INTERNATIONAL SOCIETY FOR NEUROETHOLOGY

Newsletter

JULY 2000

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ISN OFFICERS

6TH INTERNATIONAL CONGRESS OF NEUROETHOLOGY BONN, GERMANY

July 29th - August 3rd 2001

Planning for our next congress is now well advanced so be sure to put these dates in your diary and encourage everyone else to set aside time for what promises to be a stimulating and enjoyable meeting. Elsewhere in this newsletter you will find details of the scientific program - the invited talks and symposia - that is being put together by Hermann Wagner and his committee. The local arrangements are being coordinated by Horst Bleckmann. I am sure that both would welcome hearing from you with any further suggestions for the meeting.

Call for Suggested Venues for 7th International Congress of Neuroethology 2004

Would you like to host the 2004 Congress for the Society? We are looking for a venue that can offer good facilities for a meeting of 500-600 delegates including low-cost student accommodation, easy access for members who will come from all over the world, an interesting environment, and of course a local team willing to take on some of the organization, supported by the administration and funds of the society. The scientific program will be put together by a Program Committee that will work closely with the local organizers.

If you wish to propose a meeting venue please contact any member of the Executive Committee as soon as possible so that we know of your interest. We will then invite a selected number to present their case in our July 2001 Newsletter and at the Bonn meeting so that the members can vote on their choice. In these presentations we would like to hear about the projected costs - registration fees and accommodation - location of lecture theaters and poster halls, and what other attractions your venue has to offer. We have had some wonderful venues in the past Tokyo, Berlin, Montreal, Cambridge, La Jolla and Bonn. So, if you think your venue could follow these and provide another memorable meeting, then please let us hear what your venue could offer for 2004.

Committees

Thank you for your response to my call in a previous newsletter for people to serve on our committees that will seek to plan the future of our society. The following

committees have now been set up and have been asked to report in time for the next meeting of the society officers to be held in Chicago on September 25th 2000. I am sure that the chairs of these committees would welcome your comments. (You are also welcome to communicate your thoughts to any other committee member, and you can get their email addresses from the ARO web site.)

Science Committee: Chair: Harald Wolfharald (wolf@biologie.uni-ulm.de), Martin Giurfa, Ian Meinertzhagen, Eduardo Rosa-Molinar, Michael O'Shea
Charge: To consider whether the society should be involved in other activities such as workshops and meeting between congresses. To examine what other kinds of programs the society might sponsor.

Long Range Planning Committee: Chair: Catharine Rankin (crankin@cortex.psych.ubc.ca), Zen Faulkes, Martin Egelhaaf, Sarah Bottjer, Avis Cohen
Charges: To evaluate progress and suggest future directions of the Society. To seek input from members about their goals for the society.

Education & Outreach Committee: Chair: Ed Kravitz (edward_kravitz@hms.harvard.edu), Frederick Prete, James Murray, Gwen Jacobs
Charges: To examine how our Society might better communicate with the public and with other scientists to help them learn about our work; to examine how we might reach out to schools.

Malcolm Burrows
President, ISN

CONGRESS IN BONN 2001

Program

Since last October the program committee has worked on the program for the 2001 meeting in Bonn. First, we asked for input from the members as to possible topics for the meeting. Many of you responded and we received more good proposals than we could accommodate. To me, this overwhelming feedback showed that it was good to include everyone in the process of finding a stimulating program. I want to thank everyone who made suggestions or submitted proposals for their thoughts and work. Of course, the committee then had the difficult task of selecting the proposals that would represent the society best. Decisions have now been made and the program committee has put together a very attractive and balanced program.

The core of the scientific program are the plenary talks, the evening talks, the symposia, and the poster sessions. It was decided in San Diego to give the posters a more prominent role. Therefore there will be four poster sessions totaling some 10 hours of presentation time. This should allow for lively discussions at the posters. Mornings will start with plenary talks. One morning will be reserved for presentations of the recipients of the young investigator award. For the other four days, eight plenary speakers (presented here in alphabetical order) have agreed to talk:

1. Alexander Borst (UC Berkely, USA) "Neural Computation of Visual Motion Information in the Fly"
2. Alison Doupe (UC San Francisco, USA) " The Neural Basis of Vocal Learning in Songbirds"
3. Martin Heisenberg (Univ. Wuerzburg, Germany) "Fly Memories: What, Where and How"
4. John Hildebrand (Univ. Arizona, Tuscon, USA) "Neural Processing and Plasticity Underlying Odor-Modulated Behavior in Moths"
5. Carl Hopkins (Cornell Univ., Ithaca, USA) "Bioelectrogenesis and the Origins of Electrical Diversity: The Neuroethology of Electrical Communication"
6. Darcy Kelley (Columbia Univ., New York, USA): "Producing and Perceiving Male and Female Song: Molecules and Mechanisms in *Xenopus laevis*"
7. Yasushi Miyashita (Univ Tokyo, Japan) "Neural Mechanisms of Visual Long-term Memory in the Primate"
8. Barbara Webb (Univ. Stirling, United Kingdom): "Using Robots to Model Animals"

Evenings have a varied program including a boat tour on the river Rhine, a dinner at the posters, and two special evening lectures on general themes of neuroethology that will be especially suited for, and open to, the public. We are very happy that two well known colleagues have agreed to present these lectures:

1. Dean Hamer (NCI, Bethesda, USA): "The Role of Inheritance in Human Behavior"
2. Gerhard Roth (Univ. Bremen, Germany): "Evolution of brains and evolution of consciousness"

The committee has voted to not have more than two parallel symposia sessions. Since we have eight spots reserved, we could offer symposia to 16 colleagues. This selections was especially difficult because we received more than 40 proposals. After we asked for formal proposals, we still had more than 20. The committee did its best to come up with a balanced list. As of this moment, we have selected 15 of the 16 symposia (again presented in alphabetical order of organizer):

1. Adams, M (UC Riverside, USA) and Libersat, F (Beer Sheva, Israel): "Venom cocktails and the orchestrations of prey paralysis" (contributions by Libersat, Adams, Gurevitz, and Olivera)
2. Arikawa K (Yokohama Univ., Japan) and Stavenga, D (Univ. Groningen, Netherlands): "Visual ecology of invertebrate color vision (contributions by Stavenga,

Arikawa, Kelber, Cronin, Hempel de Iberra)

3. Bateson M (Univ. Newcastle, United Kingdom) and Healy S (Lethbridge, Canada): Avian models of complex information processing (contributions by Bateson, Gagliardo, Healy, Nicol, Rowe)
4. Coombs S and New J (Loyola Univ. Chicago, USA): "Multimodal sensory guidance of complex behaviors" (contributions by New, Braun and Coombs, Montgomery, Nelson, Uetz)
5. Covey E (Univ. Washington, Seattle, USA) and Moss C (Univ. Maryland, College Park, USA): "Auditory information processing and echolocation: from neurons to robot bats" (contributions by Park, Fuzessery, Metzner, Moss, Horiuchi)
6. Cronin T (Univ. Maryland BC, Baltimore, USA) and Hawryshin C (Univ. Victoria, Canada) "Polarization vision" (contributions by Dacke, Shashar, Marshall, Parkyn, Loew)
7. Eaton R (Univ. Colorado, Boulder, USA): "Dendritic computation in neuroethologically relevant systems" (contributions by Miller, Edwards, Eaton)
8. Ehret G (Univ. Ulm, Germany): "Auditory brain maps and their relation to sound perception" (contributions by Carr, Ehret, Feng and Liu, Gaese, Pollack and Imaizumi, Rauschecker, Schluze, Suga)
9. Giurfa M (FU Berlin, Germany) and E (Univ. Illinois, Urbana, USA): "Cognitive abilities in invertebrates" (contributions by Mizunami, Graham, Giurfa, Wilcox, Capaldi)
10. Hedwig B (Univ. Cambridge, United Kingdom) and Jürgens U (Primate Center, Goettingen, Germany): "Neural mechanisms of sound production: a comparative approach" (contributions by Hennig and Fonseca, Hedwig, Walkowiak, Margoliash, Schuller, Jürgens)
11. Katz P (Georgia State Univ., Atlanta, USA): "Modulatory signalling: conveying information is not always exciting (or inhibiting), but can be mind-altering" (contributions by Katz, Brezina, Mercer, Insel, Schneider)
12. Mesce K (Univ. Minnesota, USA) and Adamo S (Dalhousie Univ., Halifax, Canada): "Modulating the neuromodulators" (contributions by Mesce, Adamo, Edwards, Robinson and Schulz, Nusbaum)
13. Mogdans, J (Univ. Bonn, Germany): "Perception of nearfield flow and its importance for animal behavior" (contributions by Barth, Shimozawa, Yen, Mogdans, Dehnhardt)
14. Simmons PJ (Univ. Newcastle, United Kingdom): "Designs for signaling: from sense to action" (contributions by Laughlin, Birminham, Simmons, Bueschges, Mulloney)
15. H. Zakon (Univ. Texas, Austin, USA): "Timing is everything: the behavior and modulation of neural oscillators" (contributions by: Marder, Smith, Schmidt, Bal, Block)

The committee hopes very much that this program will attract many members (and non-members) to the Bonn meeting. The local organizers have especially looked for possibilities to make the meeting attractive for young people.

Hermann Wagner (chair of the program committee)

Update on Meeting Venue

The congress will take place in the main building of the University which is located right in the heart of the city. With the attractive program put together by the program committee, the pleasant river Rhine within close walking distance and many pubs and restaurants just around the corner I am certain that you will enjoy your stay in Germany. For those of you who are interested to learn more about Bonn and the latest announcements regarding the conferenc,e a web site (www.Zoologie.uni-bonn.de/ICN2001) will be available around August 1.

INTRODUCING THE ISN COLUMNIST

As you may recall, the last issue of this newsletter featured a wonderful piece by our colleague Dr. Ed Kravitz. The piece received “rave” reviews from many readers. Ed then showed your editor another piece that he had written (Ed likes to write). I shared this with the ISN officers and we unanimously agreed that the pieces Ed writes are so interesting that we would invite him to do a regular column. Ed quickly agreed (much to our pleasure) and in this issue we offer the first of what we anticipate to be a series of articles that allow Ed to explore his personal views on science, history, the future, or whatever might strike his fancy. We hope you enjoy this regular feature, and we invite you to share your thoughts on future columns, or on columns already written, with Ed or with the editor.

"FIELD" BEHAVIOR

Ed Kravitz
special columnist to the ISN newsletter
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DEATH OF THE MOMENT OF DISCOVERY

I'd never been to Bar Harbor, so despite the anticipation of a worrisome flight on a bumpy six-seater I accepted an invitation to deliver a seminar at The Mount Desert Island Biological Laboratory, mostly out of curiosity. It was, my friends told me, a place like the Marine Biological Laboratory in Woods Hole had been many years ago. I suspected they were right when I arrived at my cabin the first night to find the same weathered shingles, the same frame walls, the same efficient use of every square foot of space, the same sagging beds.

My seminar was delivered the next day in a church-like frame building to a curious, but interested, audience. A short walk after the seminar took us to a wooden frame cafeteria building for lunch. Long, oak, well aged, communal tables complemented the hot lunch served on partitioned trays. A thirtyish woman, one of the investigators at the laboratory, joined us at the table. She turned to me and said "you won't remember me, but I'll never forget you." Slightly embarrassed that I really didn't remember her (what on earth had I done?), and not immune to flattery (was it going to be flattery?), I, and everyone else at the table, waited with the proverbial baited breath for her next words. "I was in John Pappenheimer's physiology course about 10 years ago," she said. "Dave Potter was teaching us about synaptic physiology when you burst through the door, shouting 'Dave, Dave there's ten times more GABA in the inhibitor than in the excitor!'." At that prompt (I still did not recognize her), images came rushing back.

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John Pappenheimer, a long lean gentlemanly professor of Physiology at Harvard Medical School, had for many years taught a famous seminar in physiology for advanced graduate students. Some of the sessions were led by guest instructors. Dave Potter, with his excellent reputation for teaching medical students, was on that morning. In my wild charge into the room, I didn't see the students, I didn't see Pappenheimer. I saw only Dave. John, cool as ever, turned and said "now Ed, just calm down and tell us what this is all about."

It was the spring of 1963, a simpler time for those of us who made up the Neurophysiology Laboratory of the Department of Pharmacology at Harvard. The first Neurobiology Department in the country had not yet been formed (that happened three years later). Dave and I (and Steve Kuffler, our mentor) were engaged in experiments attempting to identify the chemical transmitter compound used to communicate inhibition in lobster neuromuscular preparations. At that time, two chemical transmitter compounds were known: acetylcholine and noradrenaline. Nobel prizes had been awarded for the identification of each, and no transmitter compound had been identified for over 20 years. Our leading candidate was gamma aminobutyric acid (GABA), derived from the common amino acid glutamate, which at the time was best known as a taste enhancer and as the cause of the headaches of the Chinese Restaurant Syndrome. We had little competition in our quest, because three years earlier, at two international conferences, leading scientists in the field had concluded that GABA was not a transmitter compound in any nervous systems. Our results suggested otherwise, but were ignored.

We fully understood the crucial steps involved in establishing that a substance served as a transmitter compound. We had to show that the material exactly duplicated the actions evoked by nerve stimulation, that the material was there, that it was concentrated in the right nerve cells, and that when the nerve cells were stimulated, enough of the material came out to duplicate the physiological effect. It was early in the story of establishing GABA as an inhibitory transmitter compound. Steve Kuffler and other eminent physiologists had shown that GABA acted like the normal transmitter compound, and many laboratories had shown that GABA was found in nervous systems. But was it found in the right place in inhibitory neurons? That remained to be established. Dave and I had worked out methods for dissecting single inhibitory and excitatory axons from lobster leg nerves and had developed a method sufficiently sensitive to measure the levels of GABA in single axons. The procedure was baroque in comparison with the way we would do these experiments today and we knew that it would take us many hours to complete the first experiment. The experiment began the morning of the first day, continued throughout that day and into the evening, and went the entire night, but the analysis still had not been completed by the time Dave had to leave to teach his class. A reluctant and very tired Dave Potter went off to do his duty, leaving me in the last stages of the analysis.

My hands were shaking and I remember the excitement as I read the fluorescence emitted by standard amounts of GABA, and then by each sample. The first single axon

readings told me that GABA was only in inhibitory axons, but how much was there, and how great was the difference between the two axon types? With each sample it became clearer that the concentrations of GABA were enormously high in inhibitory axons, and that these axons contained at least ten times more GABA than excitatory axons (later we pushed that difference up to 500 times). It was the first time that GABA had been localized to a single identified nerve cell. That was the information I clutched in my hand as I ran down from our third floor laboratories, across the medical school quadrangle, up two flights of stairs and burst in on the Pappenheimer seminar. Not world shaking perhaps, but exciting news to me and to those students.

It is difficult to describe the high of the moment of discovery. All I can say is, that for me, there is nothing like it. It is a high above all highs and a joy beyond all joys. One wants to run and share the news with everyone. Perhaps it's the knowledge that in a world that's mostly out of our control there is one very small fact that you have uncovered, and that you alone possess for a tiny instant of time. No Nobel prizes came from the identification of GABA as a transmitter compound: none were anticipated, desired, or needed. That's not why we did the experiments. For us, we had the thrill and the frustration of a great challenge facing us every day. Could we find out how inhibition was communicated? Could we learn how a nerve cell and a muscle fiber talked to each other in that tiny fragment of tissue dissected from an animal more commonly found on a dinner plate than in a dissecting dish? Could we convince our colleagues that we had the answer? Over the next several years, with further experiments and with excellent colleagues, we succeeded in doing just that.

In recent years, as the Director of a graduate program (1982-1990) and as a group leader and lecturer in ethics discussion groups, I've been surprised to find a reserve among graduate students about the thrill and joy of discovery. One year my frustration at this lack of enthusiasm led me to ask "how many of you have been told that you can't talk about your results with anyone until they are published?" Half the students in the room raised their hands. A young Assistant Professor explained that she had a three person laboratory, herself, a technician and a graduate student, and they were working on a hot topic in molecular biology. A mega-laboratory nearby, with a director who was said to have very good antennae, was working on the same project. Hence the silence edict. Others in the room joined in and echoed the need for silence. Soon most of the students and faculty in the room, were commenting on the practicality of keeping quiet about new and exciting results. One couldn't just run out in the hallway and tell everyone about your discovery because "they might hear about it." So maybe the joy and excitement of the moment of discovery is dead. Maybe nowadays the competition-driven paranoia of lab heads won't allow a moment of discovery. If you have new and exciting results you share them with no one but your mentor. The reward seems to be the anxiety of wondering whether the other guys will find out about your results before you can publish them. Competition, not communication, is what is uppermost in everyone's mind, and fame, not fun, is why people seem to be doing science. Here's an odd twist. Perhaps internet publication, with the possibility of instant publication of un-reviewed results, could once again allow students to run out into hallways to tell everyone of their exciting discoveries. Modifications of our own behavior, though, might be a better way to do that.

There is a wide eyed excitement, enthusiasm and idealism that most students have when they enter our research laboratories. Here is the world of discovery! Here is the land of the unknown! Here are powerful and evolving technologies that can cure disease, and explain how we develop, learn, think and behave in the biological world that surrounds us. Here are adventures that can bring us to the level of the very atoms and molecules involved in unraveling these mysteries. Why are we (and now, one can add, our deans and business administrators) taking the fun out of all of this? Why are we allowing this to happen? Students learn by example, and some of the examples we set leave much to be desired. Our job should be to teach students how to do science, that wonderful process that allows us to make discoveries, and not to poison their minds with our insecurities, failures and sometimes atrocious behavior.

Oh, and by the way – I've never again seen or heard from the woman who sat with us at lunch in Bar Harbor that day. Still, with about 20 students in the Pappenheimer class that long ago morning, I have fond hopes that others will surface to rekindle memories of that rare, and very special, shared moment of discovery.

AUTOBIOGRAPHICAL SKETCH

Avis Cohen
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Snapshots of growing up female in the '40's, '50s and beyond – born Avis Hope Schulner, November 29, 1941, just before the start of WWII. Sent to finishing school for unfinished girls; joined Future *Nurses* of America; told "Don't be too smart, the boys won't like you!"; won the Sons of the American Revolution award – but a problem, I was Jewish! Oh well, never mind, after the teachers argued with them. Being groped in the darkroom, being groped in the library. Married at 19; pregnant at 22; mother of two at 26. Onto the "sixties"! The Vietnam War, anti-war demonstrations; joined SDS; was a full-time mom, taking the kids to demonstrations…. Then off the walls with no more wars to fight. Books described

Avis Cohen

how women, careers, marriage and family didn't go together; you couldn't do all of them, could you? Through a Danforth Fellowship for women who went back to school, and meeting the emerging feminists – maybe I could?? WHY NOT?? At least I could try – and I did – but couldn't of done it without Mika Salpeter as a role-model and supporter when other women, including my mother, said I'd destroy my children and marriage – and my sons are great, now 35 and 32, and a 14 year old grandson, and marriage going well after 39 years together…. YES!!

I began my undergraduate education at the University of Chicago in 1959, and met biopsychology and the ethologists of America of the time. Eckhard Hess was there, and I learned all about Lorenz and his geese and how chicks imprinted. From Ted Schaefer, my mentor and boss, I learned all about the psychobiology of stress. Ted and I also discussed the reasons why ethology was having such difficulty gaining a foothold in this country in the '50s. The fear of the Nazi's was still very strong. The notion that behavior was genetically determined, and therefore perhaps racially specified, was a hard sell to a nation that had won the war against such doctrines. There were even non-native explanations for pecking in chicks – for example, the head was bobbing on the heart, providing a teaching signal for the movement. Most importantly, I was caught by the intellectual excitement in the College of the University of Chicago, and began to understand the joy of the intellectual life. I was also exposed to discovery and experimentation in a research laboratory. I was on my way to an academic life with biology as the major focus.

I left Chicago after two years, to continue my education while joining my new husband, Marshall Cohen, a math graduate student, at the University of Michigan in Ann Arbor. My experience with rats at Chicago got me a lab job at Michigan working in a psychology laboratory, implanting electrodes in the brains of rats and recording their "galvanic skin responses" when they were exposed to surprise stimuli. The work was published, with me as a co-author. But sitting in comparative physiology from William Dawson I learned that rats had no sweat glands – OOPS! By that time, my former boss in the Psychology Department had also learned this embarrassing fact, but the harm had been done…. That's one paper I never referenced or put into my CV. The experience also led me to biology and away from psychology. I worked for a brief time with Don Maynard, who developed the stomoatogastric ganglion preparation for motor systems research. He was very impressive; so impressive, I was afraid of failing him. I left his lab to work with Billy Frye, a gentle southerner who didn't scare me as much. However, Maynard influenced me more than anyone else during those early years before his premature death a few years later. He gave me a glimpse of the power of clear, logical and sometimes complex thinking and helped me to understand and appreciate what constituted overly simplistic thinking and its origins. He also helped me accept that mine was the better variety even when it sometime disagreed with others.

I claimed my BS at Michigan in 1964, only a year behind schedule, and went home to bear and raise my first son, and accompany my husband to the Institute for Advanced Studies in Princeton. In Princeton, I continued my political formation. I joined my first demonstrations against the Vietnam War and worked like a madwoman for McCarthy, Eugene, that is, only to learn a cynical lesson about American politics as we watched the disastrous Chicago Democratic Convention in 1968. The voice of 40% of the party was completely suppressed by the mainstream of the party, and the Johnson/Humphrey supporters prevailed. On the scientific front, I was exposed to a mathematical theory "catastrophe theory," developed by René Thom. Unfortunately, the theory was prematurely and loosely applied, causing considerable skepticism about the usefulness of dynamical systems modeling, of which this was an example. However, I was convinced that dynamical systems, if done well, offered a powerful approach to biological phenomena.

In 1968, my husband accepted a position in the Math Department at Cornell University, where he has been ever since. Off we went again, now with two sons in my full time care. I soon discovered that my view of dynamical systems was shared by a new Professor at Cornell, Neurobiology and Behavior, Eric Lenneberg. Being a full time mom was wearing very thin. I applied to graduate school at Cornell to do a master's degree with Lenneberg. I had no idea at that time that I would become a full time academic. I thought I'd be a technician or do such thing as befit a married woman of my generation. However, I was accepted in the PhD program, as Cornell didn't give a MS. Lenneberg assumed I could do anything – but could I? He sent me into development of motor systems, his first love, even though his work mainly centered around language development and aphasia and he knew nothing of modern motor control. He also suggested I apply for a Danforth Fellowship for Women – and I got it. GULP! Now I really had to do it!

The years at Cornell were most influenced by Lenneberg, and his global thinking, as well as the psychology people including most importantly J.J. and Eleanor Gibson and Ulrich Neisser, soon to be called cognitive psychologists. I also had a wonderful cohort that included Helen Neville, another of Eric's students, as well as other notables such as Martha Constantine (-Paton), Peter Narins, Albert Feng, and Bruce Land. I learned that psychology was more than some of the thin concepts I had been exposed to earlier, and that biology and psychology could be happily united if done carefully – not easy, but possible. Martha and I read development and formed our own opinions of the great debate between Paul Weiss and Roger Sperry. One small piece of this debate was to form the basis of my thesis. I wouldn't answer any major (or even minor question) in this realm, but it did lead me into motor systems and control of locomotion. When I presented the results at Neurosciences, Sperry, whose work I challenged, simply responded, "Oh, interesting. I believe it." Weiss, whose work I was closer to, was far more contentious.

Marshall took a visiting position, at the University of

Michigan for a semester. So, my family and I returned to Michigan and I had the very good fortune to work with Carl Gans. He became my major mentor from that point on, especially as Eric was more and more unable to help me. And finally, when Eric committed suicide, it was Gans, as well as Bob Capranica, who took over and guided me through.

During my last year of graduate school, I heard Sten Grillner give a talk about CPGs (central pattern generators). I was stunned. On the basis of Nicholas Bernstein's early work, Lenneberg had predicted such circuits must exist. He had missed the evidence that they did. However, there was no question that this was to be my new direction.

In 1977, two children and a husband in tow, I left for Stockholm and the Karolinska Institute. My husband was correct – there was NO topology in Sweden, but he made frequent trips to Germany to stay alive. After two years, Peter Wallén and I showed that the isolated spinal cord could produce the full swimming motor pattern (to be called "fictive swimming"). However, it took many months of frustrated struggle with electrical stimulation of the spinal tracts, only to accept the idea to use the American lamprey, *Ichthyomyzon unicuspis* from Carl Rovainen, and the idea of Margaret Poon's to use pharmacological stimulation with excitatory amino acids, e.g., D-glutamate which together worked like a charm.

In spring of 1979 I returned to Cornell University with no job. The Math Department generously provided me with an office in the basement while I wrote grant proposals. Over 1980, I maintained an intermittent post-doctoral position with Carl Rovainen at Washington University. He has remained important to me since that time. Back in the Cornell Math Department, I met Philip Holmes, a new faculty member looking for problems to catch his interest, and Richard Rand, an old friend. I explained the difficulty of understanding the coordinating system of the lamprey – almost nothing could destroy it! It clearly showed ascending and descending effects, and was very complicated anatomically. So began a wonderful collaboration. In 1982 we published our first paper on systems of coupled non-linear oscillators, using dynamical systems theory (!). I had returned.

But mathematicians have half-lives of about 5 years, so in the mid-'80s Nancy Kopell and I began talking about coupled oscillators. Over several trips to Boston, she and I developed many ideas, and I spread the gospel of dynamical systems to Eve Marder who found it easier to work with physicists than mathematicians. Our little lamprey group soon expanded to include Karen Sigvardt, Thelma Williams and, of course, Bard Ermentrout, Nancy's long time collaborator.

As they say, the rest is history. I stayed at Cornell, again with the help of Mika Salpeter, in my own lab and supported on my own grant to study locomotor control from 1980 to 1990, when the University of Maryland made me an offer I couldn't refuse: a tenured position. This time, my kids were fledged and long gone and my husband maintained his position at Cornell. Since 1990 we have had a commuting marriage with Marshall bearing the brunt of the traveling. Its difficult and has led to all kinds of important self-discovery, but our marriage is stronger than ever, and we are both happy in our work. Ill take it!

ISN NEWSLETTER

A society newsletter can serve several different purposes. Most importantly, it is a way of informing members of forthcoming events (e.g., future Congresses and other meetings), important happenings, and general "news" of the society and its members. However, a newsletter can be more than conveyer of news (albeit, often late these days with E-mail and the web). It can also try and provide unique insight and information that members are not likely to get from other sources. The ISN newsletter started out primarily as a purveyor of news and information. However, over the past several years we have tried to add material which helps ISN members know more about their science and the people who do that science. A few years ago we added autobiographical sketches and lab reports, and with this issue we have started a regular column by Ed Kravitz.

However, we would like to still do more with the newsletter to make it even more interesting and valuable to ISN members. Indeed our goal is to make the newsletter something that members look forward to receiving, just as they might look forward to the latest issue of their favorite journal, or, as I did as a boy, the Friday arrival of the latest issue of *Life Magazine*.

So, we solicit input from you in two ways. First, if you have suggestions for additional material that might be included of broad interest (e.g., articles like those written by Ed Kravitz) this would be most welcome. As far as we are concerned, anything is acceptable as long as it would be of interest to ISN members. Second, if you have suggestions for people who might write autobiographical sketches (as the article by Avis Cohen), or lab reports (the report on the Department of Animal Behavior by Peter Moller), we would like to hear your thoughts.

If you have ideas for people to write articles, or would like to write one yourself, please drop a note to Art Popper.

NEUROETHOLOGY LISTSERV

Reminder: The ISN maintains a Listserv. Any member 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 an E-mail to John Hildebrand at <jgh@neurobio.arizona.edu>.

NEUROETHOLOGY AND THE DEPARTMENT OF ANIMAL BEHAVIOR AT THE AMERICAN MUSEUM OF NATURAL HISTORY
(1928-1971)

By Peter Moller
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The term neuroethology did not exist in the 1920's and 'ethology,' for ideological and/or academic reasons (innate, instincts, nature, and all that 'bad stuff') was not in high esteem among post-war students of animal behavior at the Museum. But this is 2000 and here, joining the discussion on academic roots, I wish to highlight the neuroethological spirit that pervaded the department right from its beginnings.

The American Museum of Natural History (AMNH) in New York City, around 1928, started a groundbreaking tradition with the establishment of the *Department of Experimental Biology*. Eminent naturalist and all-round zoologist, G. Kingsley Noble who already served as curator of reptiles at the AMNH became its first chairman. Noble and his colleagues decided to embark on a novel, integrative, and comparative approach to address questions about evolution and development of behavior, and to seek answers on both the organismic and physiological levels of organization. A true visionary, Noble realized the significance of integrating studies of organismal biology (behavior) with neuroanatomy, physiology, endocrinology, and ecology. This sounds fairly modern indeed! The academic discipline of animal behavior at the Museum got its start at about the same time that ethology took root and began to thrive in Europe.

Very quickly, this oasis of experimental research on live organisms in the middle of New York City developed into a true hotbed of science in action encompassing both theory and research, much if not most of which, were it performed today, would be accepted as neurobiology including behavioral neuroendocrinology, neurophysiology, and neuroethology. By 1950, the department's reputation as a leading center for the study of reproductive behavior had been established through the seminal work of Noble, F. A. Beach, and L. R. Aronson. With T. C. Schneirla's arrival in 1943, the department's theoretical outlook on behavioral causation became focused on the role of developmental interactions between the organism and its environment rather than on inbuilt or innate properties.

Throughout its existence from the early 1940's to its break-up in 1981, the DAB always welcomed active research fellows, research associates, guest scientists, graduate and undergraduate students, and myriads of volunteers. In 1971, the Museum started a joint venture with the Graduate School of the City University of New York establishing the *Animal Behavior-Biopsychology Program* uniting the DAB with the doctoral programs in Biology (City College) and Biopsychology (Hunter College). Over the years, research associates and post-docs, comparative psychologists, neurobiologists, neuroethologists, and ethologists joined, among them Carl Berg, Peter Borchelt, Catherine Cox, Cheryl F. Harding, Wayne Lazar, Rae Silver, Howard Topoff, H. P. Zeigler, and this chronicler.

Setting the research agenda. Noble's 1931 book, *The Biology of Amphibia*, became a classic and served as a model for studies in comparative biology, including behavior. Much of his later work on endocrine control of anuran mating behavior was in collaboration with his doctoral student, Lester Aronson, exploring the mating patterns of *Rana pipiens* and its neural bases. Noble had gained wide attention when he, in a 1926 article in *Nature*, showed Paul Kammerer's claim of Lamarckian mechanisms at work affecting the nuptial pads of the midwife toad to be fraud. When Noble died in 1940, AMNH would have closed the department had it not been for the intense lobbying of Frank Beach, the assistant chair. Beach had been a post-doctoral scholar with Karl Lashley at Harvard where he turned his full attention to endocrine correlates of reproductive behavior. Lashley recommended a position at the AMNH, a perfect match. Following Noble's death, Beach as newly appointed chair, renamed the department to *Department of Animal Behavior* (DAB). After the war, Beach was also instrumental in getting the European school of ethology known in the U.S. Lorenz paid a visit to the AMNH in the late 50's. Until he left the DAB in 1946 to assume a professorship at Yale University, Beach left a legacy of publications that paved the way for his noted fame. He is widely acclaimed as the founder of neuroendocrinology for his seminal work on the central nervous mechanisms involved in the reproductive behavior of vertebrates, and in the analysis of factors involved in the arousal maintenance and manifestation of sexual excitement in male animals. When not indulged in testosterone, sex, and rats, in the true comparative spirit, Beach resorted to playing the trombone to test the hearing abilities of lazy, half-deaf alligators roaming about the DAB (*Amer. Nat. 78: 481-505, 1944*). 'Angry Mosquitoes' and 'Playing Fishes' appeared in *Science 101, 610-611 (1945)* and *COPEIA 1945, 241 (1945)*, respectively.

Noble and Beach had established the neuroendocrine tradition at the DAB that was left in excellent hands with Lester R. Aronson who had obtained his MA with J. W. Papez at Cornell University and his Ph.D. with Noble at NYU. Lester led the department from 1946 until his retirement in 1978, but stayed on as curator emeritus until his death in 1996. Although Beach and Aronson were good friends and shared common research interests, they never published together. Lester's research interests were truly comparative, truly endocrine, and truly neuroethological. He pursued three major lines of research, all intellectually related, vigorously and with a passion: (1) The work on neural mechanisms controlling *mating behavior in the leopard frog* addressed several still unanswered questions raised in his doctoral thesis. (2) *Cat work* started in the early 1950's and focused on the neuroendocrinological bases of reproductive behavior as well as the role of experience during development: Mating behavior in sexually

inexperienced cats after desensitization of the glans penis (with M. Cooper). Science initially rejected this 1969 manuscript because it contained the word 'penis.' Development of normal behavior was compared with reactivated behavior in castrated, hormone-treated cats: sexual behavior once established (organized, we say today) tends to remain fixed (with J. Rosenblatt). As most of the work was done on male cats, it was about time that Carol Diakow (Aronson's last NYU graduate student) studied the effects of genital desensitization and mating behavior in female cats. (3) *Fish work* was conducted both in the field in Nigeria when Lester was a Fulbright fellow (1953-54), at the Museum's Bimini field station, and in the laboratory. This work explored the role of the fish's forebrain and cerebellar functions in reproductive behavior. Neither part of the brain controls the behavior, but acts as an arousal mechanism that facilitates the functioning of the lower centers of the brain (with Harriet Kaplan). Some of the students Lester mentored have continued or are still continuing the legacy, among them Eugenie Clark, Jay Rosenblatt, Lawrence Kunstadt, Jack Izower, and Carol Diakow.

Setting the theoretical framework. The arrival of Theodore C. Schneirla to the AMNH added a powerful 'third force' in behavioral theory: epigenesis next to neo-behaviorism and extreme nativism. Schneirla was trained as psychologist at the University of Michigan where he had pioneered studies of maze learning in ants. He was the foremost American comparative psychologist of the mid-1900's. The 1935 *Principles of Animal Psychology*, co-authored with N.R.F Maier, was the leading text in the field. Schneirla's thinking about behavior and its causation affected the work of most of his colleagues in the DAB. The emphasis was now on the interaction of environment and heredity and the role played by each in the development of ants, fishes, birds, rats, cats, etc. Schneirla's work on army ants was certainly not neuroethological in nature, but his intellectual influence on immediate collaborators and doctoral students is reflected in their own work (e.g. Tavolga, Lehrman, Rosenblatt, Tobach, Adler, Shaw, Turkewitz, Topoff, Gold). (1). Behavior of army ants that "investigators formerly explained by expressions such as inbuilt or innate can be understood in terms of the energizing and pacing properties associated with developing brood of eggs, larvae, pupae and young workers." (2) Cats have no instinctive 'know how' concerning what to do when they give birth to their young, but normal relations between mother and young develop as they interact with one another (with J. Rosenblatt). (3) Feeding of the young in ringdoves is not an instinctive action on the part of the parent, but one that must be learned by experience. This behavior is learned though elaborate interaction between parent and nestling. Ringdoves also must experience their visual world during early development to optimally respond to relevant shapes (Lehrman). (4) Schooling behavior which previous investigators labeled innate can be modified by conditions in which fish were reared, depending on critical light intensities, early experience with neighbors, and interaction with their parents (E. Shaw).

As an aside, it was a stroke of luck that ringdoves ever made it to fame. Initially, Lehrman had intended to work with zebra finches; only these birds never got into the mood (the premises were much too dry, as Cheryl Harding determined many years later). However, ringdoves were constantly mating on AMNH premises, and the rest is history. Schneirla's untimely death in 1968 left a vacuum in the progress of behavioral theory. But thanks to his colleagues, notably L. Aronson, E. Tobach, J. Rosenblatt, and D. Lehrman, as well as his former student H. Topoff, his work and legacy have been celebrated in a hallmark trilogy of books published by Freeman in 1970, 1971, and 1972.

Animal behavior, epigenesis, and the neuroethological spirit. Noble, Beach, Schneirla, and Aronson have passed away, but their legacies live on in their students' work and in their students' students' work. I was very fortunate to chat in some length with William N. Tavolga, a most lively window to the past. Bill's tenure (first as volunteer and later as research associate) with the AMNH spans more than four decades, so he knew all the players. While G. K. Noble intimidated the young Tavolga when he was looking for volunteer work in herpetology ("state your business"), T.C. Schneirla became his scientific hero. Bill set up shop in the DAB, obtained his Ph.D. with Charles Breder, himself a short-time citizen of the department, and embarked on a most successful career contributing to the then nascent understanding of sound production of fishes and its biological significance. Bill considers his work with Jerome Wodinsky (a Bitterman student) on hearing thresholds in several species of marine fish his best ("a classic"). Marine fishes hear best within the range of 200-600 Hz and are virtually deaf above 2000 Hz. Later he determined that goldfish could discriminate differences of 2% in frequency and 4-5 dB in intensity. Arthur Popper, one of his former graduate students, is carrying on the fish acoustics tradition that started accidentally around 1940 when some desk space next to T.C. Schneirla's office in the DAB was vacated to make room for Bill's fish tanks stocked with gobies!

In preparing this précis I have appreciated, once again, the breadth and depth of the behavioral research that was conducted at the Museum, its theoretical underpinnings, and its healthy impact on almost everyone who was lucky enough to have been affiliated with the DAB. Lester Aronson and Bill Tavolga were my undeclared mentors. Thank you both!

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GRADUATE AND POSTGRADUATE POSITIONS

Postdoctoral training opportunities in **Comparative and Evolutionary Biology of Hearing** at the University of Maryland, College Park. Our research group includes 11 faculty and over 50 students, postdocs, and visiting scientists. Research emphasizes basic auditory mechanisms using a wide range of experimental approaches. Research models include insects, fish, amphibians, reptiles, birds, and mammals (including humans). We have strong interests in comparative and evolutionary issues. Investigators include: Drs. Catherine Carr, Robert Dooling, Sandra Gordon-Salant, William Hall, Cynthia Moss, David Poeppel, Arthur Popper, Joelle Presson, Shihab Shamma, Jonathan Simon, and David Yager. The program strongly emphasizes inter-laboratory collaborations and training. Postdoctoral positions are supported by a training grant from NIH (limited, by law, to US Citizens and permanent residents) or individual research grants. For details of our research and training program see www.Life.umd.edu/cebh or contact Dr. Popper at AP17@umail.umd.edu. UM is an Affirmative Action Equal Opportunity Employer.

Post-doctoral associate wanted to participate in our research on the **role of serotonin in the production of the swim motor program in the sea slug**, *Tritonia diomedea*. Preference will be given to applicants with electrophysiology experience. A number of potential projects are available allowing the post-doc the opportunity to use a variety of techniques such as confocal microscopy, real time imaging, flash photolysis of caged compounds, microvoltammetry, and computer modeling. We are studying how serotonin is regulated and how its effects are integrated into a known neuronal circuit. Visit http://www.gsu.edu/~biopsk/ for more information about our work. If you are interested, please send your *c.v.* (including educational background, research experience, and publication list) and the names, addresses, phone numbers and e-mail addresses of 3 references to: Paul S. Katz, Dept. of Biology, Georgia State Univ., P.O. Box 4010, Atlanta, GA 30302-4010, e-mail: pkatz@gsu.edu, Georgia State Univ. is an equal opportunity employer.

Assistant Research Scientist - Univ of Maryland
Computational neuroscience lab is seeking a full-time research assistant to aid with the recording and analysis of songbird vocalizations during the period of song learning. Responsibilities include:1. Helping to develop software for a voice-triggered system to collect and store song output from developing birds. 2. Developing signal processing software to analyze, segment, and categorize avian vocalizations.3. Maintaining a large database of song data. 4. Administering a small network of Linux/PC workstations and related peripherals. Must be able to work independently. Background in signal processing and/or machine learning desired. C programming and Unix/Linux system and network administration skills are required. For more information see http://www.bsos.umd.edu/psyc and/or http://www.umd.edu/NACS. Please send letter of interest, resume, and the names of three references to Dr. Todd Troyer, Department of Psychology, Univ.of Maryland, College Park, MD 20742; email: <ttroyer@psyc.umd.edu>

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