studying attention... yikes! I devoured the experimental and brain science work published on attention, especially the work of Mike Posner and John Jonides (now my colleague at UM!). But needless to say there were many hard, frustrating times, where I felt like I was going nowhere; bugs in programs that I couldn't fix, ideas that went round in circles, experiments that came up empty... all the usual heartaches that make one doubt their decision to go to graduate school, and their future. Just when I thought things couldn't get much worse, my thesis advisor told me he'd be spending my last year of grad school in Isreal. Think about it... in those days there was no email, no internet, and on top of that I was stuck with the Canadian mail system that required 2-3 weeks to get a post from the US!! I thought I was doomed. Then something amazing happened. Mike Posner, whom I had written about doing a post doc, wrote me back to say that he could not make a commitment for a post doc because he was moving to St. Louis and was uncertain how long he would stay there. But if I wanted to come visit him there for a while, I was welcome. No brainer! I had my own fellowship money, my thesis data were all collected, Morris was in Isreal. So I packed up and left Toronto, data in hand, off to write my dissertation, and spend some time in the lab of my attention guru, before starting my official post doc... location still to be determined.

This brief time at Washington University was an intellectual renaissance for me. I set up regular meetings with Posner to talk about his work, things I read, and every question I ever conceived about attention. I met Bob Rafal, Mike's collaborator in their groundbreaking work on unilateral neglect, and a close friend of mine to this day. Mike and I got to work on

a project studying neglect patients, and I finished my dissertation! On top of all that, I got introduced to the "family"—those who were to become the founders of "cognitive neuroscience" headed by Michael Gazzaniga, one of Posner's close colleagues.

It was now 1986. Mike Gazzaniga was at Cornell Medical Center, which was on the upper East side of Manhattan. His offer to join his lab as a postdoctoral fellow seemed karmic: I had wanted to work with him at SUNY Stony Brook for graduate school, but because he had left that program I went elsewhere. Now I had the chance to work in his lab, and return to NY, where I'd be close to family and friends. Perfect! In Gazzaniga's program in cognitive neuroscience, I studied patients with pure alexia, commissurotomy, unilateral neglect, Balint's syndrome, and other amazing deficits that gave me the opportunity to read the classic behavioral neurology literature, much of which formed the background and content for the Human Neuropsychology course that I teach at the University of Michigan today. The McDonnell Foundation had decided to infuse funds into the field of Cognitive Neuroscience, and I got to attend the first Summer Institute (a.k.a., Brain Camp) that met at Harvard in 1988, which just celebrated its 20th year!

Then Gazzaniga decided to move the program to Dartmouth, where we'd be closer to his medical colleagues that performed the split-brain surgeries, and to one of our star patients, J.W. I accepted his offer to move there as a research assistant professor of cognitive neuroscience, with an adjunct appointment in psychology. Now, the Psychology Department at Dartmouth is small, but there were several excellent cognitive psychologists there, one of whom, Howard Hughes, would become a close collaborator, and co-investigator on grants and projects over the next four years. Howard also studied visual attention, and had published several papers that I cited extensively in my dissertation, so the fit was perfect. We started several projects investigating mechanisms of attention and eye movements, in patients and healthy adults. I think we both agree that this was one of the most productive and exciting periods of our careers. I was very happy at Dartmouth. So what if I was on "soft money?" Apart from being the associate director of the Summer Institute in Cognitive Neuroscience, I did very little teaching, and I had lots of time for research. Gazzaniga's program was full of wonderful young scientists, Ron Mangun, Liz Phelps, Kathy Baynes, and famous scientists were often coming through to visit Mike and give talks. There were always exciting things on the horizon. Not only that, but I had family in the area, so I could have imagined staying at Dartmouth for many years to come.

But this was not to be. One day in 1991, Mike's secretary gave me a message saying that Ed Smith had called from the University of Michigan. He had gotten my name from Mike Posner, and Mike Gazzaniga—they were looking to hire a cognitive neuroscientist, tenure track... Ann Arbor... and would I be interested in applying for the job....

Here I am. I made it. Sixteen full and exciting years later, with a teenage son, and a wonderful life in Ann Arbor rich with colleagues, students, friends and experiences in science that I wouldn't trade for any other career. Ever.

Dave Meyer

David E. Meyer was born in Louisville, Kentucky, on

February 3, 1943, and grew up in a working-class neighborhood where he roamed the backstreets, playing in many pickup games of baseball, football, and basketball. Avid reading also became an early part of Meyer's life. With a public library card obtained during third grade, he began borrowing and reading books on many topics, including astronomy, geology, and meteorology. His reading fostered an ever-increasing interest in science and mathematics, inspiring him later to take several high school classes in algebra, geometry, trigonometry, and calculus. After high school graduation in 1961, Meyer thought that he should prepare for an engineering career. However, during a brief stint at the Case Institute of Technology, it dawned on him that he really wanted to take a "road less traveled", so he transferred to Wittenberg University - a small liberal-arts institution -- and changed his academic major to psychology. Although he expected personality psychology to be most interesting for him, what soon excited Meyer even more was an experimental psychology class in which students encountered a truly amazing fact: people's behavior can be quantified rigorously, revealing fundamental mathematical laws of human nature. This revelation, together with further study of mathematics, paved the way for the next phase of his education.

In 1964, Meyer entered the University of Michigan's Mathematical Psychology Program, where state-of-the-art training was offered by faculty such as Clyde Coombs, Robyn Dawes, David Krantz, and Keith Smith. Consistent with some of their interests, Meyer's initial research focused on risky decision making, yielding his first publication in the *Journal of Experimental Psychology*. Nevertheless, within two years, his attention wandered toward another burgeoning topic, cognitive psychology, and he began studying it with Robert Bjork, Edwin Martin, Richard Pew, and other faculty at Michigan's Human Performance Center. When exposed to Ulrich Neisser's (1967) book, *Cognitive Psychology*, Meyer was fascinated by prospects of characterizing people as dynamic information-processing systems whose mental operations might be described in computational terms. This eventuated in his doctoral dissertation on the representation and retrieval of semantic information in long-term memory, which he investigated by using sentence-verification tasks and reaction time (RT) measures to test alternative semantic-memory models relevant for language comprehension (Meyer, 1970).

Impressed by Meyer's investigations, Saul Sternberg recruited him to join the Human Information Processing Research Department at the Bell Telephone Laboratories in Murray Hill, New Jersey. Meyer arrived there in June, 1969, and was soon thrilled to watch Neil Armstrong's first walk on the moon, a feat enabled in part by the Labs' technological advances. These were invigorating days! Working beside many stimulating colleagues like Sternberg, Charles Harris, James Johnston, Bela Julesz, John Krauskopf, Thomas Landauer, Ernst Rothkopf, Kirk Smith, and George Sperling made them especially so. At the labs, several other wonderful staff members - among whom were Jack Coriell, Karl Gutschera, Ronald Knoll, and Margaret Ruddy - also helped Meyer be happy and productive.

Thus, his research moved quickly to further territories of semantic memory, where he established an experimental paradigm involving the lexical-decision task, a procedure that requires participants to judge whether various strings of

letters are real words. Here the results revealed that lexical-decision RTs are significantly shorter for words (e.g., "butter") immediately preceded by other associated words (e.g., "bread"). The discovery of such priming opened new windows through which the structure and processing of semantic information can be examined in detail. Concurrently, Roger Schvaneveldt at SUNY Stony Brook also discovered semantic priming in lexical decisions. When he shared notes with Meyer at the 1970 Psychonomic Society meeting, they agreed to co-author an article in the *Journal of Experimental Psychology* (Meyer & Schvaneveldt, 1971), which strongly influenced subsequent studies of visual word recognition and related cognitive processes. Their collaboration flourished over several years, producing more articles on semantic priming, orthographic and phonemic coding, dual-route retrieval models, and other aspects of word recognition (e.g., Meyer, Schvaneveldt, & Ruddy, 1974, 1975). Meanwhile, sparked by several Bell colleagues' research on speech perception and production, Meyer's interests also evolved.

Accompanying this evolution, an invitation from the University of Michigan to rejoin its Psychology Department attracted Meyer back to Ann Arbor in 1977, where he has been a professor ever since. Part of the attraction for this move was the opportunity to have new interactions at Michigan's Human Performance Center with senior faculty like David Krantz, Keith Smith, and Daniel Weintraub, as well as younger faculty like Keith Holyoak, John Jonides, Gary Olson, Robert Pachella, Judith Reitman, and Frank Yates. Equally crucial was the opportunity to have productive new relationships with Michigan's superb graduate students.

For example, in 1979, Meyer began collaborating with one of his first students, Peter C. Gordon. They investigated possible shared mechanisms for perceiving and producing phonetic features in speech. As a result, it appeared that voice onset time may be one such feature for which there is a shared mechanism (e.g., see Meyer & Gordon, 1984, 1985).

Expanding this investigation, Meyer and other graduate students - including Richard Abrams, Kyunghee Koh, Paul Price, and Charles Wright -- studied perceptual-motor interactions and speed-accuracy tradeoffs in manual and ocular movements like those required by the sports that Meyer had played. Along the way, rewarding collaborations were also formed with some of Meyer's faculty colleagues, Sylvan Kornblum, Keith Smith, and Neff Walker. An important focus of their work was the ubiquitous speed-accuracy tradeoff known as Fitts' law. According to it, the mean duration (T) of rapid aimed movements is a logarithmic function of the distance (D) and width (W) of a target region that the movements have to reach ($T = A + B * \log_2(2D/W)$, where A and B are positive constants). Meyer's team hypothesized that Fitts' law stems from people's efforts to cope optimally with random variability in the human motor system. This hypothesis, supported by experimentation and mathematical analyses, generated a powerful new class of stochastic optimized-submovement models that account for the logarithmic tradeoff function as well as other quantitative properties of rapid aimed movements (Meyer, Smith, & Wright, 1982; Meyer, Abrams, Kornblum, Wright, & Smith, 1988; Meyer et al., 1990).

During the 1980s, another complementary line of research by Meyer, faculty colleagues, and graduate students

- including Scott Dickman, David Irwin, John Kounios, Allen Osman, and Steven Yantis -- involved mental chronometry and questions about mental operations that mediate perception, memory, judgment, and action. For example, to what extent do these operations proceed as successive stages of information processing, and how is the output from one operation communicated to another? In seeking answers, Meyer's research developed new techniques for decomposing RT probability distributions, analyzing cognitive speed-accuracy tradeoffs, and characterizing the transmission of information between processing stages (e.g., Meyer, Yantis, Osman, & Smith, 1985; Meyer, Irwin, Osman, & Kounios, 1988; Meyer, Osman, Irwin, & Yantis, 1988). These techniques have been applied subsequently by other investigators, and they have provided a number of insights about "cognitive architecture" in the human information-processing system. Furthermore, accompanying special benefits for Meyer have come from interactions with colleagues elsewhere, including Theodore Bashore, Michael Coles, Emanuel Donchin, and William Gehring, who have extended mental chronometry through measuring event-related brain potentials and thereby helped inspire Meyer's interest in Cognitive Neuroscience.

At the same time, Meyer has continued his research on semantic priming and long-term memory. In experiments with two more students, Natalie Davidson and Ilan Yaniv, he explored how these components of cognition mediate tip-of-the-tongue phenomena, mental incubation, and problem solving. Their results, published in the *Journal of Experimental Psychology: Learning, Memory, and Cognition* (Yaniv & Meyer, 1987; Yaniv, Meyer, & Davidson, 1995), showed that stored unfulfilled goals and spontaneous opportunistic assimilation of retrieval cues may be crucial for surmounting stymied solution attempts.

Since 1990, Meyer's diverse lines of research have converged synergistically in a joint project with David Kieras, a cognitive psychologist and computer scientist at Michigan. Supported by the Office of Naval Research, Kieras and Meyer have created a computational unified theory of human cognition and action. Their theoretical framework, Executive-Process Interactive Control (EPIC), is based on a revolutionary cognitive architecture implemented in computer software that enables precise simulation models to be formulated for executive cognitive processes, working memory, and multitasking. EPIC has provided accurate quantitative accounts of people's skilled performance in both laboratory tasks and realistic contexts such as aircraft operation and human-computer interaction (e.g., Meyer & Kieras, 1997a, 1997b, 1999).

Meyer with some students including - David Fencsik, Jennifer Glass, Leon Gmeindl, Cerita Jones, Adam Krawitz, Erick Lauber, Shane Mueller, Joshua Rubinstein, Eric Schumacher, Travis Seymour, and Eileen Zurbriggen - have been carefully testing EPIC's basic assumptions through experimentation and modeling in their Brain, Cognition, and Action Laboratory (<http://www.umich.edu/~bcalab>). Also, by use of neuroimaging in collaboration with clinical neuropsychologists and neurologists at Michigan, including Jeffrey Evans and Larry Junck, Meyer is seeking to make his work more relevant for understanding the human brain (e.g., Meyer et al., 1998). Indeed, it is mainly based on such collaborations that Meyer's research has progressed over the years, so he gratefully acknowledges the major contributions

that have been made by all of his collaborators.

Daniel Weissman

I am a cognitive neuroscientist seeking to understand the brain mechanisms underlying attention. Using a combination of behavioral methods and non-invasive brain imaging techniques, such as fMRI and EEG, I study the neural circuitry that enables humans to pay attention to stimuli of interest while minimizing distraction from competing stimuli. My research interests also include interactions between attention and working memory, the neural correlates of momentary lapses in attention, interactions between brain systems underlying executive control and social cognition, the effects of interhemispheric communication on attention, and disruptions of attention that are associated with various clinical conditions (e.g., ADHD, Alzheimer's Disease, drug addiction and sleep deprivation).

AUTOBIOGRAPHIES OF SOME EMERITUS FACULTY

Matthew Alpern (deceased 1996; Emeritus)

I grew up in the Great Depression. I graduated high school in 1937 with a mediocre record but having taken more than the usual number of courses. One of those odd courses was Business Law which convinced me that I would never be a lawyer (as an uncle was and which previously had been an ambition).

History interested me more than any other high school course, but the idea that I might be an academic seemed impossibly ambitious. It still does. The spirit of those years offered little to encourage those who perform only in the middle, so (with no confidence in myself) I searched for a feasible career suited to modest ability.

Optometry

I'd worn spectacles since I was about 2 years old so I was familiar with optometry and decided I might manage that. My own optometrist graduated from the Columbia University Program, but the wisest choice might have been Ohio State since I was a citizen of that state. I chose neither. Application to an Ivy League School was never considered. Among my peers, optometry at OSU was notorious for freshman Engineering Drawing, reputed to fail good people. I chose older and larger if the two private optometry colleges in Chicago.

A romantic film led to dreams of a career in optometry research but the degree from my alma mater while suitable for taking the licensing examination to practice optometry in my home state was insufficient for admission to graduate school in its largest university which otherwise might have provided the necessary training. So I went to practice.

Army

In November 1942, I was drafted. I worked as an optometrist in a station hospital. A corporal and a staff sergeant between one and the captain in charge has its advantages and the occasional disadvantage. The following spring there was the chance to study (while still in uniform) one of a very few war-time critically necessary occupations at a university. I spent 15 months studying mechanical engineering at the University of Florida before returning to the troops and ultimately to a general hospital eye clinic.

Mustered out, I tried optometric practice again, first general practice then as a specialist fitting contact lenses, but the plain fact is I was no good at it. A clinician has at his disposal several different tests which normally converge to one answer. This gives me the details of diagnosis and therapy. But sooner or later test results diverge, rather than converge, and one relies on "clinical judgement" honed on experience. When such an event is "sooner" rather than "later" one has no experience and the judgement is based on pure chance. But a decision is required almost immediately! Once made, no matter how, it does harm, not good, losing valuable nights' sleep worrying about the matter. Clinician must behave as if one is certain the recommended therapy will be effective. For a novice this requires a degree of verisimilitude which, on occasion, I was unable to demonstrate.

But this blemish in my character so contradictory to success in clinical practice may in fact have survival value, in research. If one is kept from peaceful sleep by worry about divergent results, that fact may produce an explanation, or if one is fortunate, even two and if luckier still, an experimental way of deciding between them. There is no need to hurry. The longer one worries the probability that one or several of these things may happen increases. Constitutionally, I am a worrier; growing up in the depression, it came with the territory!

Graduate School

When my thoughts turned to a career in serious research I wrote to Professor Fry, who was at the time Director of the School of Optometry at the Ohio State University. Just before the war he had established his Ph.D. program in "Physiological Optics" in order to supply faculty to the 10 or so optometry schools and colleges then in existence. To join one of those faculties was then my ambition. In my letter to Frye I said I was aware that my degree in optometry by itself did not qualify me for this program but wondered if the Florida B.M.E. might mitigate the decision in favor of admission to it. In reply came forms for admission to graduate school which, duly filled out, were forwarded to the Dean of the Graduate School for a decision.

In due course I received the Ph.D. degree in Physics (Physiological Optics). As a graduate student I took a few courses in psychology one of them taught by Arthur Melton who later became a faculty colleague, here in the Psychology Department. Melton was a remarkably stimulating class room teacher and I might have further taken courses from him if he had not returned to active duty in the army (as a bird colonel). I took a lab course in experimental psychology taught by Samuel Renshaw, a visual psychologist who became known in World War II for training navel officers in ship and plane "recognition" by flashing profiles of enemy or friendly aircraft and ships in fraction of a second tachistoscopic exposures. Renshaw also was the representation of psychology on my dissertation committee. Although G.A. Frye, my supervisor, associate professor of Physics, was a bona fide psychologist having obtained his Ph.D. under well-known psychologist William McDougall, he was not a member of the Psychology Department at OSU and a rivalry of sorts existed between the students of the visual psychologists on campus.

After graduation and an additional post-doctoral year I was unable to find "a hard money" (tenure tract) appointment in one of the three research universities with the optometry

programs (i.e. Columbia, University of California Berkeley, or OSU) though all of those campuses were bulging with students supported by the G.I Bill of Rights. There were offers of sorts from several of the remaining seven and I took what seemed to my ignorant eye the most desirable: the one of the seven not a private school dedicated to the instruction of optometry alone.

Pacific University

This was a small church (Congregational) related college in the northwest: Pacific University in Forest Grove, Oregon. Even in those relatively prosperous times for education it seemed in a perennial struggle to avoid bankruptcy. A few years before during the student deprived days of World War II it has extended the breadth of its instruction by amalgamating its curriculum with that of a private optometry college, twenty-five miles to the east of Portland. It enrolled new optometry students twice each year; classes were small but though faculty members were few, the number of classes to be taught was enormous. As the newest kid on the block, I was assigned more than my share.

So it began. Though the assigned time scheduled (> 30 contact hours/week with students) was not favorable for it was expected, I was determined to do some research. Though it has little or nothing to do with the history of Michigan Biopsychology I add here a note of encouragement for those who may become depressed by similar circumstances after obtaining their own Ph. D. and leaving Michigan. It is this. A small poverty stricken college where the majority of your colleagues have long since given up the struggle for discovery, may nevertheless be fertile ground for one who is curious and eager to understand nature better. The college president may not respect you for devoting whatever time you have free of the obligation to teach at novel experimentation, rather than at golfing or fishing, but students will. Some students have ambitions of distinction for their future alma mater so you will find that they will help you in trying to make it a better academic institution. Help by assisting financially for research necessities and by volunteering their special skills to your project. Whether it is carpentry, electronics, photography, or whatever skill to your experiment of the moment may require, there are a few among the undergraduates who will do a much better job of it than you can. In a small college which has lost enthusiasm for research and scholarship, of students with these characteristics know what you are up to, and in such places, word does get around, it is not rare to find them thrusting themselves into the middle of it.

A second source of support is the small minority of faculty colleagues who still take seriously the obligation of an academic person to extend the body of mankind's knowledge and understanding. They will help you and teach you many things you must know. I found both the only physicist and the only psychologist on the faculty enormously helpful. The physicist already had in progress his own research project (modestly supported by "outside" funds) when word filtered down that Tektronix, the local industry would offer several of its latest cathode ray oscilloscopes in a small the local industry would offer several of the latest cathode ray oscilloscopes in a competition among the 6 or 7 small colleges of Western Washington and Oregon for the most meritorious research proposals. I persuaded him to join me in a project studying the human electroretinogram. The application required the signature of the president of our university. When approached, he readily

agreed because, "...if you don't get anywhere in the research we can always sell the oscilloscope and use the money it brings in for other purposes." The psychologist was Anna Berliner whose name I dropped several pages back. She was certainly the first and perhaps the only woman ever admitted as a student of Wilhelm Wundt. She received her Leipzig Ph. D. in 1914. At Pacific, she taught every psychology course in the curriculum: statistics, introduction to psychology, perception, personality and projective testing to enumerate only those I remember with certainty. Her weekly seminar was the major intellectual ferment I knew about on this campus. Its scope was broad, whatever struck her as important in the contemporary literature (books rather than papers) were subject to close, in-depth even exhaustive, analysis (S.S. Stevens Handbook of Experimental Psychology Theory, H.H. Helson, Theoretical Foundations of Psychology and R.B. Onians The Origins of European Thought 2nd Edition 1950 were just some of the books we worked through. We all: students junior and senior faculty alike joined together in the discussions. If today, despite my lack of formal training, I identify myself as a psychologist it is largely due to what I learned sitting at her feet.

After a little more than three years, a substantial body of research had been completed as a result of experiments on my own eyes, and those of students and especially my wife Rebecca (sometimes pregnant). Eventually these results, obtained on home made apparatus with almost no financial support found their way into some eight publications in peer reviewed journals such as American Journal of Optometry, A.M.A. Archives of Ophthalmology, Journal of the Optical Society of America and Science. But in retrospect, if one could predict the future. The entire enterprise might well have profited from careful cost benefit analysis. For in March 1954 I coughed up a glass full of bright arterial blood and was confined to bed. The next month Rebecca returned from the hospital with our newborn second child, our daughter Ann. On that same day I was taken to the Veterans Administration Tuberculosis Hospital in Oregon, some thirty miles to the east, where I was to spend the next 13 months.

In the meantime, I had to think of what to do in the rest of my post (post T.B.) life. Although it provided no income from the hospital year, Pacific offered my old job back; but the prospect of heavy teaching load combined with self imposed research remained as ominous as before. (My determination to preserve in research had not been undermined by thirteen months on my back in the hospital.) Nor has any tenure-track opportunities appeared in any of the possible research universities. What had changed in the 5 years, since I first entered the job market, were openings for soft money jobs available by the new interest Federal agencies in support of basic research. I took the one (of two) offers which seem most suited to my abilities.

Michigan

We drove into Ann Arbor for the first time on the Friday before Labor Day in 1955. My job was Research Associate in the Vision Research Laboratory directed by Associate Professor of Psychology (Harold) Richard Blackwell. Dick was a very smooth guy. The U. S. Army Signal Corp had a 5 megabuck annual contract (I don't think it was a grant, but I would not have then understood the difference) with the University called "Project Michigan" to study communication on the

battlefield, and Blackwell had 10% of that budget for "vision" communication. This supported some 35 or so Post doctoral Psychologist or related people such as myself.

Some Michigan Biopsychology Personalities in the latter half of the 1950's and beyond.

The Michigan expert in Biopsychology at this time was Robert A. McCleary, who graduated from John-Hopkins Medical School. He stayed on in Baltimore to earn a Hopkins Ph.D. (Psychology) before coming here as an Associate Professor for his work on neural mechanisms of autonomic function in the spring of 1957. However, in that same year he was offered and accepted a full professor appointment in the Psychology Department at the University of Chicago, where in the few years remaining in his all too short life he pursued a productive line of research and trained a number of outstanding students.

A Few Outstanding Other Faculty Colleagues and Students.

A remarkable characteristic of the University, which I only fully began to appreciate after I had been here for a decade or so, is the very high intelligence and ability of the majority of its graduate students. I have spent in one way or another over a half of century in various institutions of higher learning in this country and abroad. With the possible exception of Cambridge, rarely did it ever seem that the general intelligence and ability of the students at comparable levels approached the high level I have been accustomed to finding here. Having said that I must also add that I have been especially fortunate that the small sample of graduate students that it was my privilege to know well, i.e. those who worked with me in my laboratory, may easily have biased this admittedly subjective judgement. I owe this remarkable privilege to two very gifted psychology faculty colleagues though neither would be classified as a biopsychologist: namely Clyde Coomb and David Krantz.

* * * *

EDITOR'S NOTE: Matt Alpern never finished writing this biological sketch. He had been working on it for this Biopsychology History periodically during the mid1990s. His wife provided what he had written to the Department after his death. Although officially retired in 1991, Matt continued to go to his laboratory every day. He died on May 16, 1996. The following is part of the obituary that appeared in New York Times:

"Dr. Matthew Alpern, a psychologist at the University of Michigan who took some of the mystery out of color vision and color blindness, died on May 16 at U-M Hospitals in Ann Arbor. Dr Alpern, who lived in Ann Arbor, was 75.

The cause was congestive heart failure, the university said.

A good part of what medical science knows about the mechanisms of human vision and the nature of color vision defects stems from Dr. Alpern's studies at the Vision Research Laboratory at the university's Medical School. After a quarter century in the field he retired as professor emeritus of psychological optics and psychology in 1991, the year that he was elected to the National Academy of Sciences.

Dr. Alpern's contributions covered many aspects of sight, and some of his papers on psychophysics—the relationships between physical stimuli and the resultant sensations—are considered classics. Among other subjects he explored pigment

deficits in people with abnormal three-color vision.

His studies of optics affected areas like contact lenses, prisms, scattered light and antagonism, an irregularity in the curvature of the lens. As for eye movement, he made his mark with work on the divergence and convergence of light rays entering the lens, as well as involuntary movement the eyes make when scanning a line of type.

He was an authority on the physiology of the pupillary light reflex and, with other ophthalmologists, invented a technique known as focal electroretinography. This allows the precise measurement of the electric response of the retinal cones—cells that serve light and color vision and visual activity—for the diagnosis of various eye diseases.”

Charles M. Butter

As an undergraduate majoring in Experimental Psychology at Harvard (in those days experimental and the “soft side” of psychology were in separate departments), I was fortunate to come into contact with some of the big names, including B. F. Skinner, S. S. Stevens, G. von Bekesy and with those who would soon be big names, such as Phil Teitelbaum and Floyd Ratliff. In my junior year I asked the chairman, Eddie Newman, about summer jobs that would give me laboratory experience (and an opportunity to demonstrate my independence from overprotective parents). Newman told me there was a graduate student named Larry Weiskrantz working on his dissertation research at The Institute for Living (a private psychiatric hospital with research facilities) in Hartford, under the direction of Karl Pribram. And so that summer I found myself among these pioneers, who included Mort Mishkin, in what was then modern biopsychology, testing Weiskrantz’s amgdalectomized monkeys and listening to, as well as timidly venturing my own opinions about, the big issues of the day, such as does the cerebral cortex work as a whole or is it parceled into discrete functional systems, and taking part in research that overwhelmingly supported (to the everlasting embarrassment of Lashley’s followers) the latter view. Pribram was a dynamic figure; he also possessed a somewhat bizarre personality and was prone to outbursts, in spite of which he provided the proper “father figure” for an impressionable young student. At the end of the summer, we all attended the annual A.P.A. meeting in New York, where I heard a young, fast-talking, hypo manic fellow called Jim Olds publicly describe for the first time the existence of reinforcement centers in the brain. After spending another summer testing Teitelbaum’s hypothalamic-hyperphagic (and very nasty) rats, I was off to Duke University as a graduate student, where under the direction of Norman Guttman, I studied stimulus generalization in pigeons. Among my fellow students were stimulating individuals such as Gil Gottlieb and Herb Crovitz, who provided lively intellectual discussions of such issues as whether existentialism and scientific psychology were necessarily at odds and whether Dostoevsky followed in the footsteps of Poe (in those days, grad students weren’t afraid to take on the Big Issues). I also had the opportunity to work in the lab of Irv Diamond, then a young biopsychologist from Chicago, testing cats with disconnected sensory tracts. The next step seemed natural – postdoctoral work with Mort Mishkin at NIH – in what was to become the Laboratory of Neuropsychology, where the Pribram tradition continued under the guidance of

Hal Rosvold, the lab chief. Mishkin and Rosvold, as well as Allan Mirsky, provided the kind of direction, counseling and training that could serve as a model for what every postdoc needs (and, sadly, do not always get). There I continued to develop my interest in the visual system, as well as to delve into the mysteries of the frontal lobe, in a very stimulating environment where neuroscience was beginning to emerge as a research field under the guidance of Kety, Sokoloff, Axelrod and Evarts, as well as Mishkin and Rosvold. And just across town, there were Nauta, Brady and Galambos at Walter Reed Army Hospital. In those days of rapidly increasing budgets for science and expanding universities, it wasn’t difficult to find my first (to some people first “real”) job, here in Psychology at The University of Michigan. In those days, leadership in biopsychology training and teaching were provided by Jim Olds (older but still just as hypomanic) who, along with Bill McKeachie, then the chairman, provided the guidance and help in getting my own monkey lab started and took a real interest in my development. Other biopsychologists included Bob Isaacson, Steve Fox at Mental Health Research Institute, Rus and Karen DeValois and Mat Alpern, as well as Bill Stebbins, who came to Michigan the same year I did (1962). In those days, NIH grants, even to start labs, were a piece of cake (some of you may remember the modified whisky ad frequently tacked to faculty doors: “While You’re Up, Gem Me a Grant”). Those of us who came of professional age, so to speak, in the 50’s and 60’s often wonder how well we would do (or what we would do?), starting out in today’s competitive atmosphere; those to whom we pass on the torch will need to grip it more tenaciously than we had to.

Dr. Butter retired in 1999.

David B. Moody

I was one of those kids who always wanted to figure out what makes things work, which, I suppose, is a general characteristic of scientists in general. Unfortunately, from my parent’s perspective, I used to manifest my curiosity by taking mechanical things apart to see what was inside. My skills at disassembly were significantly better than those of reassembly, so I suppose I really learned how things used to work before I got at them.

It wasn’t until high school, however, that I was interested in a career in some scientific field. I was fortunate to have a high school teacher who excelled at getting her students enthusiastic about and involved in research in the laboratory. I spent three years doing various projects in the biology lab, but my real interests were still more directed at things in electrical and mechanical.

My undergraduate career took place in a small liberal arts college in Upstate New York, near Utica. Hamilton College, at that time, had only about 600 students, and provided opportunities in both the arts and the sciences. I saw myself as headed towards engineering, so I decided to major in Physics as the field that best represented my curiosity about how things worked. Introductory Physics was fine... no problem. Then came Atomic and Nuclear Physics... a different story! It was time to consider other alternatives. That same semester, I happened to be enrolled in an Introductory Psychology class which was not at all what I expected Psychology to be. There was a lab as part of the course in which we had a chance to train

rats to press levers, and to learn about things like discrimination and generalization, chains of behavior, secondary reinforcement and the like. The professor, Bill Stebbins, was pretty good also – you can read his story elsewhere in the document. About halfway into the semester Bill announced that he would be leaving to pursue a postdoctoral fellowship in Seattle. His replacement, George Geis, was as enthusiastic and effective a teacher as Bill was, and maintained my interest in the field. Both Bill and George were products of the Psychology Graduate Program at Columbia University, which probably also influenced my eventual choice of graduate school.

There were several unique aspects of the Psychology Department at Hamilton, not the least of which was the number of Psych majors who subsequently went on to graduate school, and are still in the field. Bill Stebbins used to get a lot of kidding because we claimed that he produced more people with PhD's as a result of his teaching at a 600 student liberal arts college without any graduate program than he did here at Michigan. The other unique thing that the Psych Department had to offer was a real grant supported-research lab (Stebbins') that was doing research on reaction time in rats. It was a really impressive lab, especially for somebody who is interested in electrical and mechanical gadgetry. There were relays and flashing lights galore that were used to run the experiments. As I recall, to full racks full of gadgets. When Bill left for Seattle, another students and I was given the task of figuring out how it all worked, and the opportunity to use the equipment for our own research projects.

So my choice of an alternative major to Physics should be simple, right? Wrong! I was going the school on a New York State Regents Science and Engineering Scholarship, and the New York State Regents, in all their wisdom, didn't consider Psychology a science! I had to find another major, which ended up being mathematics. Even so, my time was spent in psychology working in the lab and getting those rats to respond just as fast as their little paws could move. When senior year came, I had to decide what was next. Hamilton had provided a fairly steady stream of graduate students to the graduate program at Columbia, with a few stragglers heading to Harvard and other such places. I applied to both, and got in, but without support at Harvard. The lure of machines and flashing lights and gadgetry also led me to interview with IBM, and the final decision was between a job in industry and graduate school. Obviously, graduate school won out, after some arm twisting by George Geis.

The Columbia program at the time was one of the hotbeds of the experimental analysis of behavior ("Skinnerian psychology"). The leading textbook in the field was Keller and Schoenfeld were on the faculty there, along with the number of other notables. My funding was in the form of a research assistantship with Herb Terrace, a student of Skinner's, who was just in the process of setting up a pigeon lab to study the phenomenon of "Errorless Learning". Whatever skills I acquired tinkering with Stebbins' lab set up at Hamilton came in really handy piecing together the new pigeon lab. The other strong area at Columbia was that of sensory psychology. Particularly vision under Clarence Graham. When the time came to devise a dissertation project, it ended up being a mixture of operant techniques I had picked up at Hamilton and Columbia, the reaction time procedure that had been worked

out by Bill Stebbins, and a question about the functioning of the visual system. I ended up an equal-reaction time procedure to determine the equal brightness function of the pigmented rat. Animals had to be tested 7 days a week in a single testing chamber, so I was rather tied to the lab for a period of time. Eventually, I figured out a way to have a light come on when the testing session was completed, and by placing the end-session light in my office window, I could go across the street, change animals, and then go back home.

During the time I was figuring out my career, Herb Terrace, my Columbia mentor, encountered Bill Stebbins at a meeting, and Bill, who was now at Michigan, mentioned that he was looking for a postdoc who wanted to do research in the auditory system using the techniques of the animal psychophysics. The rest of this "isn't it a small world" story is, as they say, history. I came to University of Michigan in 1967 as a postdoc in the Kresge Hearing Research Institute where I was switched from rat vision to monkey hearing. The reaction-time procedure that had begun at Hamilton still played a big part in the research program, and it eventually led to the technique we still use to measure auditory functioning in monkeys. The postdoc eventually developed into a regular position, due in part to the fact that Bill's colleague, Joe Miller, has accepted a position at the University of Washington, freeing up a position at Kresge. Joe, incidentally, has since returned as the Director of Kresge Hearing Research Institute.

The years, some 26 of them at this point, at Michigan have gone by rather quickly. It has been an exciting time, both from the point of view of students and colleagues we've encountered along the way, as well as the research findings we've been able to unearth. That monkeys hear a lot like humans isn't surprising; they are, after all, man's closest relative in the animal kingdom. In many ways, the differences we've found have been more interesting than the similarities. Why, for example, should two closely related species differ in their ability to learn discrimination between acoustic communication symbols made by one of the species? There is nothing in their pattern of basic auditory function that would predict such a difference, yet one species readily learns the discrimination and the other one does not. The species that makes the sounds and learns the discrimination also shows hemispheric dominance in making the discrimination, just as do humans discriminating speech. What other special abilities are hidden away in the auditory functioning of different species? As another example, why should one species show a particular sensitivity to a drug that causes deafness, while other species show almost no effect of the drug? Or again, why are monkeys so poor at discriminating between sounds of different frequencies, when their abilities to make discriminations based on most other acoustic dimensions rival those of humans?

We've also had a chance to work with the hearing of a number of species other than primates. On my first visit to Kresge, I encountered a raccoon, in one of the animal room cages. One of the students thought it would be an interesting species to train. We later discovered that raccoons are clever creatures, perhaps more so than your average psychologist. We haven't given up on very many creatures, however. Guinea pigs, gerbils, chinchillas, blue jays, quail and pigeons have all told us what they can hear, some more willingly than others. Our only

frustration that I can recall other than the raccoon, was our trio of monitor lizards. Apparently, pure tones are not of much interest to those creatures, since they never learned to respond when the tone was on. From some of the other animals, however, we've learned a fair amount. The chinchillas, for example, have told us about critical bands, and have revealed the interesting fact that low-frequency regions of the ear can incur substantial damage before much change is seen in hearing sensitivity, but that relatively small amounts of damage can reduce sensitivity to differences in frequency. The quail are currently revealing that birds have the ability to regenerate sensory cells in their ears, and to fully recover auditory function following damage.

In all, we've learned a lot and had a good time doing so. There's still more to do....

William "Bill" Stebbins

After listening to the likes of Frank Beach, Clark Hull, Leonard Doob and others, and, with a fairly undistinguished undergraduate record, I left New Haven in 1951 with fantasies of becoming the director of personnel at, say DuPont or some other such industrial giant. I felt certain that personnel Psychology was where the action was, but the Korean War was on, and the government had other ideas about my future. I was drafted and sent to beautiful Fort Dix in New Jersey where in four months I learned to use every lethal weapon known to man at the time, and became an instructor in the use of the mortar and bazooka. Basic training at an end we received orders for overseas when I picked up the flu and was left behind in the base hospital. No one knew quite what to do with me until one day an officer appeared and said that my papers had me listed as a Biological Sciences Research Assistant. It turned out, he said, that a request had just come into headquarters for same, and that I was to pack my bag immediately and embark for Washington D. C. and the Walter Reed Army Medical Center, where I was to spend the remainder of my army career as Captain Joe Brady's research assistant. I can truthfully say that the Army played a major role in shaping my future. Part way through my stay at Walter Reed we were joined by Murray Sidman who had just received his Ph.D. from Columbia, and Joe and Murray persuaded me to apply to Columbia when I left the army in 1953. That was a good year. I got out of the Army in late July, got married in August, and started at Columbia in September. Behaviorism was rampant at Columbia in those days, and I was right in the middle of it and thoroughly enjoying myself. By 1957 it was time to move on and I received my Ph.D. under Fred Keller, and Nat Schoenfeld, and, after teaching for my final year at Barnard College (across the street from Columbia and part of it), I left for the Mohawk Valley where I had taken a job at Hamilton College in little Clinton, New York.

Great students at Hamilton and many went on to graduate school, but, after four years, I felt the lack of graduate students and serious professional colleagues, and I also felt inadequately prepared in the bio regions of Biopsychology. After little reflection I obtained, in 1961, a postdoctoral fellowship in physiology and biophysics at the University of Washington in Seattle. The department was relatively new, and, interestingly enough, the faculty had been recruited en masse from the physiology department at Yale with notables like Ted Ruch, Harry Patton, and Arni Towe. The week after I arrived in Seattle

Sir John Eccles came from Australia and spent a very intense week (lectures every morning and seminars every afternoon) – giant axon of the squid, Nernst equation, and so on. What was a perfectly good behaviorist doing in a place like that? It was great, and, as it turned out, I had something to offer too. There was already a need to look at behavior, and physiologists were really not prepared by training to do so. It was a happy marriage, and it lasted two years until an offer that was not to be denied came from Michigan.

In 1963 the Kresge Hearing Research was founded and Merle Lawrence, its director, who was a biological psychologist trained at Princeton by Glen Weaver, was looking for colleagues to join him. The offer came to me in Seattle and we left for Ann Arbor in the summer of 1963. "We" included my wife, Katie, and three girls, the last of whom was born in Seattle. Further details would unfortunately require more space than is permitted here.

I arrived at Michigan as an Assistant Professor in the Medical School and was promptly asked by Bill McKeachie to participate in the Psychology Department teaching program. Getting a lab started in a new place and getting funding from NIH kept me pretty busy, but I went to departmental meetings and met Jim Olds, Bob Isaacson, Charlie Butter, Phil Best, and Steve Glickman in the department in addition to John Falk in Pathology and Bob Schuster in Pharmacology and his young postdoc, Jim Woods. John, Bob, and I all had joint appointments in the Medical School but participated in departmental functions and had graduate students from Biopsychology. Dave Moody joined me as a postdoc after a few years and then stayed on as a colleague. In the middle sixties, there was a great efflux from the Biopsychology group, and as far as I can tell, all left for different reasons. It was not a combined strategy. We regrouped in the late 60's and Bill McKeachie asked me to chair the area and try to get it going again. Roger Davis and Matt Alpern joined us early in that period. It was a happy and productive group and we set up a formal graduation program to take the place of the vacuum that was left for presumably greener pastures and Jim Woods took Schuster's place as our resident behavioral pharmacologist. I will let the current faculty describe their subsequent arrival on the scene.

There seemed to be plenty of students in those days interested in Biopsychology, and, at one point, David Moody and I encountered ten graduate and four undergraduate students in our lab. From Biopsych students included Swayzer Green, Joan Sinnott, Chuck Brown, Cindy Prosen, Jack Orr, Mike Peterson, Brad May, Mitch Sommers, and Maria Feitosa together with graduate students from elsewhere in Psychology, David Smith and the Psychological Acoustics Program, Bill Clark and Jim Pugh and two very able postdocs, Mike Beecher and Joe Miller (currently director at KHRI).

Fortunately we continued to receive good support from both NINCDS and NSF. Grant money was not a problem in those days. Our work centered mostly non-human primates, and we engaged in a long-term study with Joe Hawkins (joint in Kresge and Physiology) on the function of the hair cells in the inner ear in hearing. We were able to selectively destroy the hair cells by the use of certain antibiotics and intense noise at certain frequencies and relate that to the hearing loss in our animal subjects. Our other research was purely behavioral

and we tried to get a handle on the ways in which non-human primates perceive the complex sounds found in their own species calls. We worked closely with some of the field behaviorists Peter Marler and Steve Green and we were able to demonstrate cerebral laterality, species specific perception of monkey calls in non-human primate, categorical perception and critical features of complex sounds. It was all great fun, and then the administrative bug seized me, and I became involved in Faculty Senate, on the Provost's Budget Priorities Committee, and finally, where I reside today, in the Dean's office of the Graduate School. Also great fun of a different kind, for it gives one an interesting perspective on the entire academic enterprise, but it effectively removed me from active participation in Biopsychology, and that I miss. (Professor Stebbins retired in 1995)

Elliot S. Valenstein

I enlisted in the U. S. Army right after high school and spent about five years in China, Burma, and India during World War II. When I got out of the army, I didn't know what I wanted to do, but I knew that I wanted more education. Although I read quite a bit during my high school years, I was mostly interested in sports and my grades were barely mediocre. At that time, it was difficult getting into City College without a good high school record, but I managed to be accepted on probation. I was so eager for an education that I completed all my undergraduate work in three years with a sufficiently good enough record to be made a Phi Beta Kappa member in my junior year. The striking change between my high school and college grades speaks to the relative contribution of motivation and ability to scholastic achievement. I was not the only one that was "saved" by having their wayward ways interrupted by a war.

After changing my undergraduate major a number of times, almost settling on a history major, I was "turned on" to psychology by some excellent teachers. At CCNY, I worked as an assistant to Gardner Murphy, doing library research for his books on the history of psychology and on personality. While I was thrilled to be working as the "great man's" assistant, I was most involved in the several "undergraduate" research projects that I started in Herbert Birch's comparative psychology course. One was a Kuo-like study of the development of kittens' response to movement. My partner and I spent an enormous amount of time raising (including feeding) newborn kittens in the dark. Through my association with Birch and through friendships with Danny Lehrman and Jay Rosenblatt (who were Birch's teaching assistants), I came to know the comparative psychology group (T. C. Schneirla, Lester Aronson, Ethel Tobach and others) working at the American Museum of Natural History. I didn't know Schneirla was a Michigan Ph.D. and, of course, I had no idea at the time that I would end up at UM, spending more of my life there than at any other place.

As a result of having contracted malaria and a few other "jungle fevers," I had a little more financial support under the GI Bill than the average veteran and could consider getting married to Thelma, who I met shortly after being discharged. I believe I was still a Freshmen when we married, but I was about five years older than today's typical freshman. There were, however, many other students my age with a war just behind them and that made for a unique, exciting educational experience. I can remember beginning a philosophy course with the following

assignment: "You are in a concentration camp where a member of an underground group has killed a Nazi guard. The German commandant is convinced that you know who killed the guard and has announced that 50 prisoners will be shot the next morning unless you give him the name. You do know who did the killing. What would you do and what ethical principles would you follow?"

Although I knew virtually nothing about graduate schools I applied to the University of Kansas primarily because Martin Scheerer, who had impressed everyone as a teacher at CCNY had been given an appointment at Kansas. I applied for the VA training program in clinical psychology mainly because it paid a good stipend, but before my first year ended, I decided to work instead in William C. Young's laboratory doing experiments on hormones and behavior. Young was a Professor of Anatomy and as a result, I ended up satisfying both the requirements of the Psychology and Anatomy Departments.

It was during this period while working in Young's laboratory that my colleagues (Phoenix, Goy, and Young) demonstrated for the first time that exposure to androgens during early development could masculinize the behavior (as well as anatomy) of female guinea pigs. Incidentally, because I was identified by Young as a psychologist and he liked me, I ended up being responsible for bringing Bob Goy and Charlie Phoenix to the lab and, thereby, completely changing their careers. The guinea pig studies were the catalyst for much of the later research demonstrating that steroid hormones can influence brain development during early life. My 1954 thesis was entitled: "Experiential, hormonal, and genetic factors in the organization of sexual behavior in the male guinea pig" and dealt with the way steroid hormones, the experience of animals, and their genetically determined predispositions interacted in the control of sexual behavior. This work reinforced my longstanding bias—partly politically based—that the influence of biological factors cannot be understood without considering the history of the organism and other mediating environmental factors.

My mentor, Bill Young was a perfectionist when it came to writing papers and he would invite his graduate students during the summer months to spend a couple of weeks with him at a family house near Cape Cod where manuscripts would be thrashed out – word by word – until he judged them ready for submission to an editor. Although, I liked to write and took some pride in the little talent I had in that direction, experience collaborating with Bill Young taught me that any manuscript could be improved by more work.

I continued working on hormones and behavior as a USPH Postdoctoral Fellow and remained in Young's laboratory for an additional year. We started to concentrate on the so-called "organizational" effects of exposure to hormones during early development in contrast to the "activation" role of these hormones in the adult.

In 1955, I started working at the Walter Reed Army Institute of Research in Washington, D. C. Our first son, Paul was born just before we left Kansas with our few possessions (mostly books, bricks, and book shelves) in a homemade trailer. Our second son, Carl was born in Washington, two years later.

I had some hesitation in accepting a position at an army research center, but there were few positions available at the time. It turned out I was lucky because Walter Reed was about

the best place I could have worked at that period. Although I had taken a graduate course in neuroanatomy, I learned infinitely more about the subject through sharing an office for seven years with Walle Nauta and collaborating with him on comparative neuroanatomical research – using silver stains to trace the hippocampal fornix system in rats, guinea pigs, cats, and monkeys.

Although I learned quite a bit and got some respectable work done, I decided after a few years, that I did not have the temperament to sit all day at a microscope and besides my spatial ability was not good enough for that kind of work. I started to do research on rewarding brain systems, and a little work on “minor tranquilizers” and behavior.

Research on brain stimulation and rewarding neural circuits took several directions. With Bill Hodos, I did some studies on how motivational states such as hunger, thirst, and estrus influenced the strength of rewarding brain stimulation at different neural sites. Also with Bill Hodos, I determined some of the limitations of bar-pressing rate as a measure of the strength of rewarding brain stimulation. Later at the Fels Research Institute, I developed some rate-independent measures of reward strength. I was also interested at the time in whether continuous rewarding brain stimulation produced a satiation as is true of all “natural” rewards. With Bernie Beer, I followed rats for 21 days, which had free access to a lever that could deliver rewarding brain stimulation. We had to work around the clock to assure that the animals did not get twisted up in the cables attached to their electrodes. Very little satiation developed and I believe our rats which pressed the lever over 840,000 times in 20 days (averaging about 30 responses per minute) hold the “world record” for this event. I was involved in other research on rewarding brain stimulation and also developed a number of different techniques useful in doing research involving electrodes attached to freely moving animals. We developed, for example, an inexpensive brain stimulator, one of the first practical swivel devices that prevented animals from getting tied up in their cables, and with Larry Stein and Bernie Beer, I developed the Plastic Products electrode that has probably been inserted into thousands and thousands of rats.

This was a “golden era” at Walter Reed and my colleagues included (besides Nauta) Dave Hubel, Joe Brady, Murray Sidman, Bob Galambos, Harvey Karten, John Mason, Dick Herrnstein, Larry Stein, and many other talented and highly productive people. Because of the Korean War there was a military draft and those with the right connections and ability were able to arrange to do research at Walter Reed in lieu of soldiering. Bill Hodos, for example, did his Ph.D. thesis (University of Pennsylvania) on rewarding brain stimulation in my lab while in uniform.

In 1961, I was on leave from Walter Reed and spent six months in laboratories in the Soviet Union, on a National Academy of Science Fellowship. On the way to the Soviet Union, I spent several weeks in Warsaw with Jerzy Kornorski and others at the Nencki Institute and also some time with Jan Burês and others at the Institute of Physiology in Prague.

Later in 1961, I accepted a position as a Senior Scientist at the Fels Research Institute in Yellow Springs, Ohio. Yellow Springs is a village of 5,000 people and a great place to raise young children because everything was so close. We lived

about 200 yards from my laboratory. Thelma also worked in my laboratory and there are several Valenstein & Valenstein papers (including an infant rat stereotaxic atlas) resulting from that collaboration. We were able to work quite long hours, but we had plenty of “family time” because our sons, Paul and Carl, could visit us in the lab and we could all have lunch together. I was also a Professor of Psychology at Antioch College and taught several courses (without pay as I recall) and trained a steady stream of undergraduates in my laboratory at Fels. There were several postdoctoral fellows (one of them was Vern Cox, who later became Chairman of Psychology at the University of Texas, Arlington) and some senior graduate students (“Tony” Phillips was one of the latter) working with me and the extra hands and brain cells helped to complete a number of research projects on various topics.

Continuing work on rewarding brain stimulation started at Walter Reed, we demonstrated that animals would terminate stimulation at all rewarding brain sites and investigated whether they do this because the prolonged stimulation becomes aversive or no longer rewarding. We reported for the first time in animals that rewarding brain stimulation could mask the effects of otherwise painful stimuli delivered either peripherally or centrally. I regret that we did not pursue this phenomenon as it led eventually to the discovery of opioid systems that mask pain, but our work was too early as no one knew at the time that there were such systems in the brain. We also investigated the neural mechanisms underlying appetite and body weight regulation and our discovery of sex differences in the rate of body weight increases following ventromedial hypothalamic lesions forced us to speculate about metabolic factors. Although our speculations were not always correct, we did raise some overdue questioning of whether the ventromedial nucleus was solely a “satiety center.”

During the 1960’s, there were a succession of papers demonstrating that stimulation of discrete centers primarily in the lateral hypothalamus could evoke eating, drinking, sexual behavior, hoarding, maternal behavior, etc. it was generally accepted that hypothalamic stimulation evoked the specific motivational states underlying hunger, thirst, sexual and maternal behavior, hoarding, and so forth. There seemed to be a furious competition, at the time, to be the first to get an article into Science describing yet another motivational center in the hypothalamus.

Some of our observations made us skeptical about the way the “stimulus-bound behavior” data was being used to “Balkanize” the hypothalamus into discrete motivational areas. We began what turned out to be a large number of studies designed to get a more complete picture of the evoked behavior under different conditions. We explored different ways of changing the testing conditions to learn more about the relationship between stimulating a specific hypothalamic site and a discrete behavior and to determine more about the nature of the motivational state evoked by stimulation. The results, taken together, led to serious questions about whether the stimulated animals were really hungry, thirsty, sexy, and so forth. Stimulated animals would switch from drinking to eating, to other behaviors if given the opportunity. Animals thought to have been made hungry by the stimulation would not switch to other foods and animals thought to have been mad thirsty would continue to lap at empty drinking bottles during the stimulation. Moreover, our

histological analysis revealed that the sites capable of evoking different behaviors were much more widespread and much less discrete than had been claimed.

The response to our publications was bimodal. Many felt that the type of behavioral analysis we did was long overdue and were pleased to have “electrode phrenology” criticized. Others criticized our findings on various empirical grounds and claimed (unfairly I believe) that we were arguing for a “neo-Lashlain equipotentiality” in which there was no localization within large areas of the brain. All of this controversy and interest was intellectually exhilarating and emotionally challenging. The controversy was partly responsible for one of our articles being cited so frequently that it became a “Citation Classic.”

In later studies at the University of Michigan, my students and I demonstrated that the predisposition of individual animals was often as important as anatomical location of the electrode in determining what behavior would be evoked by hypothalamic stimulation. When it was shown that animals aroused by “tail-pinch” would display many of the same behaviors, the argument for discrete anatomical specificity of motivational systems was further weakened. Much of the more recent research on dopamine pathways supports the idea that there are some neural circuits common to diverse motivational systems that course through the hypothalamus. I have, however, as many reservations about “neurotransmitter phrenology” as I did about “electrode phrenology.”

During the 1969-1970 academic year, I was on leave as a Visiting Professor at the University of California at Berkeley where I did some teaching and research with Irv Zucker and his students. During this leave period, I accepted an offer from the University of Michigan and came to Ann Arbor in the fall of 1970 as a Full Professor of Psychology with a joint appointment in the newly organized Neuroscience Program. I had been offered a position at Michigan several years before, but had turned it down as I didn't want to disrupt my ongoing research. By 1970, however, the research was turning in a new direction and a move would not have been as disruptive.

Meanwhile, at the University, I continued research along the same line I had been doing at the Fels Research Institute as well as teaching. Whereas formerly I was in the lab most of the time, at this point in my career the research was being done primarily by a succession of excellent (at least most of them) graduate students and some research was in collaboration with faculty colleagues. I also served on and chaired a great many grant review panels (Study Sections for NIH, NIMH, and NSF), the Scientific Advisory Board for the Wisconsin Primate Center, and the New York State Committee for evaluating Psychology Doctoral Programs. I also served as Trustee of the James McKean Cattell fund, which provides financial support for academics on sabbatical leave.

From 1977-1990, I was the Chairman of the Biopsychology Area and helped in the recruitment of many of the present Area staff. I spent a sabbatical leave during the 1976-77 academic year writing a book at the Advanced Institute for the Behavioral Sciences in Stanford and seven years later, I spent a sabbatical leave at the National Humanities Center in the Research Triangle in North Carolina working on another book. On two occasions I spent shorter periods doing research in Israel

(once at Hebrew University and once at Tel Aviv University) on Fulbright Fellowships.

I have been extremely fortunate in having been invited to many different countries: an invitation to participate in a “scientific and cultural exchange program” enabled me to visit laboratories in the (then) Soviet Union during a six month visit and the opportunity to adopt as an ancestor a physiologist named Spiro (my middle name and my mother's maiden name), who was a collaborator of Ivan Sechenov. Among other more memorable invitations was an invitation to give a “tutorial lecture” to neurosurgeons in Madrid and a chance to travel widely in Spain; teaching in Shiraz (Iran) in an IBRO sponsored program gave me a chance to see Persepolis; after a lecture at Cambridge University I tried the snuff offered to me following dinner at the “high table”; lecturing at Peking University gave me the chance to return to China after a 40 year's absence; a lecture at an international meeting in Japan provided a chance to sail on the “inland sea” and to travel quite a bit around that country; a couple of invitations to give lectures at a neurology institute in Mexico City provided opportunities for my family to explore Aztec and Mayan ruins; being a Resident Scholar in the Rockefeller Foundation's Villa Serbelloni enabled me to do some traveling in Italy; collecting some information about the neurologist Egas Moniz involved traveling around a good part of Portugal; during the summer spent as a Visiting Scientist at the Oregon Primate Center there was time to do great salmon fishing; before giving the Owen Holmes lecture at Lethbridge University (in Alberta) Thelma and I explored the Canadian Rockies; and various invitations to lecture at international conferences in such diverse places as The Netherlands, Belgium, Hungary and Australia provided additional opportunities for travel abroad; while the year I was a Phi Beta Kappa Visiting Scholar I visited some of the best liberal arts colleges around the U. S. There were other trips over the years and scientists from China, Israel, England and other countries, who worked in my laboratory for various lengths of time. Unquestionable, one of the more pleasant aspects of a career in science and academia is that it makes it possible to travel widely and to have friends and colleagues around the world.

By the mid-1980's, I started to reduce my laboratory research and gradually became more involved in writing books. A few of my books, Brain Control, Great and Desperate Cures, and Blaming the Brain received a lot of attention and this led to more invitations to give talks and to travel to some fascinating places that I otherwise would probably never had visited.

As a result of writing about psychosurgery, I was asked to participate in many symposia on ethical issues in research and medical practice. I became quite visible on these issues because of newspaper interviews, being on William Buckley's TV program Firing Line, and the like and the report I wrote on the current practice of psychosurgery for The National Commission on the Protection of Human Subjects in Biomedical and Behavioral Research has been quoted extensively as has a subsequent book, The Psychosurgery Debate, which dealt with the scientific, legal, and ethical issues raised by this practice. In retrospect, I have mixed feelings about having let myself be drawn out of the laboratory. I was, however, stimulated by having to think about ethical issues and having my ideas challenged in a completely new arena. I realize that it is

impossible to do everything and what I did do was worthwhile.

In any case, having diminished my research output, it was clearly going to be difficult to get the funding necessary to support a laboratory. Besides, I was by that time well into the sixth decade of my life, and beginning to face the reality that I had become reluctant to undertake long-term research projects or to learn new techniques if they would not pay off for several years. Not the right frame of mind for training students.

I had been gradually giving up my laboratory space to more active colleagues and decided it was time to completely close my laboratory and make it possible for the Biopsychology Area to recruit someone who would be actively involved in research and in training students. I felt that I did not have the patience, the temperament, or the talent to be a successful university administrator. Besides, I wanted to spend more time writing. I arranged to retire in the fall of 1994, but I planned to continue my affiliation with the Area and University in various capacities.

AUTOBIOGRAPHIES OF SOME FACULTY WHO RETIRED OR LEFT THE UNIVERSITY FOR OTHER POSITIONS

Seema Bhatnagar

I entered the Psychology Department at McGill University in Montreal, where I grew up, with an interest in combining physiology with behavior and disease. The Psychology department at McGill was very physiologically oriented and, while this displeased a great number of Psychology majors, I was very happy to take every physiological psychology course that was offered. Even as undergraduates, we had some awareness that we were studying in a distinguished department with faculty such as Ron Melzack, Peter Milner and Brenda Milner and the shadows of Hebb. In particular, the Physiological Psychology course taught by Peter Milner was inspiring, and by my second year I knew I wanted to go to graduate school and conduct research on some aspect of physiological psychology. My summer and undergraduate research projects examined the visual systems of cats and gerbils and amphetamine-induced stereotypy in rats. However, it was not until I worked as a research assistant in the lab of Michael Meaney, at Concordia University (also in Montreal), that I found a specific field of research that was completely fascinating to me. This research was on the long-lasting effects of maternal separation on behavioral and neuroendocrine responsivity to stress exposure in adulthood. I eventually did my Ph.D. in Neurological Sciences with Michael who had by that time moved to McGill University. The atmosphere in the lab and at the Douglas Hospital Research Center (where the lab was located) was an invigorating one for graduate students. The Research Center was an energetic, supportive, yet sometimes fractious, place full of colorful characters. It was unheard of for a student in the lab to be working on only their own research project without helping other students or working on larger lab projects. My own research examined how maternal separation altered neuroendocrine responses to chronic stress. I was interested in following up on this research by examining the neural substrates of chronic stress effects on neuroendocrine function and began post-doctoral work with Mary Dallman at the University of California at San Francisco. Having grown up in eastern Canada, California was

that strange place out west and I had no particular desire to live there and moved there only to work with Mary. As could be predicted, I came to love living in San Francisco fairly quickly. In Mary's lab I was lucky enough to find the same sense of collegiality and collaboration as in Michael's lab although UCSF was a much more competitive environment than any I had previously encountered. Both Michael and Mary are generous individuals who gave me great freedom in developing my research interests. Not all my training has been with generous and positive mentors, but I will leave that discussion for the next edition. Overall, the research experiences that have been most rewarding to me were those that involved cooperativity and a large measure of risk-taking. I feel fortunate to have had these experiences as I begin my own lab as a new faculty member at the University of Michigan.

Roger Davis

I thought about being a biologist before I could have had any idea of what that might be. I loved keeping animals and hanging out around the river. My father began wondering about me at age 7 when he found me with a razor blade examining the entrails of bait minnows he brought home from his fishing trips. Twenty years later, though I was still cutting up fish and had no 'real' job, I think that he saw that it was ok. I was happy and the roller blades were inexpensive.

I quit high school to enter the military service before September 1946, thereby gaining the opportunity to go to college on the original GI Bill. The first year in the USAF was pretty depressing. There was a lot of KP and guard duty while we waited for assignments to training programs. My break came when I was shipped to Oklahoma air force base for training as a weather observer. I enjoyed learning about weather but never envisioned a career in meteorology.

The next two years I spent in relative luxury, watching weather pass a tiny airbase by Selma, AL, where officers came to study military history. There was little for enlisted men to do in Selma. I liked plotting the weather maps from teletype data and lofting unorthodox assemblies of hydrogen balloons to measure winds at record altitudes. The airbase had a good library which I had pretty much to myself, so I had a peaceful, educational time in the military. I had just turned 20 when discharged and given 48 months of full support to attend college.

I began undergraduate training at the University of Wisconsin at Milwaukee in January of 1950. I majored in biology. My zoology instructor urged me to go to the University of Michigan, where she had studied. I moved to Ann Arbor in the fall of 1951, and stayed until August of 1954. I received an education in classical zoology, including comparative vertebrate anatomy, physiology, and embryology, and fish biology, invertebrate biology, with some botany and geology, in addition to basic courses in physics, chemistry and foreign languages. I mainly associated with the Department of Fisheries of the School of Natural Resources, and the Fish Division of the Museum of Zoology. I labeled many boxes and jars of preserved fish in the Museum, and briefly considered a career in ichthyology, but I wanted to study behavioral phenomena.

Fisheries was a very small Department. Professor Karl Lagler and John Bardach were the principal staff members. John

supervised my Master's research project, which dealt with body temperature, or heat transfer, in fish. John had recently received a Ph. D. in Zoology from the University of Wisconsin. His stories about the Department and Lake Mendota convinced me to go to Madison for further graduate work.

I graduated from the University of Michigan in July of 1954, using the last 48 months of support and having a B.S. degree in Zoology and a MS. Degree in Fish Biology.

Leaving Ann Arbor, I biked to Madison, Wisconsin, where I was admitted as a graduate student and hired as a teaching assistant in the Department of Zoology. I started the Ph.D. program in September of 1954, and graduated in the summer of 1960. I had a wonderful time at the UW. I majored in zoology, minored in geology, taught labs in general zoology, comparative embryology. My Ph.D. dissertation was on daily activity rhythms of fish. I found that fish can learn a daily, or semi-daily feeding time. The internal clock synchronizes on sunrise, i.e., the daily change from dark to light is the "time giver", using the jargon of the day. This finding was the beginning of my interests in learning and memory.

The Zoology department was still premodern, or quasi-Victorian, while I was there, which suited me completely. It was a simpler time, relatively idealistic and, in some ways, innocent. We foraged for most things that we used in research and research supervision by faculty was typically light. You made it on your own or you did not.

I spent two summers in Woods Hole, MA. In 1955, I had a departmental fellowship to study marine ecology at the Marine Biological Laboratory. The following summer I returned to work as a research assistant to Prof. Charles Jenner, University of North Carolina, at Chapel Hill. Those summers were full of many memorable experiences, biking from Madison, learning about the ocean and marine life. I formed a strong attachment to the Laboratory, the Cape and living near the sea, and I was eager to return there when I left Madison, to continue work on daily activity rhythms in fish.

I returned to Woods Hole in September of 1960, with a post doctoral NSF grant on daily rhythms in fish. The grant was administered through the University of Michigan Department of Fisheries. My lab was in the US Department of Interior Fisheries Laboratory. My wife, Nancy, Barden, and I lived in Woods Hole, on Great Harbor, next to the USFWS Public Aquarium. What a deal! It was difficult leaving there, in 1963, when the grant ended and we returned to Ann Arbor.

That fall I instructed in comparative physiology in the Zoology Department. In January of 1964 I took position as a postdoctoral fellow in the Mental Health Research Institute, working on biochemical correlates of memory storage in Bernard Agranoff's laboratory. I had begun studying methods of measuring RNA and DNA while in Woods Hole, after reading Holger Hyden's work on changes in nucleotide ratios correlated with learning in rats. Agranoff was using antibiotic antimetabolites, such as puromycin, to investigate mechanisms of memory consolidation fish. It was a great opportunity for a fish biologist interested in brain mechanisms of learning and memory. My immediate associates were Paul Klinger, Mary Hirtzel, Peter Homes, and Al Vinegar. Paul still works for MHRI. Mari (now Mari Golub) is a Biopsychologist, studying neurotoxicology in developing primates, at Davis, CA. Peter teaches at EMU and Al

is at school in Ohio.

I had a NIMH Career award from 1968 – 1973 by the end of which I became interested in brain mechanisms of social behavior and reproductive behavior in the paradise fish. Graduate students who worked with me on those topics included Mary Lou Cheal, now an experimental psychologist working for the Air force in AZ; Jeffery Kassel, now a lawyer in Madison, WI; Patricia Schwagmeyer, a biopsychologist until her recent, untimely death.

In 1980, I received a grant from the EPA for developing a behavioral assay for measuring sub lethal neurotoxicity. The assay was based on the regenerating of goldfish optic nerve. We used classical visual conditioning to measure the time recovery of visual function following optic nerve lesions, as a measure of neurotoxicity.

Cholchicine and related alkaloids were used as experimental toxins. We also used the preparation to investigate behavioral correlates of neuronal competition in regenerating optic nerve preparation. Collaborators who made these projects possible were Susan Benloucif, who went on to receive a Ph.D. in biopsychology. At UC Berkley; Paul Klinger, and in particular, Barbara Schlumpf. Barbara earned a Ph.D. at Michigan and worked for several years on behavioral assays of visual function in human infants. Currently she is a full time mother and homemaker.

In the 1980's, discoveries related to the NMDA receptors, long-term potentiation and associative learning renewed my interests in brain mechanism of learning and memory. Paul Klinger and I adapted the Pavlovian fear conditioning preparation in goldfish for studies of the effects of NMDA receptor antagonist on learning. We were joined by Xiajuan Xu whose Ph. D dissertation established that the NDMA antagonist MK-801 blocks learning in fish agreeing with finding in mammals.

We have since found another inhibitor of NMDA receptor function, HA 966, which antagonizes the glycine 'facilitator' site of the NMDA receptor complex, also blocks learning in goldfish. This confirms that NMDA receptor functions play a critical role in fear learning in fish. Of greater interest to me has been the finding that MK-801 blocks learning related to the contiguity of the CS and US but not discrimination of the CS. When fish are pre-trained by conditioning of CS1, learning of CS2 is not disrupted by MK-801. Pseudoconditioning with CS1 does not protect learning of CS2 from MK-801.

As far as I know, this is the first evidence, behavioral or otherwise, that *different components of learning* within a paradigm, or task, are mediated by different neurotransmitter systems. Learning in different tasks has been shown to vary in sensitivity to blockade by NMDA receptor antagonists; for example, in rats, AP5 blocks spatial learning but not visual discrimination learning.

My retirement furlough begins in June 1993. I want to complete a few additional experiments during the coming year, to find out more about the brain mechanisms of contiguity and CS discrimination learning. Also, I am moving to Aspen, CO to begin renovating our house overlooking Ajax, the killer ski area. Leaving biopsychology, I will paint wilderness landscapes, explore trout streams and mountain meadows, and polish my

carpentry and skiing skills.

Warren Holmes

I grew up in Northwest, near Portland, OR, which is only a short distance from Cascades, a mountain range that figured prominently in the early development and subsequent career. From the time I was in grade school, I thought I wanted to be a physician and so as an undergraduate at Willamette University in Salem, OR, I majored in chemistry. A junior year of Avignon, France, opened my eyes to a number of delights outside the realm of the hard sciences and forces me to examine my interests in medicine, which proved to be less compelling than I had thought. The realization that medicine was not what I wanted to pursue came late in my undergraduate career and so, after receiving my BS in Chemistry, I did what any sensible person would do who had been going to school since the age of five years, I registered to take more classes, this time as a non-degree candidate at the University of Washington.

During my first year in Seattle (1970), I took a psychology course from Dr. Davida Teller, who opened my eyes to empirical research and the scientific method. Having come from a small liberal arts school where research was rarely done, I had no idea how exciting and fascinating empirical work could be. I was attracted immediately to testing ideas and explanations by examining them logically in a hypothesis-testing format. Although I discovered that I was not interested in studying the visual system, which was Davida's passion, I did learn that I wanted to pursue a Ph. D and was admitted in 1971 to the Psychology Department at the University of Washington.

During my first two years in graduate school, I took courses on the natural science side of psychology, but could not identify a specific area in which I wanted to work. During these two years, I also took courses in the Department of Educational Psychology and worked with Dr. David Island. David was the most influential educator I have ever encountered and the philosophy of education, which guides the teaching I do today, was developed during the two years I worked closely with him.

Although I learned much during my first two years in graduate school, it was not always in the classroom nor was it apparent to the graduate committee that what I was learning could be translated into "satisfactory progress towards a degree." The graduate committee suggested that I complete a research project at the master's level by September 1974, or "consider options outside the university." I took the committee's message to heart, and working under the supervision of Drs. John Lockard and David Island, I completed a master's thesis on human nonverbal behavior, which, among other things, taught me to think twice about using videotapes that inevitably require ad nauseam review.

In retrospect, my professional future was sealed in April 1974, when I attended the organizational meeting of an animal behavior seminar that was led by Dr. David Barash. David described an opportunity for a graduate student to study marmots in Alaska, about 60 miles northeast of Anchorage. To this day, I can easily picture myself trundling around the Hatcher Pass area watching hoary marmots, but I watched lots of them during the day I spent hiking, backpacking, and climbing in the north and central Cascades of Washington. Moreover, I knew that I loved outdoors and back country and that Alaska had an abundance of both (and some marmots too).

I spent three field seasons (June through September, 1974-1976) studying the foraging behavior and mating system of hoary marmots in the Talkeetna Mountains of south-central Alaska, about 60 miles northeast of Anchorage. To this day, I can easily picture myself around the Hatcher Pass area watching hoary marmots, arctic ground squirrels, pikas, and golden eagles, and marveling at the beauty and the serenity of wilderness. During my field seasons in Alaska, I often thought how unfortunate it was that as an undergraduate I had focused so intensively on medical school that I never questioned my pursuit of a chemistry degree. What I actually enjoyed as an undergraduate were organismal-level biology courses rather than chemistry or micro-level biology courses. Such courses, however, were not part of a chemistry degree nor in mind did they seem relevant to my goal of medical school admission. Luckily, I got back on track with my field research in Alaska on marmot social behavior.

The mid 1970's was an exciting time to be doing graduate work in animal behavior because Darwinian theory was being applied in new ways to studies of social behavior in a wide array of organisms, including humans. Building on a foundation laid down in the mid 1960's by George Williams and William, a number of evolutionary-minded investigators were interrupting everything from mating rituals to sex ratios to nepotism in an ecological and adoptive framework. In 1977, having collected but not written up my dissertation data, I started a line of research that would hold my attention for 10 years and figure prominently in my getting a job offer from the Psychology Department at the University of Michigan. Hamilton's "inclusive fitness" theory had revolutionized the study of many kinds of social behavior and embedded in Hamilton's theory was the assumption that animals could recognize their genetic relatives and alter their social interactions in accord with kinship. In my research on various species of ground squirrels, I sought to describe kin-recognition abilities and explain how such abilities developed as a result of early social experience.

In part because I became so interested in kin recognition, I had not completed analyzing or writing up my dissertation data when the Psychology Department at Michigan offered me a tenure-track position in July 1978. The dissertation was completed (but not until May 1979) and I moved to Michigan in January 1979. The following fall, Linda Forrest, with whom I had been "involved" on and off since September 30, 1967, accepted a faculty position at Michigan State University, and we settled in together in a small town halfway between Ann Arbor and East Lansing.

From 1978 through 1983, Dr. Paul Sherman and I collaborated to study kin recognition (my laboratory research) and nepotism (Paul's field research) in Belding's ground squirrels, which are found throughout the Cascades and Sierra Nevada in the western U.S. I continued my laboratory studies until 1987, but then decided to redirect my research toward the field, which I always found more satisfying than working in the laboratory. I believe firmly that researchers who pursue questions that interest them in environments that reward them will do better science than those who ignore their own interests and values to pursue a line of work. Acting on my belief, I began in 1988 a series of studies on the development of ground squirrel nepotism at a biological station in the eastern Sierra Nevada

near Mammoth Lakes, CA. besides some remarkable scenery and intriguing study organisms, the eastern Sierra Nevada offer outstanding mountain biking and back-country hiking.

The daily grind of conducting developmental studies on organisms that are active for only about three months/year (they hibernate the rest of the time) can wear one down and generate a real need for some serious R & R. My solution is to spend about four weeks windsurfing in the Columbia River Gorge after my field season ends. Linda and I pitch our tent in a campground in the Mount Hood National Forest and live a life dictated by wind speed, sail size, and physical stamina before we return to Michigan each fall to resume our “academic lives.”

Jeffrey Hutsler

My interest in the brain and behavior began prior to college when I worked as a counselor at a camp for disabled children located in the Santa Cruz Mountains of California. That first very hard summer turned into four more serving not only as a counselor, but as a program coordinator and head counselor. My interest and love of working with these kids was not, unfortunately, matched by a burning interest in academic pursuits. I attended San Jose State University and spent almost every moment of my time either working at the camp or a respite home for the disabled that was located in the heart of Santa Clara. My first few years in college were spent completing general requirements until I was forced to finally declare a major in my junior year. Given my outside interests, psychology seemed like an appropriate choice and I immediately gravitated to biopsychology. During my last college years, I continued to work at the summer camp and also worked a graveyard shift at the respite facility. In addition I worked for a short while in the main office of the Crippled Children’s Society of Santa Clara County where I organized and started a preschool day camp. These activities kept me so busy through my college years that when I reached my senior year I still had no idea what I wanted to do with my life. Although I was inspired by my work with disabled populations, camp counselor was not exactly a career. Given my lack of direction I decided to enter graduate school and began applying to neuropsychology and biopsychology programs. I was accepted into the psychology department at the University of California, Davis where I worked with Leo Chalupa on the development of the cat visual system. This productive research period taught me a great deal about development and how to examine the structure of the nervous system; however I did not have the sense of fulfillment and purpose that my previous activities had given me so I decided to switch my emphasis. I took a postdoctoral position with Michael Gazzaniga who had recently arrived at Davis to be director of the newly established Center for Neuroscience. Mike was looking for someone to conduct neuroanatomical studies in postmortem human tissue so together we began to study the organization of human language cortex. This was a new research undertaking for both of us and required setting up a neuroanatomy lab from scratch in his new space at Davis. These studies focused on regional variation of the microanatomical and macroanatomical organization of numerous areas in the temporal lobes of human subjects and established that subtle organizational differences in cortical circuitry can be present in homotopic cortical regions that are asymmetric in their function. After this great postdoctoral experience Mike decided

to move to Dartmouth College in New Hampshire and invited me to work as an adjunct faculty member in their psychology department. While at Dartmouth I had the opportunity to collaborate with Miguel Marin-Padilla, a semi-retired pediatrician who had spent the lion’s share of his career studying cortical development in humans using the Golgi technique. With the help of the local hospital we began the difficult work of collecting pediatric tissue to examine the development of human language cortex. In addition I was able to incorporate my interest in patient populations by collaborating with Ronald Green, a psychiatrist at the Dartmouth-Hitchcock Medical Center who shared my interest in the macroanatomical organization of language cortex in patient groups as assessed by MRI. This period of research shaped my current interests a great deal and in the Fall of 1999 I joined the faculty at the University.

Edward Smith (Biography from American Psychologist, APA Distinguished Career Award)

Edward Smith was born in 1940 in Brooklyn, New York. His parents were Jewish immigrants from Poland, his father a tailor and his mother a homemaker. Like many others from that ethnic background at that time, Smith went to Brooklyn College. He started by majoring in English. But that fizzled, and a friend, Irv Biederman, suggested he try psychology. He took statistics with David Rabb and experimental psychology with Elizabeth Fehrer and was moved by the elegance of this approach to the mind. He had found something to do with his life (and a way out of Brooklyn). He went to the University of Michigan in 1961 for graduate work in experimental psychology. Paul Fitts and Arthur Melton had recently come to Michigan to develop a program in human experimental psychology that emphasized information processing and memory. Smith decided to work with Fitts, although it was easy to keep contact with Melton’s group once Fitts and Melton had created the Human Performance Center and all their research activities were in one building. As important as Fitts and Melton were to him, Smith probably learned at least as much as from the other students in his cohort, who included, among others, Harley Bernbach, Irv Biederman, Allan Collins, Bob Crowder, Howard Egeth, Mel Guyer, and Amos Tversky.

The research at the Human Performance Center was much concerned with the use of reaction time measures to study elementary cognitive operations. In that intellectual context, Smith and Egeth came across the work of Dick Neisser and Saul Sternberg and began doing experiments to determine why Neisser’s work implied that mental comparisons could be done in parallel whereas Sternberg’s implied that comparisons were carried out in series (it turned out to be partly a matter of practice). At the same time that his research life was beginning, Smith’s reading opened up another vista for him. He was much taken by the work on concepts of Bruner and his students at the Center for Cognitive Studies (Harvard), and Smith subsequently tried to use reaction time paradigms to study issues in concepts in his dissertation. As graduate school neared to a close, Fitts and Melton helped him obtain a postdoctoral fellowship with Bruner and Miller at the Center for Cognitive Studies. In retrospect, it was a golden era.

It didn’t last. Paul Fitts died suddenly in 1965, casting a pall over the University of Michigan’s experimental psychology

area. Smith's dissertation was picked up by Melton. However his postdoctoral plans were dashed by the Vietnam War, and in 1966, instead of going to study cognition, he enlisted in the Public Health Service and was stationed at St. Elizabeth's Hospital in Washington, DC, a huge governmental psychiatric hospital. At first, he felt totally unprepared—he could barely spell schizophrenia or psychopathology, let alone study them. But he took courses with the resident psychiatrists and was soon doing research on memory and communication deficits in schizophrenia. (He also managed to publish a review of reaction time studies of cognitive processes that he had started writing at Michigan, and it had some impact.) He was surprised by now how much he liked working at a mental institution, but when his term of service was over in 1968, he left Washington and took his first academic job at the University of Wisconsin—Madison.

This was the heart of the 1960's, and the Madison campus was boiling over with political activity and counter culturism. In the calmer moments, Smith had his first real teaching experience and did research on word perception and short-term memory.

In 1969, he read an article by one of his Michigan cohorts that changed his career. This article was by Allan Collins (and Ross Quillian), and it offered a new account of how concepts were organized in "semantic memory." It reinvigorated Smith's interests in concepts and categorization. He began research on semantic memory, but the work didn't have a chance to develop at Wisconsin: Just a year later, Stanford offered Smith an assistant professorship, and the lure of that department took him West.

Smith started at Stanford in 1970 and remained there throughout the 1970's. Those years were very formative ones for him, as they shaped virtually every aspect of his approach to research and teaching. The faculty in the cognitive area included Dick Atkinson, Gordon Bower, Herb Clark and Roger Shepard; later they were joined by Amos and Barbara Tversky and Ewart Thomas. All of those people had their influence on Smith, as did Dan Osherson and Ellen Markman in the developmental area. Smith began a line of research on word perception and reading with one of his first students, Kathy Spoehr, and continued his studies of semantic memory with two other students, Lance Rips and Ed Shoben. During that first year at Stanford, Rips and Shoben were taking a scaling course with Shepard and scaled participants' ratings about how representative instances were of their respective categories. The results indicated that the instances of a concept and, furthermore, indicated that this variation might reduce to variations in a few dimensions or semantics features of the instances. This led to the ideas that each concept had a prototype associated with it (the prototype being composed of features) and that the judged representativeness or "typicality" of an instance was determined by its similarity to the concept prototype. This in turn led to a prototype-based model of semantic categorization. This work, combined with the research that Eleanor Rosch was doing at the University of California, Berkeley, had a substantial role in fostering the notions of prototypes and semantics features in cognition.

Smith continued to do research on semantic memory and related topics at Stanford, working with a host of talented students, including, among others, Gerry Balzano, Nancy Cantor, George Furnas, Arnold Glass, Keith Holyoak, Glen Kleiman,

Steve Kosslyn, Barbara Malt, and Greg Murphy. In addition, Smith spent a sabbatical year at Rockefeller University in New York, where he met two people who continue to have a major impact on his work: Bill Estes (a significant mentor) and Doug Medin. During that year, Smith and Medin began to write a book reviewing the concepts literature; it was published a few years later as *Categories and Concepts* (1981). That sabbatical year in New York also had another effect on Smith—it gave him a desire to return East. A couple of years later, in 1979, Smith left Stanford and moved to Cambridge, Massachusetts, to join the research staff of Bolt Beranek and Newman (BBN).

Smith had developed new interests in the then-new cognitive science in his last years at Stanford, and the intellectual life at BBN fostered this interdisciplinary approach, as psychologists, educators, and computer scientists routinely collaborated. His main colleagues there included Allan Collins and Dedre Gentner, and much of his work focused on the use of schemas in text understanding. But perhaps Smith's main line of research during the years in Cambridge was a collaboration with Dan Osherson that extended his prior work on prototypes for simple concepts. They focused on the issue of conceptual combination, that is, how people combine simple concepts into complex ones. Their first paper raised some difficulties in extending a prototype model of simple concepts to deal with conceptual combination. Later, they proposed a prototype model that could do some of this semantic work.

In 1986, Smith left BBN and returned to the University of Michigan (the fifth move since graduate school!). His move was motivated, in part, by his marriage to Anne Murphy and their desire to raise a family in a conducive atmosphere. In his first few years back at Michigan, Smith was joined by Medin, and he also spent time with Dick Nisbett, both constant sources of stimulation. In addition, Smith continued to work with Osherson, as well as Eldar Shafir, on the role of prototype concepts in inductive reasoning. They studied cases in which some instances of a category are known to have a particular property, and people have to judge the extent to which other instances of that category also have the property. Their work indicated that such inductive judgments were determined by the similarity of the known instances to the other instances as well as by the degree to which the known instances "covered" the space of all category members.

In the early 1990's, with his collaboration with Osherson and Shafir thinning by distance and with the departure of Medin from Michigan, Smith made an abrupt change in his research. He had always held somewhat antireductionist views, but when he became acquainted with the brain-scanning method of PET, his objections to biological work evaporated. He began a collaboration with his colleague John Jonides that uses PET to study basic issues in short-term or working memory (something of a return to issues that had captured him as a graduate student). Smith and Jonides have found, for example, that there are distinct neural systems for spatial and verbal working memory and that each of these systems has separate circuitry for passive storage and active rehearsal processes. This research program continues to thrive, and Smith hopes to bring these neuroimaging techniques to bear on issues in categories and concepts.