INTERNATIONAL SOCIETY FOR NEUROETHOLOGY

Newsletter
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MEMBERSHIP DIRECTORY

Please check your entry in the Membership Directory at the ISN Website and notify Panacea Associates: ISN@panassoc.com of any changes to be made. The Website URL is http://www.neurobio.arizona.edu/isn/

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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AUTOBIOGRAPHICAL SKETCH

From Medicine to Neuroethology....and Back Again

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After beginning in clinical medicine, my career path through neuroethology now seems to be returning unexpectedly to the clinic. At Art's suggestion, I will provide some highlights around that circle of nearly four decades.

1960s

My inspiration to drop medicine and take up research on the nervous system was Jack Eccles. A dutiful, top-o'-the-year medical student, I was bored by the repeated cycles of swallowing medical minutiae and regurgitating them for examiners. One colorful talk by Jack Eccles, on inhibition, was enough to ignite a rocket that is still burning. Eccles had recently been "liberated" during a short period of exile in NZ, where he carried out the intracellular work on spinal motoneurons that earned him the Nobel prize and, incidentally, met Karl Popper. The encounter with Popper changed Eccles. No longer shy about framing bold hypotheses, he now reveled in their falsifiability instead of fretting about their correctness. I think that I got permanently infected by the same bug, having had lectures by the philosopher Armstrong at the University of Sydney just before hearing Eccles' bold lectures.

In contrast with Eccles, Peter Bishop was not an inspiring lecturer ("I would be amiss if I gave the students the impression that anything in *Nature* was simple..") but his wonderful, orderly lab had a magnetic attraction and left an indelible impression. I particularly remember my first encounter with agony discharges of neurons and their synchronous roaring to the strobe, like a crowd at a football match, as the electrode approached the LGN. For bureaucratic reasons, I could not work with Eccles and soon found myself surrounded by the twinkling lights and multiple screens of Bishop's superb lab, the crowning glory of which was a multichannel averager, converted from use with radionuclides and one of the first in the world to generate PST histograms. Bob Rodieck was in the lab at the same time, so there were high standards of quantitation to be upheld.

Peter Bishop gave me an astronomer's counter-rotating biprism to align the cat's eyes (which diverge in paralysis) and the task of finding out what happens when you stimulate both receptive fields of a binocular cortical neuron with a single stimulus (David Hubel and Torsten Wiesel had recently published on binocular neurons, using prisms to produce two stimuli, one for each eye). In the end, I found that the binocular neurons of cat V1 are very disparity sensitive and also vary in their preferred disparity....a finding that is now generally accepted for both cat and monkey V1.....but which took a long time to find favor, probably because it did not originate at Harvard!

1970s

My stint at Caltech seems dream like in retrospect: so much funding that I actually had to do a little work to keep my group from growing too big; superb laboratories that had been designed exactly as I had specified; undergraduates who were so much smarter than me but who often made great contributions to my research once I got over the initial shock; extremely supportive faculty who encouraged forays, such as my flirtation with a SQUID magnetometer and avian biomagnetism, that would have been foolhardy in any other circumstances; field trips to the neotropical rainforests where I was chastened by the diversity and quickly had to become more sophisticated in the formulation of evolutionary hypotheses.

Mark Konishi and I kept a few cases of champagne on hand to celebrate discoveries during that era. Some causes for celebration that I remember were quite a few front covers on *Nature* and *Science*, at least four

discoveries on the owl (cat like visual cortex, effects of deprivation, auditory receptive fields, sensory cortex specialized for the foot only), importance of the catecholamine innervation in modulating visual cortical plasticity, and differential effects according to ganglion cell class in the chiasmal defect in Siamese cats.

1980s

After being so high on the hog in the US, I was pretty depressed, at first, when I returned to Australia for family reasons. I was kept afloat by my interest in native fauna, in particular the unusual nightjar relatives found in Australia and New Guinea, as well as the visual system of the bifoveate avian raptors. My relationship with some of these birds borders on the shamanistic, and I have yet to write up many of these studies, probably because they are subtle and deep and do not have the fiery breakthrough quality that accompanied many of the findings in the previous era at Caltech.

My all-time favorite is the letter-winged kite, the only nocturnal member of its family but with a bifoveate eye and other hallmarks such as a dependence on moonlight for hunting and courting that betray its very recent shift from a diurnal niche to its limited nocturnality. Almost equally favored is the owlet-nightjar, whose visual system is owl-like except for the presence of well-developed eye movements. By studying this bird and its nightjar relatives, I came up with new evidence to support Julesz' original conjecture that stereopsis may have evolved in the first instance not for depth judgements *per se* but to break camouflage.

To Australia I also owe the "flying primates" story. If my university here had not been dilatory in building my cat colony and if John Allman had not tutored me at Caltech in primate evolution and the key primate "signatures"in the brain, I would not have been flabbergasted at what I saw in the microscope after labeling the flying fox retina from the midbrain. There were very few cells labeled, in contrast to the high density labeled from the thalamus, but even more important, the retinotectal neurons were sharply decussated...a feature hitherto thought to be confined to primates. The outlandish implication that flying foxes are actually primates, or close relatives, is the one that has caused all the stir, but it stills seems the most likely explanation for all the differences between the two kinds of baths that have been documented in bat paleontology, biology, neurobiology, molecular biology etc.

1990s

My return to clinical medicine was completely unexpected and involved a fish.....really! The fish is the sandlance, a chameleon-like teleost with eye movements that alternate from side to side. Because the visual pathway is totally crossed, this fish is an existence proof of interhemispheric switching, an idea that I had been toying with ever since hearing Rama describe the complementary cognitive styles of the two frontal lobes in humans. Both the "Go!" General (left hemisphere), and the "Stop!" Devil's Advocate (right hemisphere) are valid, arguable cognitive styles....but it does not make any sense to me to engage them simultaneously. Hence the interhemispheric switch, which would alternate activation of the left hemisphere (confident, go!, General) and the right hemisphere (cautious, stop!, Devil's Advocate).

Spurred on by this little fish, which had been rediscovered and studied intensively by Shaun Collin when he was working as my PhD student on the Reef, I went looking for evidence of a switch in humans. My undergraduate classes provided plenty of keen subjects, and we soon got weaving with some new hemispheric activating techniques such as caloric stimulation and transcranial magnetic stimulation. We found that binocular rivalry behaved in a way that suggests that it is mediated by interhemispheric switching between the highest visual cortical areas (IT cortex), in contradistinction to prevailing ideas that rivalry occurs in V1.

After testing about 100 students, I had a very good idea of the range of variation in the switch rate. So you can imagine my surprise when I sat down on my own apparatus, under exactly the same conditions, to find that I was nearly ten times slower than the average! Because I am a manic depressive and because I take lithium, my first thought was that the lithium was slowing down my switch. This proved incorrect, because taking myself of the lithium had no effect on the switch rate. The next possibility I tested by recruiting more manic depressives through my medical colleagues. They all proved to have slow switches too.

This finding has taken off, with replication trials in a number of places confirming it. Because the switch rate seems to be genetically determined, based on similar rates in monozygotic twins, I am excited by the possibility that slow switch rate may provide a marker for an underlying predisposition to bipolar disorder. I am presently checking its usefulness in diagnosis at first presentation of psychosis (where it may be very difficult to distinguish major forms of psychosis).

I attribute the psychiatric finding to my neuroethological background. It is likely that I would have been too snobbish to be caught playing around professionally with a weird 2-cm fish if I had stayed with the medical fraternity instead of taking the path I did. Indeed, my formulation of the interhemispheric switch as a series of subcortical bistable oscillators owes much to my neuroethological background in such esoterica as interhemispheric switching in birds and the paired bistable oscillators studied so well in invertebrates.

Of course, one also has to acknowledge the fateful dice. It is certainly quite a coincidence that a manic depressive made the finding on himself having set up about $100K worth of VisionWorks¨ psychophysics, as well as having the conceptual framework environment provided by neuroethological colleagues and their model animal systems.

That be as it may, I am enjoying myself immensely as I test a wide variety of people inside and outside the bipolar spectrum and learn about how they have coped with emotional swings. Most find the idea of the interhemispheric switch useful....a kind of hat rack onto which they can hang there own life experiences..... and they don't seem to mind very much when I draw my long bow and compare the human brain stem to that of a fish!

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MEMBERSHIP IN ISN

You can obtain a membership form to join ISN or update your membership information at the ISN Website or by using the form in this issues of the newsletter. Please encourage colleagues to consider joining ISN.

MATERIAL FOR FUTURE NEWSLETTERS

Send news, job advertisements, meeting announcements and other related information for the next newsletter (to be published in early July) to Arthur Popper at AP17@umail.umd.edu. All material should be sent via E-mail.

Advertisements for jobs and graduate/postdoctoral positions should be no more than 150 words. Suggestions for *feature articles*, including autobiographical sketches, research group reports, and Neuroethological Viewpoints, should also be sent to Art Popper. However, please do not submit full articles of this type without a response from the Editorial Board. Feature articles may be up to 1,500 words in length. We also welcome research commentaries, book reviews, and other material that might be of interest to the ISN community. These should be no longer than 450 words in length and should only submitted after consultation with the editor.

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RESEARCH GROUP REPORT

Thirty is a large number: staying busy with the STG

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All of us with any claim to the title of "neuroethologist" wish to understand the neural control of behavior. Many neuroethologists study behaviors that are intrinsically fascinating, such as how bees or ants navigate in complex environments, how birds learn to sing, or how owls combine auditory and visual cues to hunt their prey. As intriguing as these systems are, they present formidable challenges for the investigator wishing to discover the detailed cellular mechanisms that produce these behaviors. In contrast, those of us who study the stomatogastric nervous system of lobsters and crabs content ourselves with a behavior that at family holiday dinners elicits the inevitable question, "Are you still studying the lobster's stomach?" Each year, I answer, "Yes" to this question precisely because the stomatogastric nervous system remains in my eyes the premier preparation for asking how circuit dynamics relevant for behavior depend on the properties of the cells and their synaptic connections.

The modern era of study of the stomatogastric ganglion (STG) dates from its introduction for studies of motor pattern generation by Don Maynard in the late 1960's. He introduced it to Al Selverston, Dan Hartline, and Maurice Moulins who after Maynard's untimely death became the scientific fathers, grandfathers, or great-grandfathers of most of us now working with the stomatogastric nervous system. Today, the STG community consists of approximately 15 laboratories in Japan, Israel, Germany, France, and the United States, with a total population of about 80 investigators, including undergraduate students, graduate students, postdocs, and faculty. This size is not unlike that of the STG: our community is large when compared to the number of people working on some preparations but considerably smaller than those using *Drosophila* or studying the mechanisms of LTP. Our size also contributes to our collective success: we have critical mass, but because the stomatogastric nervous system is a small but complete nervous system, it presents the opportunity to study a diverse and rich set of experimental problems. The continued vitality of this preparation is due to its use to tackle new and different scientific questions over the years. Here I focus on several of the problems that my own group of three postdocs, four graduate students and several undergraduates presently studies. I regret that I don't have the space to describe also the work of all of my past, wonderful, and gifted students, postdocs, and colleagues.

The functional consequences of so many modulators. Why? One of the most striking features of the stomatogastric nervous system is that it is so richly modulated. The 30 or so neurons of the STG are modulated by at least 20 different substances liberated from either terminals of identified projection neurons to the STG or reaching the STG via hormonal delivery. In this, the STG resembles many regions of the vertebrate nervous system that are also richly and profusely furnished with multiple modulatory innervations. This provides a degree of potential plasticity that is difficult to comprehend. Despite the extraordinary amount of modulatory potential in the stomatogastric nervous system, these circuits maintain stability. Indeed, it is remarkably difficult to find conditions in which these networks are "overmodulated" into a nonfunctional state. Therefore, there must be a number of "built-in" mechanisms that prevent these and other circuits from being pushed out of their operating ranges. One of these mechanisms may depend on a remarkable degree of convergence of these multiple modulators.

Andrew Swensen, a graduate student, has been using voltage-clamp methods to study the currents evoked by a series of neuropeptides in STG neurons. He finds that a large number of the peptide modulators (Crustacean Cardioactive Peptide, Red Pigment Concentrating Hormone, TNRNFLRFamide, CabTRP, and the muscarinic agonist pilocarpine) all evoke a current that is indistinguishable from that evoked by proctolin, originally described by Jorge Golowasch. This finding was surprising because each of these substances produces different effects on the motor patterns evoked by the STG. This suggests that different motor patterns may be a consequence of differential receptor distribution. Additionally, because these modulators converge on a single current, at high concentrations, they saturate and occlude each other. This may be part of a mechanism that protects against "overmodulation."

Modulation and Sensory Encoding. Much work on sensory systems involves understanding how information about the stimulus is encoded in a spike train. The underlying premise of this work is that the spike train "represents" the stimulus, and, therefore, the neurons and networks that respond to that spike train can extract meaning from the spike train about the world. Paul Katz and Ron Harris-Warrick at

Cornell first described the properties of the GPR neurons. These neurons respond to stretch of some of the stomach muscles and project directly back to the STG. John Birmingham, postdoc, has recently shown that information about stretch is encoded in two ways, spikes or bursts. Moreover, the stretch response of the GPR neurons is modulated by a number of substances, which, in principle, makes the information in the spike train ambiguous. He now wishes to understand how this modulation of the primary sensory response is decoded by its postsynaptic targets.

The role of rhythmic activity in circuit development and homoeostatic regulation. Spontaneous activity early in development, while not necessarily behaviorally functional, may provide signals that allow the nervous system to establish and maintain appropriate sets of synaptic connections and to regulate the establishment of the intrinsic properties of individual neurons. Later in life, rhythmic activity may provide crucial roles enabling both synaptic and intrinsic properties to remain stable, despite ongoing processes of channel and receptor turnover, synthesis and degradation.

For a number of years, Larry Abbott's lab and my lab have collaborated on experimental and theoretical studies relevant to these issues. Mark Goldman (Abbott lab) and Jorge Golowasch are continuing theoretical and experimental studies on how activity can provide a signal that allows neurons to develop and tune their conductances to maintain a stable activity pattern. Michelle Withers is investigating the possible role of CamKinase II in these activity-dependent regulatory processes. Cristina Soto-Trevi–o is developing new models to account for the developmental and activity regulation of the strengths of the inhibitory synapses that form the pyloric rhythm.

One of our newer directions is the study of the role of modulatory inputs in the development and maturation of the stomatogastric nervous system motor patterns and cellular architecture. The Meyrand lab pioneered the use of the stomatogastric nervous system for the study of circuit maturation during development and showed that it is experimentally tractable during time periods when major modifications in the motor patterns it produces are occurring. Our two labs joined forces (ValŽrie FŽnelon visited my laboratory and worked with Valerie Kilman, Kat Richards, and Vatsala Thirumalai) to study the developmental acquisition of a number of the modulatory inputs to the STG in two species of lobsters. We found that some neuromodulatory substances are present relatively early in embryonic development, and others appear during larval life, so that the modulatory control systems of the STG develop quite slowly over a prolonged period of time. Moreover, the GPR neurons acquire their cotransmitters slowly as well, so that early in embryonic time these neurons contain only a subset of their final transmitter complement. Kat Richards, graduate student, is studying the effects of several of these neurotransmitters at different developmental stages, and finds that many of them are already physiologically active quite early in embryonic life. Richards and Bill Miller, a former postdoc, have also demonstrated that the regularity and frequency of the activity of one of the pyloric neurons increases slowly during development. Vatsala Thirumalai, a graduate student, has found that the STG motor patterns produced by juvenile lobsters are virtually indistinguishable from those produced by adults, although the neurons and the ganglion are an order of magnitude smaller, arguing that activity-dependent mechanisms must be constantly used to maintain constant network output while the STG is growing.

Coupled Oscillators and Synaptic Depression. When Yair Manor and Farzan Nadim were postdocs in the lab, they worked on several projects in which they combined experimental and theoretical methods to study fundamental issues of oscillator coupling. In one project, they described the temporal dynamics of the graded transmission between two STG neurons and discovered that the synaptic depression at this synapse could be a switch controlling the pyloric frequency. In another set of experiments they studied the interaction between the fast pyloric rhythm and the slower MCN 1-activated gastric mill rhythm using theoretical methods. They followed this with a collaboration with Marlene Bartos, then a postdoc in the Nusbaum laboratory, to test their results experimentally. These projects are particularly gratifying to me because both Nadim and Manor have recently established their own labs at Rutgers and Ben-Gurion University, Beer-Sheva, respectively, where they are pursuing these lines of inquiry.

Theory has become for us as much part of our "experimental program" as is anatomy or physiology. Models have many functions for us. Sometimes they allow us to check the reliability and sufficiency of our data. Sometimes they provide methods for new and more quantitative data analysis. Sometimes they allow us to

dare to ask big, new questions. It is comforting to know that as we head for the year 2000 and the new millennium that there remain fundamental and large unanswered questions that can be profitably addressed with a beautiful ganglion of only 30 neurons.

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ELECTRORECEPTION AND ELECTROCOMMUNICATION (Satellite meeting of ISN La Jolla, 1998). This volume gathers together the work of 28 leading authors covering a broad range of topics including the behavioral significance of electroreception, motor control of electrocommunication, sensory processing, development and regeneration, neural plasticity and systematics. The book (edited by R.W. Turner, L. Maler, and M. Burrows) appears as the Vol. 202.10 (May) issue of the *Journal of Experimental Biology*. It is available for society members to purchase at the reduced rate of $30 US (£20 sterling). To place orders, contact 'sales@biologist.com' or write/fax The Company of Biologists Limited, Bidder Building, 140 Cowley Road, Cambridge CB4 4DL, UK Tel: 44 (0) 1223 240 482 or 426 164; Fax: 44 (0) 1223 423353

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