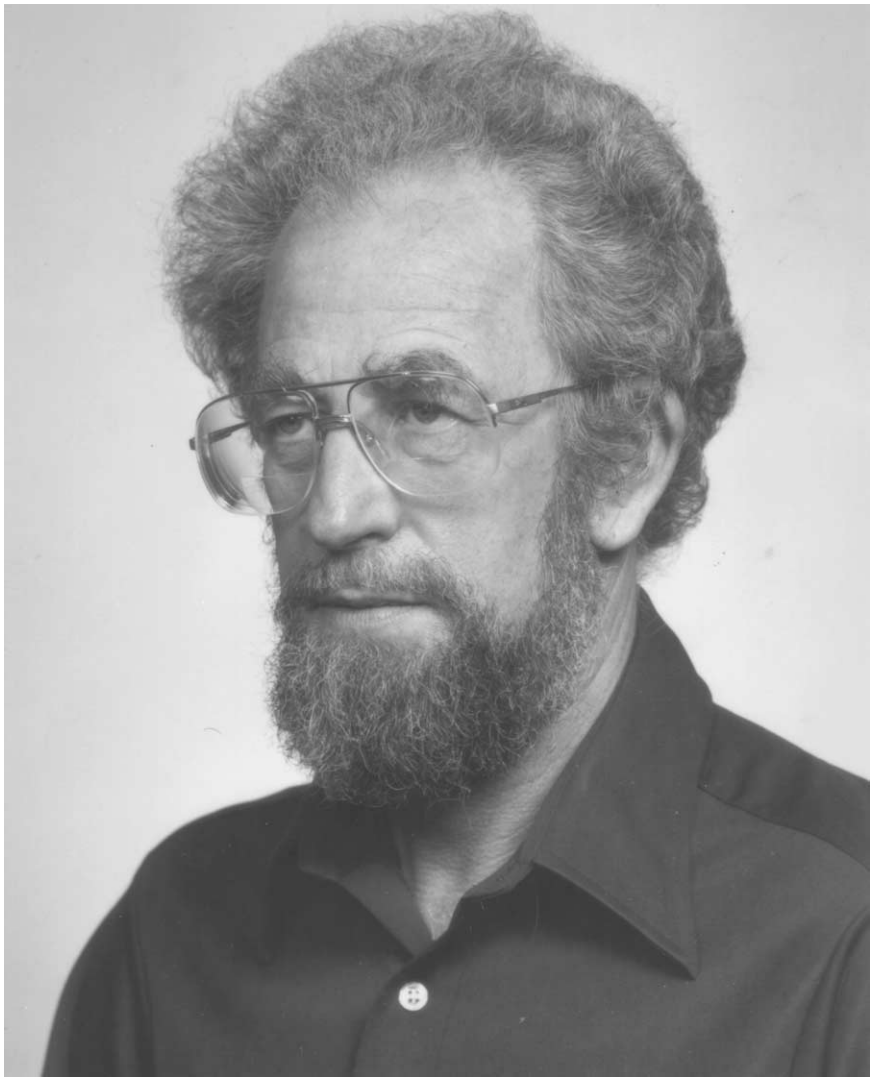

JULIAN M. DAVIDSON
(1931–2001)

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In Memoriam

Julian M. Davidson (April 15, 1931–December 31, 2001)

Edited by Benjamin D. Sachs

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Editor's Note

Julian Davidson was one of the three founding editors (with Frank A. Beach and Richard E. Whalen) of *Hormones and Behavior* and served in this capacity from 1969 to 1976. In addition, Julian made many significant research contributions to the field of behavioral neuroendocrinology. These included his studies on the neuroendocrine control of masculine sexual behavior, first in the rat and later in the human male. Julian Davidson was one of a small number of investigators who successfully bridged the divide between animal and human studies of sexuality and brain function. In light of his contributions to our field and his instrumental role in the creation of this journal, we are publishing a collection of remembrances by former students and fellows as well as colleagues and friends. Benjamin Sachs has overseen the solicitation and editing of these contributions.

Michael J. Baum
Editor

Julian Mordecai Davidson, one of the pioneers and leaders of his generation of behavioral neuroendocrinologists, died on New Year's Eve of 2001. Born in Dublin, Julian received his early education (and his dialect) in Glasgow and his higher education in Jerusalem (M.S., 1955, Hebrew University) and San Francisco (Ph.D., 1959, University of California, San Francisco). His mentors included some of the best of the previous generation of neuroendocrinologists: William Ganong at UCSF for his Ph.D. and postdoctoral fellowships with Charles Sawyer at UCLA (1959–1960) and with Frank Beach at UC Berkeley (1962–1963). In 1963 Julian joined the Department of Physiology at the Stanford University School of Medicine, where he spent his academic life.

That brief summary might suggest that Julian was destined for neuroendocrinology, but he had wanted to be a farmer. Toward that end, Julian worked after high school on a religious farm in England where, according to family lore, the manager said that Julian was the worst farmer he had ever seen, but also the most enthusiastic. When he was 19, Julian achieved his dream of working on a rural Israeli kibbutz, where he (and the kibbutz) soon decided that he was not suited for farming. He shifted to the study of animal husbandry, the major in which he earned baccalaureate and master's degrees at Hebrew University and likely the foundation of his interest in the reproductive physiology of animals.

During Julian's first years in California he met several of the people with whom he would form friendships and do science. Most important, Julian met Ann, whom he married in 1959 and with whom he had three children and a loving life partnership. While in Frank Beach's lab, Julian collaborated with *Gordon Bermant* and *Stephen Glickman,* two other postdoctoral scholars, in investigating the effects of limbic system lesions on copulatory behavior in male rats. (Names in italics indicate authors of remembrances in this collection, which appear in approximate order of the authors' first association with Julian.) Later, Bermant and Davidson would write *Biological Bases of Sexual Behavior* (1974), and Glickman and Davidson collaborated in studying hyenas in Africa and Berkeley. Among the frequent visitors was Benjamin Hart, of UC Davis, who was studying spinal mechanisms of sexual response, and Seymour Levine, a Stanford colleague. *Irving Zucker* soon joined the Berkeley faculty. These Bay Area scientists, along with others on the west coast, especially William Young's group from Oregon (including Robert Goy and Charles Phoenix), Richard Whalen's group from UC Irvine, and their several

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graduate students began meeting annually in about 1964 to present and discuss their research. Most often, this West Coast Sex Conference (WCSC) met in Palo Alto, with Julian as the always genial cohost.[1] The excitement of the WCSC reinforced Frank Beach's belief in the need for a journal devoted to what he saw as the emerging discipline of behavioral endocrinology (Beach, 1975) and in 1969 Beach invited Julian and Richard Whalen [also recently deceased; for his memorial collection, see Edwards and Etgen (2003)] to join him as founding editors of *Hormones and Behavior*. Julian continued as an editor of the journal until 1976 and as a consulting editor until 1986.

Julian is best known for his research on the neuroendocrinology of sexual behavior "of rats and men" (1981), but he had wide interests and published in several other domains. [See Morrissette and Myers (2002) for a complete list of Davidson's publications.] Most of Julian's early studies, and many of the later ones, concerned hormonal feedback mechanisms, first in the hypothalamo–pituitary–adrenal axis (including, in the spirit of the time, a study of the effects of LSD) and later in the hypothalamo–pituitary–gonadal axis. In 1966, however, Julian published his first papers on the neuroendocrinology of sexual behavior, and seminal papers they were. One of them (Davidson, 1966a) detailed the changes in the copulatory behavior of rats during the weeks and months after castration, including the counterintuitive (but robust) finding that the ejaculatory response of castrated rats follows after fewer intromissions than that of intact males. This study served as essential background for a related study (Davidson, 1966b), one of the first explorations of the effects on behavior of implanting hormones into the brain. Heimer and Larsson (1964) had recently shown that an intact medial preoptic area (MPOA) was critical to the display of copulatory behavior. Julian discovered that androgen implanted into the MPOA could restore copulation in castrated rats, thereby demonstrating the behavioral androgen sensitivity of this brain area. The next year, *George Bloch*, Julian's first graduate student, published with Julian a paper in *Science* on the stimulatory effects of an antiandrogen on the male reproductive system. By this time, Julian's lab had also been joined by *Erla Smith*, who remained as Julian's associate and research partner until the lab closed in 1993. She was coauthor of more than one-third of Julian's publications; more than that, if Julian was the lab's energizer, Erla was its stabilizing gyroscope. By the late 1960s, Julian had also begun a fruitful collaboration with Seymour (Gig) Levine, a colleague and friend in Stanford's Department of Psychiatry, on Levine's colony of squirrel monkeys, including several studies of the effects of stress. One source of stress that Julian and Gig shared was their isolation from the mainstream of their respective departments. In Julian's case, it eventually reached the stage that Julian was not so much *in* the Department of Physiology, he *was* the department, and after 1978 his lab was the sole occupant of a splendid old building, complete with rotunda and winding staircase. (Well, it was shared with cadavers stored there by the anatomy department.)

Julian tended to shrug off these slights, his relative peace perhaps facilitated by his meditation and other means of exploring altered states of consciousness. These states were also one of Julian's research interests, leading to an occasional paper (Davidson, 1976) and a remarkable book (Davidson and Davidson, 1980a) coedited with *Richard Davidson* (no relation). A chapter by Julian (Davidson, 1980b) in that volume explored the possibility of studying orgasm as the most common and accessible altered state of consciousness. By the late 1970s, Julian was more and more interested in applying to humans the kind of rigorous research he insisted upon with animals, and he went to Edinburgh to learn human sex-research techniques from *John Bancroft*, whose research in this area Julian most admired. Julian's first papers on the sexual response of men and women began to appear late in that decade and at an accelerated rate through the 1980s, with Julian soon schooling other colleagues in these techniques, including *David Rowland* and *Koos Slob*. As rewarding as he found this research, Julian was often frustrated by its pace, slowed by the necessary cooperation from physicians, informed-consent paperwork, and the fact that humans, unlike animals, sometimes failed to appear for their testing. (When I once considered making the move to human research, I was deterred by Julian's warning that for every hour that I might have spent collecting rat data, I should expect to spend at least 10 hours for analogous human data.) Ever open to research adventure, Julian joined Lawrence Frank and Stephen Glickman's studies on spotted hyenas in Africa (Frank et al., 1985, 1987) and he accompanied Gilbert Herdt to New Guinea to study a population of androgen-insensitive men (Herdt and Davidson, 1988).

Julian's excellence had always attracted to his lab a steady stream of graduate students (e.g., *George Bloch, David Damassa, Marcia Stefanick, John Clark*) and brief and longer-term visitors, including postdoctoral fellows (e.g., *Lin Myers, Diane Morrissette*) and sabbatical takers (e.g., *Ray Rosen, Manuel Mas, Sue Carter, myself*). But excellence was only part of Julian's attraction. As the following remembrances express again and again, Julian's enthusiasm for the enterprise of science, his warmth, and his rarely flagging good humor were essential parts of Julian's appeal. Julian was also deeply hurt by injustice to others, probably sensitized in part by the loss of much of his father's family in the Holocaust. He and Ann joined many a political march in support of human rights (e.g., against

[1] This conference was only open to west coast scientists, so former west coast students established the Eastern Regional Conference on Reproductive Behavior in 1969. In an evolutionary process, this ERCRB became the CRB as it lost its regional and eastern aspects and absorbed (or perhaps cannibalized) the WCSC. Then in 1997, the CRB mutated into (or perhaps was cannibalized by) the newly formed Society for Behavioral Neuroendocrinology.

the involvement of the USA in El Salvador and other Latin American countries) and they spoke with pride (and no little concern) about the passionate speeches that their daughter, Karen, then in her early teens, made at San Francisco rallies. Later Ann and Julian would join marches and rallies in support of gay rights, and they were active in Palo Alto's PFLAG (Parents, Families and Friends of Lesbians and Gays).

Among Julian's notable traits was his distractibility, and he exemplified the stereotypical absent-minded professor. For quite some time, probably a few years, these traits helped mask from his family, from his coworkers, and from Julian himself the early onset of Alzheimer's disease in the 1980s. Julian had been noted for his pithy, witty, and extemporaneous talks at meetings and seminars, and he first attributed the waning of this facility to nervousness or to side effects of medications. Other cognitive problems were similarly explained away at first, but in 1990 cognitive tests revealed the diagnosis of dementia, probably of the Alzheimer's type. [The early stages of Julian's disease are beautifully chronicled in Ann's book (A. Davidson, 1997).] Julian bore his developing disease with extraordinary courage and grace. By 1992, Julian could barely express himself with speech or understand it, but he nonetheless thoroughly enjoyed three celebrations of his career: a toast by his fellow members of the International Academy of Sex Research, a Festschrift organized by *John Clark* (1995), and an honorary degree awarded by the Universidad de La Laguna in the Canary Islands.

The Alzheimer's disease which ultimately took his life had long since taken from Julian two of the things he loved most: science and language. But until near the end, Julian sustained his joy of family, his joy of music, his joy of love, his joy of life. Julian has bequeathed to us a remarkable body of knowledge. More than that, he has bequeathed a large number of scientists and teachers whose research and critical thinking continues to be inspired by his life and work.

Julian is survived by Ann and their children, Benjamin (Assistant Dean of Students and Director of the Lesbian, Gay and Bisexual Community Center at Stanford), Karen Davidson de Sá (a reporter with the San Jose Mercury-News), and Jeffrey (a musician in New York City), and three grandchildren.

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Thoughts of Julian: a reminiscence

It was 1962. We had been postdoctoral fellows in Frank Beach's laboratory for some months when Julian Davidson arrived. It was immediately obvious that a "real" endocrinologist was on the scene. We were both impressed and charmed, as conversations ranged from science through gossip and back. On the gossip front, Julian's evaluations of eminent scientists were frequently punctuated with Yiddish summary descriptors and delivered in a (to us) unique Scottish accent. Although working on separate projects, we joined forces for a study of the effects of amygdala and hippocampal lesions on the sexual behavior of male rats (Bermant et al., 1968). One of us (G.B.) had assumed responsibility for the behavior, and another (S.E.G.) for the CNS work, while Julian examined the histology of the testes. It required some effort to persuade Julian to accept his place as a coauthor. On departing from Berkeley in 1964, our interactions with Julian followed different tracks.

Gordon Bermant

The first thing that comes to mind is how easy things were with Julian. During the 11 years that we worked off-and-on writing *The Biological Bases of Sexual Behavior* (Bermant and Davidson, 1974), I cannot remember a single instance of discord or disagreement. The harmony was due in large measure to the lightness of Julian's personal touch in relationships.

When Julian and Ann came to the Battelle Seattle Research Center for the 1974–1975 academic year, he devoted much of his time to developing his ideas on the physiology of consciousness. Over the course of the year, we had many conversations about mind and brain, states and streams of consciousness, and the roles of meditation and pharmacology in cultivating mental life. Not for the first or last time in his career, Julian was ahead of the game. When the important book *The Embodied Mind* appeared in 1991 (Varela et al., 1991), it occurred to me that Julian had been working toward the theme of the book, its world view actually, more than 15 years earlier.

Julian was a generous colleague. Through him I was invited to participate in the annual meetings of the Winter Conference on Brain Research for 5 years during the 1970s. What a splendid opportunity that was, to combine intense discussions of research with several hours of skiing each day. Julian reveled in the skiing as he did in the science. I shared a condominium with Julian and Ann on a couple of those occasions. At the end of each day there was time for a glass of wine and conviviality.

When the Davidsons visited NIH for a year in the mid-80s so that Julian could consult in one of the laboratories, my wife Geri and I spent time with them socially. One day we picnicked at a lovely spot in rural Maryland along the CandO Canal. Ann had packed a splendid lunch that we enjoyed while sitting in the warm spring sun. What sticks with me especially from that occasion is a sense of

Julian's engagement with the moment, his graceful ability to relax.

Steve Glickman

My contacts with Julian, after departure from the Beach lab, were limited to occasional conversations at scientific meetings or more prolonged days of work and evenings of play at several NIH Study Section meetings in the 1970s. That sporadic pattern was to change as the result of a trip that Julian and Ann made to Laurence Frank's field site in the Masai Mara Reserve in Kenya in the early 1980s. Laurence was engaged in a behavioral–ecological research program, and Julian and Laurence joined forces to probe the endocrine substrates of "masculinization" in female spotted hyenas. Laurence, Julian, and Erla Smith published the first paper demonstrating that (contrary to the existing literature) plasma testosterone was higher in males than in females, as long as the males were adult residents of the clan (Frank et al., 1985). In addition, they reported that the gonads were the primary source of testosterone in both male and female hyenas (Frank et al., 1985). But, Julian and Laurence decided that a thorough analysis of hormones, behavior, and sexual differentiation would require the access provided by a captive colony. At a lunch in Berkeley in the fall of 1982, they persuaded me that, if funding could be arranged, it would be worth establishing such a colony at the Field Station in the Berkeley Hills.

The original NIH grant was prepared in Spring 1983, with Julian writing the endocrine sections and Laurence providing expertise regarding hyena behavior and field biology. In a prescient addition to the Research Plan, Julian added androstenedione to the list of hormones to be assayed. This later proved to be the primary circulating androgen in female spotted hyenas (Glickman et al., 1987). When radioimmunoassays could no longer be carried out in Julian's laboratory, he arranged a collaboration with Pentti Siiteri that has endured for many years. Our last collaborative paper, a review of sexual differentiation in spotted hyenas, was published in 1992 (Glickman et al., 1992). Julian's powers had begun to fray, but his contributions in the early years of the project were crucial, and working with him delightful. Personally, I am still in his debt.

Some final thoughts

We last saw Julian in Florida on the occasion of a Festschrift held in his honor in 1994 (Clark, 1995). By that time his disease had rendered him unable to follow the flow of science, but he was perfectly capable of expressing sincere appreciation to the friends and colleagues who attended the meeting. After the meeting, Julian, Ann, and some others of us went to a state park in search of birds and alligators. During the outing everyone had fun, Julian not least among us. We treasure the photographs of Julian from that time. The extent to which he remained so "distinctively Julian" provides remarkable testimony to the compelling, unique nature of his personality.

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A gifted mentor

I first met Julian in 1963. My interest in hormones and sexual behavior had started a year earlier, in 1962, while I was a doctoral student in psychology at Claremont Graduate School. I was recording the electrical activity of various areas of the rat brain during the stages of the estrus cycle, busy implanting bipolar electrodes and measuring wave patterns. I was also spending hundreds of hours at UCLA studying articles relating sexual behavior to neural activity and neuroendocrinology, most of it over my head. I couldn't shake off the uneasy feeling that my focus was too narrow, that despite the fact that my main interest was in hormones and behavior and my feeling that I belonged in psychology, I would be better off switching to physiology. Thus, it was with considerable trepidation that I interviewed at Stanford, worried that I would be overwhelmed by all of the material to learn, but seeing no other way. Enter Julian. As part of the interview, he asked me how I thought negative feedback regulation of gonadotropins worked. Positive and negative feedback were the topics of many of the articles I had read (including some of Julian's work) but, despite having done my homework, I could not formulate an answer—I did not know how negative feedback worked! My anxiety was extreme. Julian, however, didn't seem to be bothered at all with my ignorance and simply discussed with me the many possibilities of negative feedback regulation, pointing out which issues he wished to research. Before I knew it the anxiety was gone; I was caught in Julian's excitement for the hunt, an excitement that was undiminished by the fact that I couldn't put it all together. Thus, I settled for a Master's degree in psychology and became Julian's first graduate student, in 1964.

This trait of Julian's, of encouraging intense, detailed discussions without having to pretend that we knew precisely what was what, put me at ease; it felt real. This was not to say that we were not expected to know our stuff. Quite the opposite. Knowledge of the literature, the facts, was a serious matter to Julian. However, the issues under consideration were always considered as incomplete. In my experience, Julian was always interested in the bigger picture and, fun and exciting as it was, he felt that whatever details we were researching would answer only a small part of the whole. (Julian was an unabashed admirer of Frank Beach. I remember once, in a mid-80s West Coast Sex

Conference, I had presented data showing that CCK microinjections affect lordosis but that effects were dependent on the brain area and dose of estrogen. Afterward, in private [thank goodness!] Frank asked, "George, what does it *really* mean?" Not only was I at a complete loss for words, but I was instantly reminded of Julian.) Julian's desire to be connected to a bigger picture came out in many other ways. At times he would mention that he should study humans, indicating his dissatisfaction with rats. (Julian did eventually incorporate humans in his studies.) Julian was also intensely interested in spiritual things, indicating his dissatisfaction with research, even science, as a way to feed his inner life. The issue of spiritual growth often became a topic of discussion between us. In fact, during a 10-year period starting after my postdoc when I pursued other aims and quit university-level academia, Julian made it a point to get us together now and then, not so much to talk about his research (of course, it came up) but to connect about life in general. During these years (1974–1984) Julian more than once mentioned that he longed at times to do something like what I was doing. During these conversations he was once again my mentor, reinforcing the feeling that research was a wonderful way to spend one's life, but that there was also a bigger picture, and it was important to pursue it.

Julian was, of course, a wonderful teacher. He was constantly teaching, connecting his extensive knowledge of the literature to the topic at hand. He also made sure that I met some of his colleagues. These meetings were important, in no small part because it was not as yet fashionable to think of behavior as an important way to look into the workings of the brain (at most colleges and universities "psychobiology" was not part of the curriculum), and there were not that many of us. At the early West Coast Sex Conferences I met Frank Beach, Bill Young, Bob Goy, Irv Zucker, Dick Whalen, Ben Hart, and others (these meetings were quite small, fewer than 20 people). Through Julian I also met Gig Levine (a member of my Ph.D. committee), Fran Ganong (with whom I did a postdoc), and Charles Sawyer (a friend during my postdoctoral years with Roger Gorski). Julian was also protective of his students. When I was defending my Ph.D. dissertation at Stanford, a member of my committee asked how I would go about synthesizing testosterone. I didn't know and was about to say so when Julian cut in, "I would ask a chemist."

Julian instilled in me a way to do research, and I have often noticed that what I expect from my students is not that different from what he expected from me. He wanted us to think hard and clearly. The issues needed to be understood, meaning not only a thorough familiarity with the relevant literature, but consideration of every detail of the experimental protocol. (I remember Julian's dismay when someone would interpret a short ejaculation latency as a sign of increased sexual behavior.) Also, if things got too complicated something was probably amiss; good ideas were simple ones (testable with relatively simple statistics). Importantly, the research had to be exciting, meaning that it needed to be connected with our motivation to get an answer, and had to be fun. This is the Julian I will always remember.

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Provincial meets the 1960s

It is impossible to condense my memories and relationship with Julian into a short essay like this, but I will endeavor to do so. My first meeting with Julian was in the spring of 1964 when I met with him in Jordan Hall at Stanford University to ask if he would consider having me as a postdoctoral fellow, so that I could be trained in the exciting new field of reproductive neuroendocrinology. I wore heels, nylons, and a dress suitable for a soon to be Ph.D. from the University of Washington School of Medicine. Much to my surprise, I was greeted by a young, casually dressed, medium sized, bundle of energy with short wiry curly hair, a mustache, and a Scottish accent tinged with some other accent I later learned was Israeli. He was not what I had envisioned as a Stanford Professor. It was, I believe, a fortuitous meeting for both of us. He agreed to take me as a postdoc, and in December of 1964 I began my work with him, first as a fellow for 3 years and then as his research associate for an additional 25+ years until his early retirement in 1993. As Ann Davidson often remarked, Julian had two wives to attempt to keep him in order, Ann at home and I in the laboratory.

My first true understanding that Julian had absolutely no concept of time came when my husband and I attended our first party at Ann and Julian's home in Menlo Park. We arrived promptly at the designated hour, only to find a young Ben running around nude and the Davidsons clearly not ready for visitors, much less a party. I was embarrassed, believing that we had come at the wrong time or day; such was not the case. Julian never developed any real concept of how long it would take to do anything; this applied to the time required to get to any airport in order to catch a flight, how long any meeting would last, how long it would take to get a grant application ready for submission, how long a particular surgical procedure might take, or for that matter how many hours there really were in a day. He was not concerned about such mundane things. His mind was too full of questions, ideas, and plans. This was his great asset and talent, and I think this trait enabled him to accomplish so much in a relatively short period of time; it is almost as if he had some unconscious warning that his time would be too short.

Julian also had the ability to multitask, which was a gift unless he was driving down Highway 101 in the midst of traffic, carrying on a detailed discussion with someone in the back seat (he believed in eye contact). Most of us

learned by experience to offer to drive. It was much easier on the nerves. I soon became aware that money was not an important aspect of Julian's life. One never went to lunch, coffee, etc., with him unless one had enough money or plastic on hand to cover the bill in case of need. By necessity, I quickly became the financial manager of the lab.

Julian was magnetic; he was electric; he was insatiably curious about all things, not just science. Julian by his very nature expanded my world. My rigid 1950s background was not prepared for his 1960s activism, his cosmopolitan attitude, and his world experiences. Working with and for him was a challenging and exciting experience. We had people from all over the world spend time in our laboratory to learn from us and for us to learn from them. We experimented with all sort of creatures: rabbits, rats, mice, monkeys, bats, goats, hyenas, and of course the human animal. One of my fondest memories is laughing so hard while watching Julian trying to net a squirrel monkey at the Stanford Research Institute.

Julian was always ready for something new and/or different. He was very tolerant of those who worked with him, be they staff, student, or postdoc. It was not unusual for us to have a small baby or child in the lab at odd times due to baby-sitting complications. He had a true love affair with the written word. I learned that very early on when I went over the first draft of my initial manuscript with him. I could have become very discouraged with my writing skills if I had not realized that he loved words and that to him writing was an art form. I often teased Julian by asking him about his "word of the month."

The last 3 years of our working relationship were hell. Hell for him because he knew his brilliant mind was failing rapidly, and hell for me because I knew our symbiotic relationship was no longer viable. All my teasing over the years about him being the prototypical absent-minded professor had become a truly terrible reality. The glass had shattered and there was no way to mend it, nor could it be replaced. My job was to pick up all the tiny pieces, to remember the great times and wonderful people, and to allow both of us to retire with as much peace and dignity as possible.

Erla R. Smith
Penn Valley, CA, USA

Good-bye laddie

No sooner had Julian departed my house in 1991 than I began sobbing uncontrollably. My dear friend, he of the nimble mind, quick wit, and broad learning, was showing signs of the cognitive decline that would eventually leave him speechless. Alzheimer's disease inexorably robbed him of his most endearing features, but spared some unnameable essence that continued to profoundly affect those in his orbit.

Julian was one of the preeminent behavioral endocrinologists of his generation, a pioneer investigator of the neural substrates underlying hormonal control of sex behavior and gonadotropin secretion. No one was more successful in developing useful animal models of human sexuality. His studies were elegant in their simplicity and established principles of broad significance. On reading them, you could be excused for muttering to yourself "now why didn't I think of that?" One notable example was the demonstration that male rats, be they vigorous copulators or completely disinterested in sexual activity, all had blood concentrations of androgens far in excess of the amounts needed to activate mating behavior. This explained previous failures to establish correlations between individual differences in mating activity and hormone concentrations; it reinforced the notion that the biological basis for such differences must lie in hormone target tissues in the nervous system.

Julian followed his muse, often disregarding fashionable trends, and was not uncomfortable swimming against the tide. A classically trained endocrinologist, he took a bold step, frowned on by physiologists of that era, when he sought training in animal behavior. He subsequently embarked on a life-long study of various behaviors in animals. Later, when physiologists were increasingly abandoning organismal approaches in favor of reductionistic molecular analyses, he followed a different route and applied the insights gained from animal research to the study of human sexuality. Again, he generated a string of innovative results, some overturning long-held dogma. He was also one of the early investigators of the physiological sequelae of meditation. His nontraditional demeanor and pursuit of subject matter not typical of a physiologist were not always acknowledged with timely institutional rewards. Julian bore these slights with dignity.

Julian was deeply passionate about his work, but not at the expense of taking himself or the enterprise too seriously. His presentations and personal interactions were laced with humor, sometimes of the outrageous variety. His generosity was reflected in the support and assistance he offered many individuals, including a string of visitors to his Stanford laboratories. Over the course of 20 years Berkeley graduate students and postdoctoral fellows received material assistance and encouragement from Julian and his long-time associate Erla Smith.

Julian hungered for the diverse experiences life can offer. He tried to persuade me to accompany him to Cuba long before such journeys became fashionable; in the early stages of his terminal illness he invited me to travel with him to India. Poet Mary Oliver could have been describing Julian when she wrote:

> When it's over, I want to say: all my life
> I was a bride married to amazement.
> I was the bridegroom, taking the world into my arms.

Julian was not without foibles. He was a charter member of the multitasking fraternity, habitually late and overextended, as well as rather absent-minded. A visit to his

Stanford labs entailed passing through a room filled with medical school cadavers; once this challenge was over, Julian wasn't always there to meet you, but all was mitigated when he eventually greeted you with the wonderfully accented "Hello laddie." For many years we would take long walks in the hills adjoining our respective campuses. With the progression of his disease and his retrenchment from academic activity, the frequency of these jaunts increased. I treasured his friendship and company and often revisit memories of those times.

At the outset of a visit to Julian about 3 years ago, he acknowledged my partner Ellen's presence but refused to so much as look my way, despite repeated urging from Ann, his wife, that he do so. After a few minutes and occasional surreptitious glances my way, a broad smile creased his face, he gently cradled my head in his hands and rained kisses on my face. There are few greater gifts.

Ann was a blessing in Julian's life; his loving, caring companion, mother of their three accomplished children, each a free spirit in the Julian tradition. Her book, *Alzheimer's: A Love Story*, chronicles one year in Julian's life (Davidson, 1997). It is a comfort to the many who have read it, unflinching in relating the heartbreak but also the great rewards to be derived from living with a person of diminished capacity.

A scientist's work is ephemeral. Few of us leaves behind an oeuvre that endures for more than a decade. Our impact on our family, friends and community is more long-lasting. Julian, a man with a deep sense of social justice and an egalitarian perspective, was not only an accomplished scientist; he participated in many worthy causes outside the realm of science. He leaves behind a large group of individuals whose lives he enriched and in whose memories he continues to dwell. Goodbye laddie, I miss you so.

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A grateful student remembers

Economic statistics tell us that American retail spending is highest in December. The reality of this fact hit home in January of 1969 during my first year as an undergraduate at Stanford. Driven by impending poverty, I responded to a posting in the student employment office for a part-time laboratory glassware washer in the Physiology Department. Clutching the referral information, I found myself in one of the original sandstone buildings of the outer Quad, knocking on the second-floor office door of Dr. Julian Davidson. After a distracted period of silence, and just as I was about ready to conclude that the office was empty, a gentle voice with a unique Scottish accent bade me to "come in." A great head of curly hair, a full beard and a level of enthusiasm that was almost overwhelming—Julian seemed to reflect perfectly my mental image of a science professor. Only later, after coming to know Julian's high ethical standards, honesty, inquisitive mind, and unbounded creativity, did I realize the depth of the resemblance. A meeting with Dr. Erla Smith, a quick tour of the lab, and I was hired.

Within a few months, my checking account was off the critical list and I was asked if I wanted to work extra hours providing help with an "assay." Experiences that lie far outside the realm of expectation tend to leave lasting impressions and this was the case for my first exposure to research. The assay being performed was one of the original biological assays for LH and involved injecting plasma samples into immature rats and measuring the depletion of ascorbic acid in the ovaries (OAAD assay). Unlike the high through-put techniques of today, these assays were extremely labor intensive, involved everyone in the laboratory, and took most of a day to complete. Erla was a natural straw boss. I watched with amazement as she efficiently directed the set-up procedures and instructed each of us in our tasks for the assay. About 15 min after the assay had begun, Julian came rushing into the lab, a little winded by a hurried bike ride from home and a run up the stairs. The old physiology building was partly serviced by vintage 1900 wiring but the electricity and energy that flashed through the air when Julian entered the lab was quintessential 21st century. He cheerfully greeted everyone with a "hello lassies and laddies" while putting on his lab coat and taking a seat next to Erla at the surgical table. With a warm return greeting and a knowing smile, Erla passed him a set of surgical instruments reminding him to be sure to remove the ovary. At the time, this instruction seemed odd given that the basic premise for this assay, as it was explained to me, involved the removal of ovaries and measurement of their ascorbic acid content. While performing ovariectomies, Julian summarized the findings of several articles that he had recently read, told stories about his children, and initiated lively discussions on topics ranging from anti-war activism to a new course on human sexuality. Standing at my station, I could see that Erla was continually glancing in Julian's direction. The reason for Erla's curious instruction and watchful eye became clear as the day progressed. Engrossed in a conversation or a debate, on several occasions Julian would begin closing an incision and Erla would pipe up in midsentence, "Did you remember the ovary?" Invariably, Julian would double check only to find an ovary still in place. Julian was a master at multitasking long before it was so readily supported by technology. Interactions with students, consideration of novel ideas, discussions about interesting scientific topics all proceeded in parallel—it was just the mundane or routine that tended to fall off the radar screen.

Julian and I had many long talks about science as a career and the academic way of life during my undergraduate years. I still remember and appreciate his candor as we discussed the good and bad aspects of his experiences in

academia. His message to me was simple. Be sure to pick a career that you are passionate about and don't be afraid to try something different. Julian's warmth, passion, and honesty were truly compelling. By the middle of my junior year, I realized that I had become infected with Julian's love of science and decided to pursue graduate studies. Coincidentally, another love appeared in my life at that time, a "frosh" by the name of Sarah. Sarah and I would often meet in the lab after work and frequently be greeted by Julian, always with a welcoming smile and sincere interest in how we were doing. The very special bond that Julian shared with his wife, Ann, was evident to everyone in the lab and undoubtedly helped to imprint our own fledgling relationship. Thirty-two years later, Sarah and I look back and marvel with gratitude at the profound impact Julian, Ann, and the Davidson Lab had on our lives.

For many compelling reasons, some more obvious than others, I began graduate studies in Julian's laboratory in 1972. I was drawn to the endocrine aspects of the work being done in the lab—developing radioimmunoassays and studying feedback mechanisms. However, Julian tried to make sure that all of his students conducted behavioral studies and that we were mindful of the relationship of our studies on hormone–behavior interactions to medical practice. As biological research was becoming increasingly more molecular and reductionistic, I think that Julian wanted to be sure that we didn't lose sight of the importance of understanding the integrative nature of biological systems and the ability of science to improve the human condition. From the rough drafts of our first papers, that were nearly obliterated by the red ink from corrections, to the advice he gave us and the example he set, Julian always will be remembered by his grateful students as an influential mentor, a lover of science and of life, and a wonderful friend.

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Receptivity and rigor

I first was introduced to Julian Davidson when I was a graduate student in the early 1970s. Seymour Weingartner, who was then at Plenum Press, thought that, in addition to being namesakes (though unrelated), we shared many interests in common, among them a passionate interest in the scientific study of consciousness, a then rather unfashionable topic in the biobehavioral sciences. What most impressed me about Julian in these initial encounters was his generosity, his curiosity, and his willingness to expand his intellectual horizons. These qualities made an enormous impression on me during these formative years of my career.

Julian and I collaborated on editing a book entitled *The Psychobiology of Consciousness* (Davidson and Davidson, 1980). This book was one of the very first on this topic and presaged the current interest in the neurobiology of consciousness. It was a delight working on this with Julian; he generously dedicated an enormous amount of time to this project and, for someone considerably his junior, he always treated me as an equal partner and helped me with my own chapter for this volume. Julian had a wonderful balance of scientific rigor and openness that impressed me and indelibly affected me. This is a combination that I have striven for in my own work since this collaboration early in my career.

Julian had a firm conviction that subjective changes in consciousness associated with particular practices or behaviors were subserved by systematic changes in underlying biology. While the associations that we were able to document at the time the book was edited were quite coarse, they nevertheless hinted at the prospect of developing a more rigorous science of consciousness. That challenge has now been forcefully taken up in recent times with prominent neuroscientists taking on this issue with creative new approaches (e.g., Crick and Koch, 1995; Tononi and Edelman, 1998). Julian would have been gratified to witness this enormous explosion of interest in this topic but would also have asked deep questions about the importance of the work, where it was going, and whether it really explained the central features that mark human conscious experience as unique.

For those who knew him, Julian's pursuit of science was passionate and fun. He was infectious in his enthusiasm and provided an extraordinary model for those around him. His presence in the academy will be sorely missed but his mark will be enduring.

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Mentor, special friend, head of family

I met Julian in 1975, when he was studying altered states of consciousness on sabbatical. I immediately liked this curly-haired, bearded man and looked forward to Stanford's physiology graduate program. After our meeting we had a chaotic lunch with his young children. It was fun to see him as a father and even more interesting to watch his family grow and mature together over the next 27 years.

In his lab too I felt that I had joined a large, close-knit family, with Julian as head and Erla Smith as his partner. Two graduating students and David Damassa were older siblings; Barbara Tennent was a close sister; an English technician provided aunt-like support; Gary Gray, a postdoctoral fellow, was a close cousin; and Ben Sachs, on sabbatical, felt like a favorite uncle. At meetings and conferences, we felt a family pride about belonging to Julian Davidson's laboratory.

In the late 1970s, Stanford's Physiology Department consisted mostly of nearly retired or emeritus faculty. There was "political" pressure to end the graduate program. Julian staunchly protected me, his new "daughter." Fortunately, the pressure subsided when the department was moved from its "prime" campus location, in 1978, into an old building that had withstood the 1906 earthquake. We loved our spacious laboratory, windowed rotunda, and Julian's homey office. I have many memories, sitting on his couch, engaged in scientific discourse, with him writing enthusiastically on his blackboard. Unfortunately, the building was condemned after the 1989 earthquake and the lab moved again. Whether out of fondness for his old office or as a result of his failing memory, Julian returned to the abandoned building often, rode his bike down the long hall, and sat in there, for hours. After Julian's death, his wife Ann, Danusia Szumoski, a later graduate student, and I visited his office and found autobiographic jottings on his blackboard.

After the move, new students were accepted, the lab family turned over, and I became one of the "older siblings," with my own projects. My growing interest in exercise physiology led me to the Stanford Center for Research in Disease Prevention (SCRDP), where I became involved in a training study of men. When my rat project failed, Julian felt a responsibility to focus my work toward a dissertation topic. He raised questions about spontaneous seminal emission in the rat and suggested specific penile reflex experiments. I was growing increasingly uncomfortable with animal research, however, and couldn't see the relevance of my work. Yet, it was clear that the time had come for me to accept guidance from the head of the family.

Julian was about to leave for another sabbatical when I announced that I was going to leave the program. He visited me at my home and persuaded me to adopt instead several goals for the year: a project interviewing spinal cord injured men about their sexual function and measuring their testosterone levels; studying sex hormones in the SCRDP exercise study; taking additional courses relevant to clinical research; and completing my doctoral examination.

By the time Julian returned, I was clear about the light that my rat studies could shed on human sexual reflexes. Ironically, while I was ready to pursue a more traditional route to completing my training, Julian had decided to shift his personal research to human sexuality. The lab's character was totally transformed. New members of the family were doing human research; Erla, Jack Clark, and I were the last of the "rat pack." I didn't mind; the new family was just as interesting and dedicated as previous ones, and just as proud to be in Julian Davidson's laboratory.

After celebrating my graduation with the 1982 family, I considered two career choices: a postdoctoral project in molecular physiology at UCSF and a fellowship in cardiovascular disease prevention at the SCRDP. One was a natural extension of my doctoral work and would develop me as a neurophysiologist and behaviorist. The other would reroute me completely, but would enable me to pursue my interests in the role of sex hormones in human diseases. Julian counseled me as a mentor and true friend. He made it clear and obvious that I should pursue the path that would please and excite me as a whole person, not just as a scientist.

My fellowship at the SCRDP set a course which would eventually involve me in three of the major clinical trials of postmenopausal hormones for heart disease, the PEPI, HERS, and WHI trials, and pull me into research on breast cancer and osteoporosis. Julian shared my early excitement about these studies over regular lunches on the Stanford campus. At one such lunch, about a year before Julian's Alzheimer's diagnosis, I first realized something was wrong. I asked a neurologist who had trained in Julian's lab to arrange a meeting with Julian to assess whether he had had a stroke. His disorientation became more obvious over time and we no longer discussed science.

After his diagnosis, we continued regular lunch meetings. A little later, Julian often just appeared at my lab, leaving his bike in the doorway of the building. And when Julian could no longer navigate his bike, Ann, Danusia, and I would picnic with him in the hills behind campus, on his birthday and at other times during the year.

In more recent years, Danusia and I joined Ann to celebrate Julian's birthday, and other occasions, at a lake not far from his residential facility. He enjoyed walking, whistling, and humming (*Oh My Darling Clementine, You Take the High Road, Hava Nagilah*). We always brought a chocolate cake from Prolific Oven, a tradition from early lab days. Occasionally, Julian, who no longer spoke, would finger the cake, lick his fingers, and surprise us by exclaiming "lovely" or "delicious"; then, he would peer into our eyes from under his wild, bristled eyebrows, and, for just a moment, we knew he was still there.

The last time Danusia and I saw Julian, Hanukah 2001, he could no longer eat solids, but enjoyed the chocolate malt we brought. I loved Julian as much in his Alzheimer's years as in my graduate and later career years, and I feel fortunate to have grown up in the families of Julian Davidson's laboratory.

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From rats to human beings

I first met Julian Davidson in 1973, at a meeting in Berlin organized by Schering. I had just started my career as a "behavioral endocrinologist" at Oxford, with a psychophysiological study of the effects of cyproterone acetate, the Schering anti-androgen. Julian was already a world authority on the neuroendocrinology of sexual behavior in the rat. Based in California, but born in Ireland and with several of his family in Glasgow, Scotland, we had other links. We

kept in touch and in 1979 Julian and his wife Ann spent a few months in Edinburgh, where I was working at the Medical Research Council's Reproductive Biology Unit. Julian had made a groundbreaking career decision. Already established as a leading animal researcher, he wanted to extend his research interests to the human. His visit to spend time with me was to help him get started.

We were currently running psychophysiological studies of the effects of testosterone (T) replacement on sexual responsiveness in hypogonadal men. Julian and I embarked on a new but ill-fated study. Could we get the same effects with dihydrotestosterone (DHT) that we were getting with T? Using rectal suppositories as our method of administering DHT, we set about increasing DHT to around or just above normal circulating levels, but with no observable effect on sexual responsiveness. It then dawned on us that we were attempting to reinstate normal plasma levels of DHT, when plasma levels of this hormone were irrelevant. DHT is mainly produced in the periphery by 5α-reduction of T. In hypogonadal men, we should have been giving the same amount of DHT as we would normally give of T. I was relieved to have Julian there to share the embarrassment of this experimental *faux pas.*

I learned a great deal from Julian during those few months, and he learned enough about researching humans to take off. He returned to Stanford and in the same year published a paper on androgen replacement in hypogonadal men, soon followed by a succession of other influential papers and chapters on human sexuality, including his essay on "The Psychobiology of Sexual Experience," a seminal study of psychophysiological assessment of the effects of testosterone replacement in hypogonadal men, and another on the effects of T replacement on nocturnal penile tumescence. By now Julian was scooping our lab, which was doing related research.

Characteristically, Julian soon gathered round him a group of enthusiastic human sexuality researchers. Including the studies already mentioned, the lab published significant research on diverse aspects of human sexuality: the role of oxytocin, the effects of menopause, 5α-alpha reductase deficiency, aging in males, and diabetes, as well as the effect of T on tactile sensitivity in men, hormonal treatment of male-to-female transsexuals, and hormonal effects of spinal cord injury.

In striving to understand the complexities of hormonal control of human sexuality, the evidence from animal studies has been crucially important. Most of the experimental manipulations are only possible in animal research. Whereas there have been animal researchers, such as Frank Beach and, more recently, Ben Sachs and Kim Wallen, who have taken a particular interest in the relevance of animal studies to understanding human sexuality, Julian Davidson was unusual, having established himself as a leading animal researcher, in getting "hands on" experience with human studies. I very much hope that others will follow this example (in both directions), as it adds valuable depth to the field.

My last scholarly encounter with Julian was at a conference on androgens in Florida in early 1990. We were the two behavioral speakers, Julian on sexual behavior, I on aggression. Julian's presentation covered the ground but was not up to his usual standard. That evening, Julian, Cindy Graham, and I went for a walk along the beach. Julian told us that he thought he was losing his memory. Within a few months he had been diagnosed as suffering from Alzheimer's disease.

I had a few more opportunities to spend time with Julian before he died. The most poignant occasion was in June 1993, when the International Academy of Sex Research had its annual conference at Asilomar, California. The program included a special tribute to Julian Davidson. Julian was of course there, and several people spoke of their experiences with Julian over the years. Prior to the meeting, with Ann's help, Julian had prepared a statement he wanted to make to the Academy members. He and Ann asked me if I would read it. The statement described how Julian was changing; how he was striving to become a new and different kind of person, accepting that there were many things he could no longer do. As quoted in Ann Davidson's book about Julian (Davidson, 1997), he ended with the comment "My brain ain't what it used to be. I also have arthritis in my big toes. But I want you to know that, in between, everything works just fine." The audience exploded into laughter, relieved to escape, at least temporarily, from the intense emotions generated by Julian's statement. Best of all, Julian was, for the rest of the evening, a very happy and proud man.

I am proud to have known Julian. I am indebted to him for his scholarship and for his friendship. I will miss him.

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Aphrodisiacs and long shots

I first learned of Julian Davidson in the early 1970s while taking a physiological psychology course (to complete my social science requirements at Southampton College) taught by Bob Orndoff, who had trained with Frank Beach at Berkeley. But there were several delays before we met. After my delayed (by nonsense and a visit to Vietnam) B.A. degree and a first marriage, I went to work at Kings County Hospital as a medical technologist and fostered my fledgling interest in behavioral neuroendocrinology by working with Carol Diakow at Adelphi University. Then there was an ad for "Informal Study in Neuroendocrinology" at UCSF, so I moved to California. Well, things did not go well, and several years later I went to San Francisco State for a

Masters, resurrecting the things I had learned about hormones, neurons, and behavior (sex, of course) from Bob and Carol. As I was finishing, I thought that it would be great to work for Julian Davidson, who happened to be out of the country. When he got back, I went to see him and he said he didn't have any funds for me. I countered that I had until October to use up the last of my GI bill benefits. So we talked about war and peace and . . . for hours and he supported my application. I joined the lab in January of 1980 and as it turned out I was to be his last graduate student. At that point there were two other graduate students (Marcia Stefanick and Walter Greenleaf). During my 3½ years under his wing, there was the everconstant Erla Smith, an everchanging number of postdocs and technicians, and a variety of visiting scientists (Sue Carter and Manuel Mas spent the most time there during this period). I started on some studies extending my Master's work and fell into the hyperpolactinemia abyss. Then I visited some friends at UCSF and met Loek van de Kar. Loek told me that J. P. Long at Iowa had synthesized a new dopamine agonist that was an aphrodisiac, so I talked to Julian and he invited Loek down for lunch. They talked some in Hebrew—Loek was surprised by Hebrew with a brogue! Anyway, we got the drug from J.P. and found that it was not really an aphrodisiac, but did induce ejaculation in and *ex copula* (Clark et al., 1982; Stefanick et al., 1982). Later it turned out that it was a better serotonin 1A agonist than it was a dopamine agonist (Clark et al., 1985), but that's another story. As I continued down the road of sex and drugs, I noticed that Marcia was upset—it turns out that she had been trying to get Julian to let her do studies on opiates, and Julian told her that the lab was not going to do sex and drugs. I guess it's all in the timing, and Julian just couldn't resist the aphrodisiac angle (opiates were thought to be inhibitory after all).

After a while Julian told me that I had done enough but needed to get some neurochemical data. But I was not quite ready to stop. I had been thinking that the field of sex and drugs had a plethora of studies on dopamine and serotonin, but not much on adrenergic influences. Julian left the country and I convinced Erla to help me do some studies on adrenergic drugs. We started with clonidine and saw inhibition, then tried to inhibit the inhibition with yohimbine (Clark et al., 1985a). As the date of Julian's return approached, Erla and I contemplated how to tell him about this study. We decided that I would go in with the rationale and design and, after getting his approval for the study, would call Erla in to present the results. (I had convinced Erla that I would not fail so there was no Plan B.) Together we decided to proceed to characterize the sexual effects of yohimbine (e.g., Clark et al., 1984, 1985b), firmly establishing the Stanford lab in sex and drugs.

I then left to do a postdoc with Satya (and Pushpa) Kalra in Florida, where my first task was to quantify feeding behavior. Early on, Sati and I had a disagreement, and I made the mistake of informing him how Julian would have responded, and he said, "I am no Julian Davidson." Somehow I needed him to say that and we developed a true friendship which persists. As I neared the end of my postdoc and was job hunting, I conferred with Julian. His comment about big name versus smaller, less prestigious institutions was "Would you rather be the tail of a donkey or the hair of a lion?" To this day I am unsure of what this advice meant.

Julian was the most intellectually open person I have ever talked to. In response to a question on whether he was concerned that someone might "steal" his ideas, he said that it was OK, as long as they do the experiment right it is one less that we have to do. Julian was also limpid as a writer and speaker. (When I got upset at him for calling my writing limpid, he responded by saying, "Before you get upset laddie, look it up." I later upset John Bancroft by referring to his "limpid" review). One anecdote exemplifies Julian's ability to be limpid. He was presenting one of the last papers at a conference that was running quite over schedule and was asked to be as brief as he could be. Julian simply went to the microphone and said "Castrated rats don't fuck" and left the stage. (I cannot verify whether or not the story is true, but it could be.)

I think of Julian often and miss him more than I can express in words. But there is not a void. He continues to inspire me. I remember one conversation wherein he said, "Only do science because you are having fun. When it stops being fun, find something else to do." Are we having fun yet? Keep it up.

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Serious and transcendental

I had the pleasure to know several Julian Davidsons. All were charming. All were interesting. All loved science and shared that passion with colleagues and students. Two are particularly memorable.

One Julian was a serious and disciplined pioneer in the area of neuroendocrinology. Serious Julian maintained a carrel in the library where he spent each morning studying the latest books and journals. Serious Julian completed ambitious studies of human and animal behavior. Many of his studies are classics, conducted with a rigor that few have been able to match. Serious Julian expected others to share his high standards and work ethic. Serious Julian was a full professor at Stanford.

Transcendental Julian shared many traits with Serious Julian, but he seemed less comfortable in academic regalia. Transcendental Julian had a different and even more ambitious set of goals. Transcendental Julian was in search of consciousness. Transcendental Julian respected his Jewish ancestry and dabbled in Eastern mysticism. Transcendental Julian sought the intersection between biology and the se-

crets of the soul. I believe it was Transcendental Julian who faced Alzheimer's disease with courage and even humor. This delightful and complex man and all of his facets will be missed.

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Friend and master

In 1976, Luciano Martini, Head of the Institute of Endocrinology in Milan University, then the mecca of European neuroendocrinology, organized an international symposium on Androgens and Antiandrogens. Martini's meetings are legendary for his great ability for gathering the investigators in the cutting edge of the topic they addressed. I was at the time in Martini's institute in my first postdoc leave from Spain and didn't miss that opportunity for listening to the European and American "first raters" in the field. Most of the presentations kept the rather formal style that was in order in European academe in those days. So it was refreshing when this casually dressed Californian with a Scottish accent gave his talk in a nonchalant, subtly ironical style that drove into despair the otherwise excellent simultaneous translator. I was most impressed, however, by the scientific content of his presentation. For the first time I was seeing sexual behavior described as an androgen-dependent variable nearly as quantitatively reliable as the hormone radioimmunoassay techniques I was learning at Luciano's lab. The speaker was Julian Davidson. It was from his presentation that I decided to include the assessment of sexual behavior as a key variable, together with serum hormone levels, in the neuroendocrine studies I would undertake back in my home lab. So I did, and that was my launching into the field of sexual behavior. I also decided that I should visit Davidson's lab sometime.

In 1982, when planning a sabbatical leave from La Laguna University (ULL, Tenerife, Spain) I wrote to Julian. I was amazed and puzzled when I got a letter from his secretary saying that Dr. Davidson was off in the African bush for some time. (Later I would learn that this was his first field trip to study hyenas.) After Julian's return to Stanford, we agreed that I would come to his lab throughout 1983 (actually I stayed for another year) under a Fogarty (NIH) Fellowship.

In a January 1983 night I arrived at San Francisco airport with my wife and 2-year-old daughter. Julian had long been waiting for us and became from that moment the kindest host. We were touched that he and Ann had arranged for a crib for little Maria in the motel apartment he had reserved for us and that he tenderly handed us a supermarket bag with breakfast stuff. The next day he drove us to his home for lunch and a relaxed Sunday afternoon. There we met Ann, that extraordinary woman, and their kids, a family I would come to love dearly. Since the very onset of my relationship with Julian we found lots of common interests and viewpoints on a wide range of issues, from science to philosophy, politics, history, leisure, and fun. Our many fascinating discussions over the years were always spiced with Julian's wit.

By that time basic research studies in Julian's lab had moved deep into the role of the monoamine neurotransmitters in sexual behavior, which translated into a fair number of influential papers. Ironically, Julian used to say that in his early scientific years he had made a "solemn oath on his ancestors' bones of never going into the biogenic amines field." The main approach available at the time involved treating animals with drugs thought to influence the synthesis or release of some transmitter or the activation of one (or several) of its receptor(s). As the number of published receptors and their various subtypes kept rising to the present endless list, we both felt increasingly uneasy with making physiological inferences from pharmacological data of difficult interpretation. We agreed on the need to learn what was actually going on in the body, that is, to study the changes in the endogenous transmitters associated with sexual behavior.

It was from that realization that I started setting up a neurochemistry lab when I returned to La Laguna in 1985. Julian and I got an international cooperation grant that allowed reciprocal visits for several years. During Julian's stay in Tenerife in 1987, we submitted our joint paper on changes in the content of the main monoamines and their metabolites in relevant brain and spinal cord areas of copulating male rats (Mas et al., 1987). This pioneer study was followed by others from our and other laboratories based on such methodology. The next challenge, however, was doing it in living animals. That brought me into the newborn field of in vivo neurochemistry (using voltammetry or microdialysis). During his visits to La Laguna, Julian encouraged us to pursue this approach. Indeed, he was the enthusiastic witness of some of our initial voltammetric recordings in mating rats. They eventually translated into the first report describing changes in the release and metabolism of an endogenous transmitter (dopamine) in the brain of copulating animals (Mas et al., 1990), an approach increasingly used since to clarify the neurotransmitter dynamics associated with sexual functioning.

During my stay in Stanford I became increasingly interested in the sexual physiology studies on men and women that were underway in Julian's lab, something unthinkable to carry out in my country at that time. With Julian's encouragement, I planned to start this type of research at my home university. Sometimes Julian would introduce me amusingly as "the future Spanish Masters and Johnson." Julian's input contributed much to our present ULL Center for Sexological Studies (CESEX), a multidisciplinary team conducting a variety of clinical and physiological studies on human sexuality. During his visits to La Laguna Julian also

made a great impression on many of my colleagues, which translated into our faculty's unanimity in conferring upon him a doctorate *honoris causa.* Indeed his loss has been deeply regretted here.

I hope that these few recollections testify to Julian's great influence on my scientific career and more indirectly on several other members of my university and colleagues and students throughout Spain. I treasure Julian's memory as one of the greatest persons and scientists I have had the fortune to know.

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Lab as family

There are many warm, happy, silly, and some sad memories of the laboratory of Julian Davidson and Erla Smith. The Hollywood or boob tube version of a research laboratory, the few times anyone might actually see one portrayed, usually includes people in their white coats, looking very serious and seriously doing science. It all looks thoroughly professional and thoroughly boring. Yet, all of us who have worked as graduate students, postdoctoral fellows, visiting scholars, and scientists know that scientific laboratories aren't really like that and that each one has unique characteristics. The personality, work ethic, and play ethic (or lack of it) of a laboratory and the people in it can make or break a work experience. We think everyone who passed through Julian's and Erla's laboratory came to appreciate the rich scientific and warm family atmosphere. The laboratory sprawled in one of the original sandstone buildings on the Stanford campus, built in the late 1800s, complete with a rotunda that echoed when you hooted. Behind the building was the university museum. Within the building was one of the best places many of us ever worked in our scientific careers.

Bearded, bespectacled, and a bit bowlegged, Julian sported a portent of fun by the devilish spark in his eyes. This was as much the essence of Julian as his prodigious work ethic. As postdocs, we were made to feel part of the pack, and as time went on, we saw the lab family that Julian built. Each Friday we would have what we fondly called "lab lunch," with everyone—including Julian—taking turns bringing the meal. We would eat as a family and discuss the work of the week or a journal article, like a family exchanging ideas, including differing opinions and an occasional shouting match. As postdocs, lab lunch helped to integrate us into the scientific community by sharing ideas with leaders in the field who were there for the day or a sabbatical—people like Ben Sachs, Koos Slob, and Ray Rosen.

Outside of work, Julian and his wife Ann included us in many gatherings at their home. We felt Julian cared about us; we weren't just postdocs passing through the lab who would be slave labor for a few years and leave. His generosity as a person, honesty and integrity as a researcher, and cutting-edge ability as a scientist made Julian a wonderful model and mentor, and a lasting influence on our professional lives.

Of course, doing sex research was an interesting job, and Julian's approach could be both serious and absurd. Once I (L.M.) questioned the accuracy of the reported placement of electrodes on the penis for a manuscript we were working on with David Rowland. Julian dragged me off to the other end of the building that housed the anatomy department and asked John Dolph to assist us in our scientific quest. He presented a torso for our examination, complete with genitalia, and we had quite a debate about how many millimeters were really at stake. Anything in the interest of science!!

Julian's enthusiasm for research was evident in big and small ways. One morning I (D.M.) was jogging before work when out of the blue Julian was suddenly jogging beside me, excitingly telling me he had just found a medical doctor to conduct the needed physical exams for one of our studies. Julian had spied me as he and Ann were driving along Palo Alto's Stanford Avenue, which paralleled the jogging path. Julian jumped out of the car and joined me in his street clothes to share the exciting news and a short jog.

We were both in Julian's lab in the late 1980s. A reminiscence of working with him then—when his Alzheimer's disease was yet to be diagnosed—would not be complete, or honest, unless we acknowledged what he then called his memory problems: first, forgetting or double-booking appointments; later, editing manuscripts with words the opposite of what he intended. But as one observer remarked, these early changes did not signal pathology because his behavior was still, well, so Julian-like.

The Loma Prieta earthquake of 1989 damaged the old physiology building and Julian's laboratory was moved across the street. At different times following his early retirement, we each visited the now-empty building and were saddened to note the end of an era, in both a personal and a professional way. Julian and Erla were the last of their ilk in a department that changed its focus to molecular and cellular physiology. They were the last in the department who not only conducted whole animal research but also studied the human animal. We hope that their legacy will spur others to be adventurous in their studies and to continue to make the business of science a family affair.

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Understanding human sexuality: integrating biology and behavior

In some ways our story is probably similar to that of others who knew Julian. Like them, we came to appreciate his various dimensions—a vibrant mind, a charming wit, an ingratiating spirit, a kind and humanistic heart, and sometimes a man of eccentric character. In other respects, our story has its own twist. Although neither of us studied directly under Julian, much of our collaborative research today is a legacy of a meeting we had at Julian's lab in 1985 in the Physiology II building at Stanford University. This collaborative research program emerged from ideas first hatched in conversation with Julian and later developed and scribbled on placemats at an outdoor café across from the conference center at the University of Minnesota during the 1987 meeting of the International Academy of Sex Research.

As researchers trained in behavioral endocrinology, we were each drawn to Julian's lab to learn about issues of human sexuality, including the details of studying sexual response in a laboratory setting. Prior to that time, we had individually come to know and respect Julian's work, as his seminal papers were often cited in papers based on our own research studies on reproductive behavior in animals. In the early 1980s, Julian was one of the first students of the reproductive biology and behavior of animals who was bucking academic tradition and moving his research program in the direction of greater applicability. As neophytes to the study of human sexuality, the two of us looked to Julian as one who could mentor us in the laboratory study of human sexual response. Equally important, one of us (D.R.) searched for validation that the cool perspective of a dedicated rodent/animal scientist could find respectability in the study of a potentially prurient topic involving erotica, strain gauges, sexual arousal, and secretions.

Indeed it was Julian's grounding in behavioral endocrinology that helped lend new perspective to the study of human sexual response. Before the 1980s, there had been limited dialogue and integration between the biological/biomedical sciences and the behavioral/social sciences in the study of human sexuality. Julian's understanding of the role of biology in behavior and his insistence on precise definitions and objectivity in measurement helped bring new rigor to this field of study which, at least since Masters and Johnson, had evolved only minimally. Yet, we witnessed (and, as fellow behavioral endocrinologists, shared) Julian's struggle to attribute functional significance to these sometimes overly precise, objective laboratory measurements. Unlike mounts, intromissions, and postejaculatory intervals—easily quantified and interpreted measures of rodent sexual behavior—the interpretation of such outcomes as a fourfold increase in penile sensory threshold or a 10-mm decrease in penile circumference on broad constructs such as sexual satisfaction in men sometimes defied obvious relevance. Nevertheless, we admired Julian's attempts to develop biologically inclusive models of human sexual response and grapple with topics many of us simply avoided (e.g., Davidson, 1980).

But we suspect it was as much Julian's *scientific stature* as his *biological* perspective that added to the legitimacy of laboratory sex research and helped bridge the gulf between the biological and social science schools of thought. Already well respected within circles of reproductive biology and behavior, Julian's entry into the field of human sexuality strengthened its credibility and opened the way for other students of animal behavior (including each of us) to venture in.

But perhaps the best asset Julian brought to the study of human sexuality was his unassuming personality. His self-effacing manner, his desire to learn from others as much as to teach them, and his respect for the people and ideas of sexology helped build trust between disciplines. Most would agree that Julian's person was as attractive as his ideas, and we believe both played a role in fostering the dialogue between social and biological scientists.

In following Julian's footsteps—one as a visiting scientist on sabbatical and the other as visitor in Julian's lab—each of us developed research agendas focusing on human sexual response and dysfunction, agendas which eventually intersected and later partially converged. The outgrowth of that initial meeting at Stanford spawned a 15-year collaboration, many research papers and chapters, and a warm, leisurely friendship that continues with a research agenda.

Although we were never his students, it was Julian's willingness to open his mind, heart, and home (thanks also to Ann) that counts us among his devotees. Both of us, with little to offer in return, imposed upon him for support, guidance, and time—and we were heartily welcomed. We were the beneficiaries of his benevolence, his dedication as a teacher, and his willingness to take risks on other people without expectation of anything in return. We were supported by the environment he created, growing professionally as scientists and learning about the pitfalls, rewards, and politics of sex research. And at the same time we enjoyed the experience. We communed as a family under Julian's tutelage at leisurely Friday lab lunches, we were amused when he rode his bicycle through the hallways of Physiology II, and we overlooked his forgetfulness, assuming it served some greater purpose by allowing him to focus on the immediate problem and ignore the annoying trivia of life.

The success of a scientist can be measured through all sorts of indices, and Julian has undoubtedly been successful by any of these. Less measurable but equally valuable is the impact Julian has had on science through his personal commitment to those involved in research endeavors, from office staff to students to colleagues. Ultimately Julian reminded us that good modeling and mentor-

ing are critical ingredients for the propagation of good science.

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A scientist for all seasons

Sexual behavior is complex and multifaceted, involving significant physiologic, psychologic, and interpersonal dimensions. Cross-species differences, gender determinants, and contextual effects are further complications. Societal and ethical constraints limit the content and methodology of sexual behavior research, and institutional funding is invariably scarce. Clearly, this is not an area of research for the faint of heart or narrow of vision. Few scholars in recent memory have embraced the intellectual challenges and personal demands of sexual behavior research with as much commitment and dedication as Julian Davidson. Beyond his unique scientific contributions, which are impressive by any standard, Julian was an ideal role model and mentor for a generation of younger colleagues and researchers in the field. In this respect, his legacy extends far beyond the corpus of his own specific contributions and is alive today in the ongoing work of a large cadre of friends, colleagues, and former students, many of whom have contributed to this collection. We all owe much to Julian, not only for his scientific insights and critical research but also for the personal qualities he brought to the task. For our many long and stimulating discussions over the years, I will always be deeply indebted to him.

The role of hormones in sexual behavior was an early and abiding topic of interest for Julian. He began his research in this important area in the early 1960s, at a time when few theoretical concepts or experimental models were available. His early, well-controlled studies in collaboration on hormone–behavior relationships became landmarks and set the stage for much of the theory-building and experimentation in the following decades. Julian was among the first behavioral endocrinologists to demonstrate the specific roles of estrogen and androgen on sexual behavior in different species, and he conducted more innovative and influential studies on this topic than anyone prior or since. Current concepts of androgen as the "libido hormone" in both men and women, as well as our understanding of interactions between central and peripheral effects of sex steroid hormones on sexual behavior, owe much to Julian's pioneering work. The experimental models developed in his laboratory were highly imaginative and unique, providing a foundation for much of the subsequent experimentation in the field. The research was always meticulously conducted, with one study leading to another in clear, logical progression. And Julian's presentation of his findings was always balanced, thoughtful, and self-effacing.

Not content with his exploration of hormones and sexual behavior in animal models, Julian and his associates went on to tackle these difficult questions in human males and females. New experimental models and theories were urgently needed, and Julian rose enthusiastically to the task. Beginning in the early 1980s, his research group conducted a series of elegant and highly original studies of sex steroid effects in various patient groups, as well as in normal men and women. Again, these studies set the stage for much of the subsequent research on the topic. His group was the first to evaluate hormone–sexual behavior effects in humans across both laboratory-based and naturalistic settings. Julian's efforts to integrate findings from these studies into a broader conceptual model of hormonal and nonhormonal effects on sexual behavior, and his ongoing attempts to develop clinical applications of the work, are still influential. The recent upsurge of clinical interest and applied research on hormone replacement therapy for sexual arousal and desire disorders in women can be traced, in part, to the key findings and concepts that came out his laboratory in the 1980s.

The neuropharmacology of sexual behavior is a related area of research to which Julian and his associates made early and lasting contributions. Again, the group began by testing basic hypotheses and concepts in well-designed animal models. Their experimental studies of serotonergic and adrenergic mechanisms, in particular, established the role of specific neurotransmitters and pathways, as well as the potential for pharmacological manipulation of sexual behavior in both animal and human models. In fact, the first experimental evidence for prosexual effects of an oral agent (yohimbine) came out of these early studies. In this respect, his work foreshadowed the current explosion of research on PDE-5s and other proerectile agents. Julian's group was also among the first to demonstrate specific inihibitory effects of serotonergic drugs, anticipating the current interest in adverse effects of serotonin uptake inhibitors by almost two decades. Other studies from this period were highly influential in showing excitatory effects of dopaminergic agents, an area of increasing research and clinical interest today. As with his work on hormones and sexual behavior, Julian contributed as avidly to theory building and hypothesis generation in this area as he did to data collection and publication of critical research findings. The burgeoning field of sexual pharmacology owes much to the theoretical and experimental contributions of his group. Again, our frequent personal contacts and collaboration did much to direct my own work in this area.

As important and influential as his contributions to research and theory was Julian's dedication to training and mentorship. For many years, his laboratory provided a

unique training opportunity and intellectual foundation for graduate students and postdoctoral trainees with a special interest in endocrine or neuropharmacological aspects of sexual behavior. In recognition of the excellence of his contributions in this area, Julian received a series of prestigious NIH training grants in the 1970s and 1980s. Many distinguished contributors in the area of sexual behavior research, some of whom are represented in this collection, received their critical research training in Julian's laboratory. The list is long and impressive and provides further testament to the enduring nature of his contributions.

Highly productive scientists and scholars are all-too-often narrowly focused individuals in other areas. Nothing could be further from the truth in Julian's life. His interests were always broad-ranging and his passions deep. He loved music, song, and literature, and above all, people and life. He was well-rounded and complete in all the ways that matter. I have known few scientists who were more committed fathers and husbands, and a better friend would be hard to find. A sweeter smile is hard to imagine.

We miss you dearly, Julian!

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