



INTERNATIONAL SOCIETY FOR NEUROETHOLOGY

Newsletter
March 2000

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Changes in the MEMBERSHIP DIRECTORY

In the past, ISN would compile and print copies of the membership directory that was sent to all society members. However, this has proven to be very expensive, both for printing and mailing. Moreover, since the directories were produced only every several years, these quickly became out of date. Instead, members have come to rely far more on the web-based membership directory that can be found on the ISN Website <http://www.neurobio.arizona.edu/isn/>. After consideration by the Executive Committee, and unanimous approval by the ISN Council, we have decided to suspend the printed directory. We do realize, however, that some members do not have easy access to the web, and we have made provisions for these members to request copies of the directory. These will be sent as text or word processor files by e-mail, or as PDF files. If you need this directory, please contact our business office at ISN@panassoc.com. You should also contact our business office if you have a change in your directory information so that this can be entered onto our web-based directory as soon as possible.

NEUROETHOLOGY LISTSERV

Reminder: The ISN maintains a Listserv as a benefit of membership. Any member in good standing may join the Listserv and use it to broadcast announcements, requests for information or materials needed for

research, etc. Members who have joined the listserv receive all notices posted to it, including meeting announcements, advertisements of job openings and postdoc positions, fellowships, etc. To join the listserv or update your e-mail address for its messages, please send e-mail to John Hildebrand, Past President of the ISN, at <jgh@neurobio.arizona.edu>.

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1999 ISN ANNUAL FINANCIAL REPORT

Prepared by Sheryl Coombs, Treasurer

January 25, 2000

Balance as of 12/31/98		\$214,385.60
Revenues in 1999		\$32,597.61
Investment Portfolio Growth*	\$5,932.03	
Bank Interest:	\$877.58	
Membership Dues:	\$25,738.00	
Donations:	\$50.00	
Conference	\$0.00	
Other	\$0.00	
Debits in 1999:		(\$18,693.20)
Operating Expenses	(\$18,693.20)	
Conference Expenses	\$0.00	
New Balance as of 12/31/99		\$228,290.01
Total Assets -- Liabilities		\$228,290.01

* Growth in Market Value as of 12/31/99

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AUTOBIOGRAPHICAL SKETCH

Editors note: Several months ago I asked Ed Kravitz to write an autobiographical sketch for this Newsletter. Ed was most agreeable, but when he submitted the piece it had grown to included not only Ed's autobiography, but a wonderful historical lab report for one of the groups that serves as a cornerstone of our field. Thus, I decided that this dual autobiographical sketch-lab report deserves extra space since it fits two niches. I hope other ISN members will agree that this is a fascinating history that is most appropriate for the pages of the *The New Yorker*.

Neurobiology in the 60s or "the good old days are now" By Ed Kravitz

Origins: It was dumb luck that I was invited to join Steve Kuffler, Dave Potter, Ed Furshpan, Dave Hubel and Torsten Wiesel shortly after they moved as a group from Johns Hopkins University to form the Neurophysiology Laboratory in the Department of Pharmacology at Harvard Medical School. I met Steve in the Fall of 1959 when he came to NIH searching for a biochemist to fulfill his vision of combining neurophysiology with biochemistry and neuroanatomy in order to really understand the nervous system. Steve wanted a biochemist to help identify the inhibitory transmitter compound at crustacean neuromuscular junctions.

Steve originally had contacted Roy Vagelos, a close friend of mine, who at the time was running the Stadtman laboratory at the National Cancer Institute. Roy said he wasn't interested in working in the nervous system, but that a post-doctoral fellow in the laboratory (me) might be, since I regularly presented journal club talks on neurochemical topics. In fact I had a long-standing interest in brain function dating back to my graduate school days and arguments with philosophers and psychologists about whether we could ever really understand the brain.

My plan for a transition from biochemistry to neurochemistry was to complete a postdoctoral year at NIH, followed

by further postdoctoral studies with Oliver Lowry to learn his micro-methods for studying the chemistry of single nerve cells, and with David Nachmansohn to learn about nerve conduction and transmission. Nachmansohn did not believe in chemical transmission and had developed an elaborate scheme for how conduction and transmission worked. His model involved acetylcholine release, receptor-binding, hydrolysis and resynthesis, all taking place within the membranes of conducting cells. This cycle then supposedly triggered the entry of the Na^+ that generated action potentials *and* synaptic potentials. My biochemical colleagues warned me that physiologists had it in for Nachmansohn because he had shown that their chemical theories of transmission were wrong. Luckily for me, I did not follow my original plan. After the meeting with Steve, a trip to Boston in a blizzard, a snow ball fight in Harvard Yard, and another meeting in Boston with Steve and the other members of the group, I was convinced that a move to Harvard Medical School was the right thing for me to do. It's a decision I've never regretted.

A decade at HMS (1960-1970): So much happened during the ten years at Harvard Medical School that began with us as members of the Laboratory of Neurophysiology (1960-1966), and ended with us as the core of what may have been the first Neurobiology Department in the world (1966-on), that it's difficult to know where to begin. On a personal level, I came to HMS as a Research Fellow in 1960, and within nine years was a full Professor. At the time, it was unheard of that any single department at HMS should have more than two full professors. Yet with Steve's amazing political skills, a supportive Dean of the Medical School (Bob Ebert), and our strong research and teaching activities, six tenured full professors made up the new Department of Neurobiology within a few years of its formation.

It was the decade during which Hubel and Wiesel were doing the experiments that would earn them the Nobel Prize in Physiology. It also was early in the days of using computers for physiological investigations, and I distinctly remember Dave and Torsten saying that they mostly used the computer to keep their laboratory warm. I remember watching them one time plot the receptive fields of cells using their slide projector with its adjustable slit that they manually swung back and forth across a large sheet of paper they marked with a pencil, while the computer hummed softly in the background warming the room.

For me, it was the time of the demonstration that GABA is an inhibitory transmitter compound, a body of work carried out despite the proclamations of two international congresses (held in 1960 and 1961) that GABA was not a transmitter compound in invertebrate or vertebrate nervous systems (Ernst Florey and Jack Eccles led the defense of those positions). Before I joined the laboratory, Steve, Dave Potter and other colleagues had shown that GABA was present in the central and peripheral nervous systems of lobsters. GABA also had been shown to have a high physiological specific activity, but other inhibitory compounds were present in the tissue extracts and it was not known whether any of these was uniquely associated with inhibitory neurons. Spurred on by Florey's ability to dissect single axons from crustacean nerve bundles, Steve, Dave Potter and I dissected meter lengths of single inhibitory and excitatory axons from lobster walking leg nerves, and with extracts of these nerves showed that GABA alone of the inhibitory substances was asymmetrically distributed, while glutamate, the principal excitatory compound, was present in both excitatory and inhibitory axons. Masanori Otsuka (on sabbatical from Tokyo Medical and Dental University) joined us shortly after the completion of these experiments and his studies generated the first detailed charts of the positions of identified neurons in an invertebrate central ganglion in which physiological identification, cell body location and single cell biochemistry were combined to construct the maps. When Masanori presented these results to a packed meeting room at a FASEB meeting (before the days of the Society for Neuroscience) five minutes of applause followed his talk, something I never had heard before.

The crucial release experiment that demonstrated the transmitter role of GABA was completed while my three colleagues literally were up in the air. Masanori Otsuka was on his way back to Japan, Les Iversen (a post-doctoral fellow sent to us by Julius Axelrod and Arnold Burgen) was on his way to England and Zach Hall (my first graduate student) was on his way to California. The four of us began this experiment together, but the three others had to leave for the airport during the experiment, leaving me to do the final analysis. Fortunately for all of us, the experiment worked. There was no Email in those days, so Usnail mail and phone calls were used to tell everyone the results and that we now had in hand the final crucial piece of evidence that GABA was a transmitter compound. During the next few years, additional experiments from our laboratory suggested an explanation for the selective accumulation of GABA in inhibitory neurons [with Deric Bownds (a post-doctoral fellow), Perry Molinoff (a medical student) and Zach Hall], and demonstrated the existence of a highly-specific GABA uptake system for GABA and identified the site of uptake (with Les Iversen and Paula Orkand, another post-doctoral fellow).

Towards the second half of the decade, Tony Stretton (a post-doctoral fellow sent to us by Sydney Brenner) and I began our studies with the dye Procion Yellow. Tony and I were interested in whether identified cells in lobster ganglia always had the same geometrical shape. This interest arose from Tony's background in molecular genetics, the two of us starting to ask questions like "was the shape of neurons genetically specified" and the Otsuka maps showing that the cell bodies of identified neurons were pretty much in the same positions from ganglion to ganglion.

and from animal to animal. Our biochemical colleagues wondered how two good biochemists could be wasting their time on such a mundane anatomical problem. At the time, Ed Furshpan and Jaime Alvarez had accumulated an extensive collection of dyes in their search for a substance they could electrophoretically eject from microelectrodes into Mauthner cells. Among Ed and Jaime's collection was a Procion dye, and this worked best of all the substances we tested. Still their dye did not fully stain the neuropil processes of the neurons we injected. A visit to Imperial Chemicals in Providence, R.I. provided us with 120 Procion-related dyes, all of which were tested (with wonderful technical help from Edith Maier). Only Procion Yellow of the 120 dyes showed the features we required (highly soluble, readily releasable from microelectrodes, completely filled cells and their processes, survived fixation and dehydration, was fluorescent and was easily seen in tissue sections).

I vividly remember Edith completing the first reconstructions of a pair of identical cells from different animals, while Tony and I hovered over her shoulder. As each data point from the photographs of serial sections was hand-drawn onto the reconstructions it became clearer and clearer that the two cells had close to the same morphological shape in the two animals. In great excitement Tony and I ran down the hallway telling everyone the results. Our ardor was cooled, however, by the responses we received, ranging from "So what?" to "What did you expect?" After all, Purkinje cells all have pretty much the same shape too. At first, only Hubel and Wiesel recognized the potential of the method, and within days they were attempting to fill vertebrate CNS neurons with the dye. Procion Yellow had a short lifetime, being replaced within a few years by the much more fluorescent and easier to obtain Lucifer Yellow, but Tony and I had the joy of developing a technology that we knew would allow investigators to unravel the morphology of complex synaptic regions, a task that Bullock and Horridge had declared to be impossible just a few years earlier in their monumental work "Structure and function in the nervous system of invertebrates."

Life in Neurophysiology and Neurobiology: There were many facets to our lives at HMS during the 60s. Science was first and foremost, but there was much more. Steve was Dad to his "boys" (the academic world of the 60s was very much a male-dominated world—it still is today, but fortunately things are getting better) and Thanksgiving dinners with him, Phyllis and the Kuffler kids (Susy, Damien, Genie, and Julian), and regular Sunday morning phone calls were part of the routine of our lives. Steve never returned from a trip without greetings for each of us from colleagues. He was a notorious punster, and at one time was restricted to one pun a day (a rule he regularly broke). Probably the most chaotic time of the year though, was the end of November when the design for the annual Christmas card had to be created. All work stopped as we brain stormed the topical theme for the year, after which all activities in and around the photography lab stopped while photos were taken of everyone in the department, the card was constructed, photographed and printed, addressed to colleagues all over the world, and sent out.

Food: The legendary Christmas parties began with a "social hour" and party games, and continued with a huge sit down meal cooked by Theresa (our lab assistant for many years) and her family in the jam-packed lunchroom. After dinner, there was the "suit joke" and the student skit satirizing the faculty. Finally the tables and chairs were removed from the lunchroom and the dancing started. Lab spring picnics and communal meals at Woods Hole in the summers complemented the "eating scene," as food, somehow or other, seemed to be a central theme in our lives. Steve was a visible and active presence at all these events, and almost all of our children were tumbled upside down over his shoulder at least a few times over the years. Once a month "evening meetings" were held at which each lab group took their turn preparing dinner for the department and presenting their latest experiments in detail. While these ended up as long evenings, it was an important way in which we kept abreast of what was happening in an ever-growing department. Almost daily seminars were held over lunch (see below) and the week concluded with a departmental beer hour (with elaborate snacks) on Friday afternoons, that started as a sherry hour.

Much more serious, non-academic events of great magnitude filled our lives during that era too, including a war we opposed, blatant racism in our universities, and the assassinations of Jack and Robert Kennedy and Martin Luther King, Jr. These things weighed heavily on us, raising our social consciousness, and dominating our existence for periods of time during the decade. Ed Furshpan, Dave Potter, Torsten Wiesel and I formed the core of the group that established a program to bring substantial numbers of minority students to HMS. In the 30 years of existence of that program over 700 minority doctors have graduated from HMS (compared to about 25 in the previous 30 years). My office was one of the Harvard Medical School Strike Centers that were contacted by students from Kent State University after the Ohio National Guard fired at and killed four unarmed students on the campus, and I was the central organizer of a Teach-In at HMS on the legality of the Vietnam War when classes were officially suspended at the medical school for the first time in its history.

Two other parts of life in the first decade of the department are worthy of special mention. These are first, the lunchtime seminars, which represent an important way we learned to appreciate the magnitude of our rapidly growing field, and second, a commitment to outstanding teaching, that was a cornerstone of our activities from the beginning.

The Lunchtime Seminars: I don't remember when the scheduling of talks at lunchtime began. When we arrived at HMS, all medical school departmental seminars were held at 4PM, usually with tea beforehand. Our seminars probably grew out of the elaborate, highly ritualized lunches we ate together in the pharmacology department lunchroom (much to the amusement of the rest of pharmacology). I suspect they started by us first asking guests to join in the repast, then asking them to tell us what they were doing. The logic of having seminars at lunchtime was, "Well you have to eat lunch anyhow, and we all eat together, so why not listen to talks at the same time." Sometimes, for days on end, we had lunchtime seminars. No notices were sent out announcing these seminars, and only rarely were they formally scheduled in advance. Instead they were written on a calendar hanging on the lunchroom door, which therefore had to be checked daily to see whether there was a talk that day. Steve's wide circle of friends regarded a stop in Boston as an essential part of any trip. As each of us became more prominent in our respective fields, we too had regular visitors. Essentially all visitors were asked to tell us about their latest experiments over lunch. At first this caught visitors by surprise. Pleading that they had not brought slides, we said, "It's OK, just go to the board and tell us what you're doing—it's really very informal." On second visits though, friends showed up with sets of slides in their pockets and talks prepared, just in case.

The entire department turned out for seminars, cramming into the small lunchroom that was the hub of so many departmental activities. Great scurrying around preparing lunches preceded the talks, which started around 12:15 (the origin of the 12:15 start-time of the much more formal departmental seminars today). The seminar speakers were introduced by their hosts and then the trial began. Speakers were lucky to show one or two slides (if they had brought slides) or to get through the introduction to their presentation, before the questions started flying. At times it seemed as if every detail of every slide was being questioned, which had to be frustrating for the speakers, but was exciting for us. There was a shared overwhelming desire to really know and understand what was being done, why it was being done, and whether the results supported the conclusions. I don't believe it was arrogance on our part, although I suspect it bordered on rudeness. The discussions could go on for hours, until we, or the visitors, exhausted by the ordeal, called for closure. On one visit to the department, Paul Greengard, who had a biochemistry seminar scheduled for 4 in the afternoon, was asked to deliver a lunchtime seminar. An exhausted Paul barely finished the session when it was time for him to deliver his biochemistry seminar (which we all attended, of course).

More often than not the seminars were the highlights of our days and they were exhilarating. It's the way we learned about the breadth of our newly emerging field. We were treated to Bernard Katz delivering a three hour Saturday morning discourse on synaptic transmission in this way, and visited and lectured to by many many past, present and soon-to-be giants of the early days of neurobiology (Seymour Benzer, Sydney Brenner, Ted Bullock, Jose del Castillo, Francis Crick, Jack Eccles, W. Feldberg, TP Feng, Norm Geschwind, Paul Greengard, S. (Hagi) Hagiwara, Eric Kandel, Vernon Mountcastle, Walle Nauta, Rami Rahamimoff, Miriam Salpeter, Gordon Shepherd, Ladislav Tauc, Pat Wall, and Victor Whitaker were just a few of our very large pool of regular visitors).

Teaching: Dating to our earliest days at HMS, and under the leadership of Ed Furshpan and Dave Potter, our department has had a serious, dedicated commitment to outstanding instruction. The Neurobiology block of the medical school curriculum consistently received rave reviews from medical students. On occasion this has led to notice by the greater medical community as well (we were visited by the president of the American Academy of Neurology in the late 1960s, who wondered why so many young doctors from HMS were turning towards Neurology). In the early years, Ed and Dave headed off to Woods Hole several weeks before the scheduled start of the neuro-block of teaching for medical students (Area III in those days), to prepare their lectures. The lectures were not memorized, but instead were an elegantly crafted, carefully thought through and argued out system of presenting neurophysiology in a comprehensive and comprehensible manner, with one lecture building on the previous one, and leading logically into the next. To do this, Ed and Dave stood in front of and "rehearsed" each other, thrashing out the best ways to cover the material, and examining the existing literature to construct their set of lectures. The result was some of the clearest and best lectures ever presented at HMS, and a system of teaching and learning a difficult subject that the medical students loved.

Upon my arrival at HMS, I joined Ed and Dave at Woods Hole for these "rehearsals" and added my few "biochemistry of synaptic transmission" lectures to their elegant set of neurophysiology lectures. A few well-placed "jokes" also were added to the lectures (probably because Jack Diamond, a visiting colleague from Canada, and I joined Ed and Dave in Woods Hole), and these too built on each other and showed up in multiple lectures. Presentations by Dave Hubel and Torsten Wiesel rounded out the Area III lecture set. Steve lectured for one or two of the early years, but wasn't invited to participate in future years because his presentations were not considered clear enough (we suspected that Steve did this on purpose). The popular Kuffler and Nicholls textbook "From Neuron to Brain" was heavily based on the spectacular teaching system originally devised by Ed and Dave. On top of all of that, Ed and Dave memorized the names of the medical students from the class photos sent to us each fall,

and surprised and delighted many a medical student of that era by calling them by their first names as they walked in the door for the first class sessions.

Impact: Steve Kuffler used to say "the good old days are now." He meant that in the best sense, which was don't look back with nostalgia at what used to be. It's a philosophy I agree with, and this article, therefore, is not an attempt to offer a sentimental view of the "good old days." The first decade of Neurobiology was unique, and an exciting time for all of us. But the progress being made today in the human genome, in our understanding of how the nervous system works, and in unravelling the mysteries of neurological and neuropsychiatric disorders, dwarfs many of the accomplishments of those early days. Society too has made remarkable strides with women and minorities making up large portions of our student and post-doctoral populations and increasingly occupying prominent academic positions as well. The grant scene could be better of course, and there are serious challenges to academic excellence being promulgated by grant-dollar counting administrators that will have to be dealt with. I learned much from my colleagues and mentors of the early 60s, and throughout my career have tried to emulate Steve and run my laboratory as a "family," and Ed and Dave and maintain a dedication to teaching. Science was fun in the early 60s, and I suspect we could keep it fun with some serious attention to that aspect of academic life by all of us. Overall though, "the good old days are now" still seems to ring true to me.

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