

APPENDIX III

MY 1950-1972 LIFE IN PBio WAS EXHILARATING!

by

J. Walter Woodbury (1923 -)

PREFACE

My memories are almost the sole source of the material in this document. Many experimental findings show that memories are notoriously malleable and often wrong. My intent is to provide personal glimpses of the early heady days of the buildup of the great Department of Physiology and Biophysics at the University of Washington, School of Medicine. I hope that these will convey something of the feel for what it was like to be there then. I checked most dates and quote a few paragraphs but it is difficult or impossible to document most of what I have written. I apologize for any factual errors and/or misattributions.

PROLOG

Academic family

My grandfather John T. Woodbury, Sr. (1863-1936) was a lifelong educator in St. George, UT in pioneer times, My father, Angus M. Woodbury (1886-1964) obtained a PhD at 45 years of age and became a well-known biology/ecology professor at the University of Utah. All four of his sons completed PhDs, as did his two son-in-laws. My niece Karen Woodbury Hughes is professor emerita of Botany, University of Tennessee, Knoxville; my son, Dixon J. Woodbury is currently a professor of physiology and developmental biology at Brigham Young University, Provo, UT. A grandson, Nathan G. Lovell earned a PhD degree in Material Science and Engineering at the Massachusetts Institute of Technology, Cambridge in 2010.

1940-1945

I entered the University of Utah in autumn quarter 1940 intending to major in physics. As for most other people, things changed abruptly for me on 7 December 1941. Since physicists and engineers were in short supply I thought that I could best help the war effort by earning a de-

gree as fast as possible. I graduated with a BS in physics in August 1943, nine quarters later when I was barely 20. I had to take 20 credit hour the last quarter and found out that the resulting continuous sensory overload temporarily greatly diminished my ability to learn. One of the professors in the Physics department arranged for me to join the Radiation Laboratory at MIT as a Staff Member on 1 October 1943. The mission of the lab was to design, test and carry into pre-production radar sets for use on land, sea, air and in anti-aircraft fire control. Morale was high and the lab did a marvelous job. It is reasonable to believe that our superior radar equipment appreciably shortened the war. I was assigned to the Test Equipment Group 51, Applications Section. I remember very little of my routine duties but had a few memorable experiences tuning up the radar sets on a PT boat and a heavy cruiser during their shakedown cruises. The war ended in August 1945 and the lab shut down as rapidly as possible. I was terminated 30 November, returned to Salt Lake City and entered Graduate School at the University of Utah in January 1946.

Graduate School

I intended to pursue a PhD in physics but my oldest brother, Lowell (13 years my senior) talked me into going into physiology instead. I am glad he did! To ease the transition, I earned a Master of Science degree in physiology in 1947. My professors worried -- but I did not -- that I might stunt my career by getting all three degrees at the University of Utah. On the other hand, they wanted to keep me since I would be the first graduate student in Physiology. They got around this delicate problem by regarding my two years at the Radiation Laboratory as almost enough external experience. The other part of the package was that I was “urged” to spend the Autumn Quarter of 1948 touring centers of neuroscience research in the east. My first stop was at the Department of Physiology, University of Chicago, where I spent six weeks working with Gilbert Ling. He had recently perfected the fabrication and use of ultrafine glass micropipette electrodes to measure the voltage difference across the membrane of frog skeletal muscle cells. It was a really enjoyable and valuable experience. I learned how to pull micropipette electrodes by heating a short piece of capillary glass tubing in the fringes of a Bunsen burner flame! We experimented nearly every night for most of the night. We gathered enough data to write a paper on the effects of temperature on the resting potential of frog Sartorius muscle. I visited labs at the University of Rochester, New York, Yale University, Johns Hopkins Medical Center and a few other places that I don't recall.

I was one of the first awardees of a new NIH Pre-doctoral Fellowship Program. The stipend was only slightly less (\$2200/year) than my initial salary as a full time employee of the Radiation Lab (\$2400). My thesis research was on recording membrane resting and action potentials from single myelinated nerve fibers of the frog sciatic nerve. I received the PhD degree in August 1950 at the same time that my wife, Betty, received an MS degree in biology. Her thesis

title was “The Taxonomy of the Aquatic and Semi-Aquatic Hemiptera of Utah”.

Move to Seattle

I started looking for a position in the fall of 1949 and received three genuine offers, post-doctoral fellowships at Johns Hopkins and Cal Tech and an instructorship at UW. By this time I was tired of being a student and eager to get on with my life. Thus the Seattle offer was the most attractive. I knew that Johns Hopkins Medical School was located in a slum and despite its prestige, I would not go there. So Betty and I drove to Cal Tech and then up the coast to Seattle. I gave seminars and given “The Tour” at both places. Professors Wiersma and Van Harrevelde at Cal Tech treated me cordially but marine invertebrate neurophysiology held little interest for me; I obtained my training in a medical school and was really seeking a position in a medical school.

When we drove up to the brand new UW Health Sciences Building at the University of Washington, a few days later, its newness, large size and functional design amazed me. I was equally impressed with the facilities: seemingly ample space, three student laboratories and electronics and machine shops and ample size office and lab. I think Harry Patton gave me The Tour. He was a happy, friendly, genuine man who gave me the impression that he really wanted me to accept the position. I didn't say so but I really wanted the position..

THEODORE CEDRIC RUCH

First Impressions

Ted Ruch did not make much of an impression the first time we met. He was quiet, reserved and cordial but he did not fit my image of a dynamic department chair. A few hours later, during my closing interview with him, I dimly perceived that there was more to Ted than met my eye. He offered me the instructorship and

then concluded with the simple phrase, “You’ll come,” with such sincerity and subdued eagerness that I still remember it. I made a pretense of considering all my options but I had already made up my mind. I really wanted the Instructorship but was not sure what I had done to merit the offer. Nearly sixty years later I learned Ted’s view when I read the first draft of Marge Anderson’s history of PBio and found this statement:

Dr. Ruch was especially impressed by the fact that Woodbury had six full-length publications in national journals during his predoctoral career, and he came from a distinguished academic family (1949-50 Annual Report, pg 1).

Believe it or not, this fact had never occurred to me before. Therefore, I checked it out in my CV and found it was true. Another likely factor was his prescient early and continuing emphasis on developing a strong sub-department of biophysics and I fitted his needs. Over the years I slowly came to recognize some of the subtle qualities that enabled him to build an enduring, topnotch research and teaching department. I hope that the following comments shed some light on Ted’s personality, characteristics, operating principles, outlooks and accomplishments.

Life-saving Stipends.

Al Scher and I arrived in Seattle in mid-September 1950. Our starting salaries were low (\$4200 per year) but enough to live on. I think Ted was actively looking for ways to increase our income. At that time there must have been an agreement between the new medical school and the Madigan General Hospital at Ft. Lewis, Washington, about 50 miles south to send faculty to the Fort to give lectures on the latest advances in basic and clinical medical science. Ted took advantage of this program to send his junior faculty to lecture on various aspects of physiology and thereby earn a \$50 stipend. I suspect he did this more to bolster our income

than to educate the interns and residents at the Hospital. At that time they got even less sleep than they do now and most of them would sleep through the lectures. I don’t know if any of them benefited from my lectures but that extra income was indeed welcome!

The Ruch Theory of Car Repair.

When I arrived in Seattle Ted drove an old Ford coupe convertible. It looked ancient and decrepit at that time and he drove it for several years thereafter. I never saw it with the top down and the top looked ancient when I first saw it. He parked it under an evergreen tree at home and over the years, it slowly accumulated a rather thick layer of pine needles and pine cones. I venture to suggest that he took some perverse pride in driving the most dilapidated car in his neighborhood and around the medical school – a sort of deliberate I’m-not-keeping-up with the Joneses attitude. I remember him remarking one day that his car was not working, because of a broken part, perhaps the fuel pump. He added that he was not buying a new replacement part. He figured that putting in a new part would be too strong and would cause some remaining old part to break and so he always bought used parts. This impeccable logic so impressed me that I still remember – but don’t follow -- it.

Feedback to Teachers

I was glad to find the following statement in Marge’s History: “ Dr. Ruch gave junior faculty feedback on their lectures” because this was not my experience. I have always been serious about teaching and after my very first lecture to medical students, in the winter of 1951 on the nerve-muscle synapse; I went to Ted’s office and asked him how I had done. He seemed startled and thought for a few moments and then mentioned that my voice was a bit high. I agreed with this but there was not anything that I could do about that. I have often speculated about his lack of specific comments. After

many years, I came to realize that my first lecture was pitched at much too high a level and that I went far too fast. I had recently read the now classical 1951 paper of Fatt and Katz that clearly established that synaptic transmission is chemical in nature and summarized it in my lecture. I certainly snowed the students and most of the faculty too. I suppose that Ted knew that I had really blown it and apparently didn't know, how to tell me that my lecture was much too detailed and started at much too high a level. I didn't ever ask him again and he never offered any suggestions. I wish he had. I am sure any suggestions made to others helped them.

Ted's emphasis on teaching was one of his strong points and played an important role in developing an outstanding department. I particularly admired and agreed with his insistence that the entire faculty attend all the lectures. Although this was very helpful initially, it inevitably died of boredom in a few years. While it lasted I found continuing satisfaction in seeing and hearing a complete picture of human physiology every year. I learned much about teaching dos and don'ts from the other faculty and developed a well integrated concept of overall human function.

Terrified Teacher

Considering Ted's strong emphasis on good teaching and the fact that he was a superb writer and editor, it is reasonable to suppose that Ted was a master teacher. Unfortunately, for reasons I do not know, Ted had a disabling phobia of giving a public lecture. He never lectured to the medical students but occasionally felt obliged to give a public lecture in the Neurological Study Unit on topics of his expertise. When he got up in front of the audience he appeared completely terrified and agitated; he would shift rapidly back from one foot to the other, start talking, stop and then start over again. He seldom finished a sentence without hesitations and repetitions. Simply put, he was agitated and jittery. I

found his mannerisms so distracting that I had a hard time listening to what he was trying to say. After the second time I heard him speak, I resolved that the next time I would concentrate only on what he was saying and try to block out what he was doing. I was pleased to discover what I should have known in advance: that his talk was beautifully organized and packed with useful, information. It is regrettable that he was not able to pass on his keen insights vocally as well as in his writings but this did not prevent him from reaching his goal of building a strong and durable department in a timely fashion.

Turgid Prose

John F Fulton of Yale University edited at least the 15th, 16th and 17th editions of the venerable "Howell's Text book of Physiology". Ted Ruch was the co-editor of the 18th Edition scheduled for publication in 1960. Ted asked me to write chapters on the nature of the resting and action potentials of nerve cells. I was thrilled with this opportunity because the classical Hodgkin-Huxley (HH) quantitative theory of the generation and conduction of the nerve action potential came out in 1952 but had not yet made it into physiology textbooks. Their papers provided one of the major keys to unlocking the functions of the nervous system primarily by showing that the excitable properties of nerve cells arise from membrane structures now called voltage gated ion channels. I quickly found out that expounding HH at a level comprehensible to graduate and medical students is difficult. There are quite a few new concepts that need clear and concise explanation. This was difficult challenge and I well remember taking rough drafts of parts of the chapter to Maryeva Terry Wilcox -- the editor that Ted hired to help with the book -- for her evaluation. She usually rejected what I had written, telling me what was wrong with it but I didn't get it. Finally, the Professor (Ted) called me in and we discussed my writing. He spoke of paragraph after paragraph of "turgid

prose”. I recognized the validity of the description but was not sure what to do about it. Shortly thereafter, Maryeva took the trouble to rewrite two pages of my “turgid prose” and shoved it under my nose to read. It was instantly evident that her version was very good and comparing it with what I wrote showed me what I was doing incorrectly. The difference was of course that she wrote short, simple subject-verb-object sentences and I wrote long, convoluted sentences with subject and object often transposed. After that things went quite smoothly and I had learned a valuable lesson. It was Ted’s eloquent “turgid prose” sound bite that got me on the right track. Yes indeed, Ted was an excellent writer and editor. At some later date, I glanced through my PhD thesis and found that the writing was quite acceptable and recognized that my subsequent Seattle problem was an insufficient feel for the logical order of presentation of the concepts of the HH theory .

The Vulgarians

One day when a few of us were chatting with The Professor in the hallway outside his office, he forcefully expressed his disdain, dislike and disapproval of the “vulgarians” among the medical school faculty. I remember this because it is the only time I ever heard him express his feelings so emphatically. The word itself tells a lot but not having heard it previously, I asked him what exactly he meant. I gathered from his answer, which was somewhat indirect as usual, that he was referring to the kinds of men who stand around at receptions, meetings and restrooms swapping crude anal and sexual jokes. I am quite sure this was a not so subtle criticism of the Dean of the Medical School at that time who was an unselfconscious “vulgarian”. This is the only overt clue I ever got to his core values and standards and I liked what I heard.

Ruch’s Modus Operandi

This section is highly speculative and the reader should take it for what it is worth. Ted was innately shy, mild mannered, reserved and not given to small talk. Based on these characteristics he developed an operational strategy based on appearing to be somewhat harmless, helpless and inept. This lulled his competitors while he quietly and masterfully operated behind the scenes to reach his goals. This is, of course, a gross simplification and exaggeration but I think it captures some of his sterling qualities. Despite all his moaning and groaning about lack of space (a standard ploy of all administrators, in academe, business and government), the facts speak for themselves: Ted Ruch built up a world class department of physiology and biophysics in about 15 years which has persisted to this day, about 60 years later. His final achievement as chair was his implementation of a highly successful Physiology-Psychology Training program. Finally, when he had achieved his goals for the department, he applied for and won one of four NIH funded Regional Primate Research Centers.

Finale: “You’re Not Talking about Me”

When Ted resigned as chairman of PBio to head up his Primate Research Center in 1964, the department organized a farewell banquet for him at a swank downtown club. The after-dinner program was series of paeans of praise given by senior faculty about Ted’s very considerable accomplishments. I have a few, trivial memories of the occasion. My assignment was to give some hopefully amusing introductory comments about Ted’s “retirement” (he was in his sixties at the time). I started out with an agonizing pun -- attributed to Disraeli, a famous Victorian Prime Minister, “The world is well advanced in its anecdotage.” Since Ted was not really retiring, just switching to a different track, I proposed that he was getting a new set of tires for traversing his new path and therefore “re-

tiring”, not quitting. After the paeans of praise petered out, Pat produced a prodigious pronouncement that we had nominated Theodore Cedric Ruch for the National Medal of Science. The President of the United States presents the medal, awarded to about ten prominent US scientists every year. It was fairly new at the time. Ted seemed both stunned and skeptical at this announcement and, putting aside his self-deprecatory manner, stated rather forcefully, “You’re not talking about me!”

It was a sincere and handsome tribute to Ted but I doubt that anyone, Ted included, thought he had much of a chance of receiving the Medal. He didn’t but he was a **great** chairman and a good, decent, kind human being who made a significant contribution to the betterment of the world. I honored admired and really liked him. Today I would say that I loved him but you didn’t say or even admit such thoughts to yourself in those days.

**I praise now professor Ted Ruch,
He always knew just what to do
He steered through the shoals
And reached all his goals.
He’s quiet, resourceful and true.**

HARRY DICKSON PATTON **Harry Patton, Everybody’s Friend and Friend to Everybody**

Pat was happy, friendly caring, outgoing genuine, hardworking and insightful. He liked everyone and everyone liked him. This was my initial impression of him when he gave me a tour of the department in the spring of 1950 and further experience confirmed and expanded this feeling. We quickly became good friends and conversed a few times a week generally about departmental affairs throughout my twenty two years in Seattle. When he succeeded Ted as chair, our conversations as often as not turned into consultations about the details of departmental operations. Of course he also consulted

others.

Pat always called me “J Wally Wally” and the thought of it gives me a warm, feeling, indicative of the bond of friendship between us.



HARRY D. PATTON, MD

Poking Around in the Spinal Cord I don’t remember any details but I do know that soon after I arrived in Seattle in the autumn of 1950, Pat and I agreed to collaborate on a study of spinal cord electrophysiology using intracellularly placed glass micropipette electrodes to study the properties of the various types of nerve cells. We were well qualified to undertake these experiments. He was a meticulous, experienced animal surgeon and I knew how to make electrodes, stimulate nerves and interpret the observed waveforms. We also enjoyed each other’s company and had a ball doing these experiments. We did our first experiments in the summer of 1951 and quickly obtained excellent recordings from three types of cells: Spontaneously firing primary afferent stretch receptor neurons, interneurons that fired bursts in response to dorsal root stimulation and motoneu-

rones with large synaptic potentials. We obtained these important observations at the same time that John C. Eccles was doing similar experiments in New Zealand that helped earn him a share of the Nobel Prize in 1963 along with Hodgkin and Huxley.

In the late spring of 1952, Pat received an invitation to present our results in the form of an “invited discussion” of Eccles results at a Cold Spring Harbor Symposium on “The Neuron” held in early June, only a few weeks later. Pat insisted that I go to the symposium and we worked feverishly to do some more experiments, make figures and outline our presentation. When I presented our results, it was evident that they were at least on par with those of Eccles so the sponsors asked us to submit a manuscript describing our results but within only ten days after the end of the symposium! The result was a seminal paper with unpolished figures and minimal description and discussion. Still it was good that we got that paper published because, for reasons too complex to detail here, Pat and I jointly agreed to discontinue our spinal cord studies, thereby foregoing an excellent opportunity.

Laid Back and Folksy

Pat was an excellent teacher. He had a relaxed, laid-back, folksy style, flavored with Arkanian sayings and mannerisms; but he laid out the essentials of reflex functioning clearly and concisely. I still remember with a chuckle his use of the term “higgledy-piggledy” to denote situations where things were jumbled such as the interconnections between the nerve cells in the spinal cord. He also used the word “bejabbers” some but I don’t remember the context. His occasional use of both these descriptors in his lectures added color and interest. Pat was also an excellent writer and editor as shown by the fact that Ted Ruch named him as co-editor for the 19th and 20th editions of the “Ruch-Patton” textbooks.

Making the Grades

Pat and I were very much into teaching and we gradually assumed most of the responsibility for the overall operation of the Human Physiology course. It seems a trivial thing but I remember with pleasure the many times that Pat and I got together at the end of the course to assemble the materials necessary for assigning final grades to each student. We compiled and averaged all the mid-term test scores for each student, combined them with final exam scores and then entered the results into an “advanced” mechanical desk calculator that automatically calculated the mean and standard deviation! We plotted the frequency distribution of the scores and used this to set tentative boundaries for each letter grade and then briefly discussed the borderline students. The final grades were set in a subsequent meeting of the teaching faculty. Both of us thoroughly enjoyed this annual ritual. Indeed, those were the good old days!

Thursday Noon Togetherness.

For many years we held a faculty meeting every Thursday noon in a small conference room adjacent to the cafeteria. Usually, the meeting consisted of the Chair making an assignment or a few announcements that required no action. At the end of each quarter, particularly at the end of a school year we discussed the progress – or lack thereof – of the numerous (up to thirty or more) graduate students. Oftentimes these meetings were long and agonizing. There was no problem with most of the students who did well to exceptionally well in their coursework and we quickly approved them. Almost always, though, there were a few students whose academic performances were marginal and long discussions were required to decide their fate. We usually let them continue until a retain/dismiss decision had to be made when they had completed their required coursework. I remember more than one occasion where we agreed that a marginal student could proceed if a faculty member was

willing to sponsor/mentor the student. I reluctantly accepted one such student and much to my pleased surprise and relief he turned out to be a first class experimentalist. With almost no help from me, he designed and executed an excellent thesis project and then successfully supported himself on research grants throughout a long and successful research track academic career.

The bottom line is that we ran a highly successful graduate training program, in large part, because we really cared about the welfare of the graduate students -- and the postdocs. We attracted excellent students and topnotch faculty mentored them. The result was that PBio was demonstrably the most productive department in the medical school in terms of total research grant awards, in graduate and post-doctoral training and in teaching load. Ted and Pat frequently bemoaned the fact that these notable successes did not persuade the administrators to allocate to us any additional funds or space.

It's Easy to Quit Smoking

In those "good old days" back in the fifties and sixties a majority of the population smoked: Ted Ruch, Al Young and Pat smoked cigarettes; Al Scher, Arnie Towe, Bob Rushmer and I didn't smoke. I was very well aware of Pat's smoking addiction because we were good friends and for several years we shared a hotel room at the annual FASEB meetings in Atlantic City, NJ. The very first thing he did when he woke up in the morning, even before getting out of bed, was to light up a cigarette. This was a striking demonstration to me of the addictive power of nicotine and I was not surprised when I found out many years later that nicotine is the most addictive of known substances. Not too many years later it became fashionable and desirable for health reasons to quit smoking and I thought that Pat would have a hard time to quit if/when he tried. It turned out to be easy for easy-going Pat. Somewhere in this period he

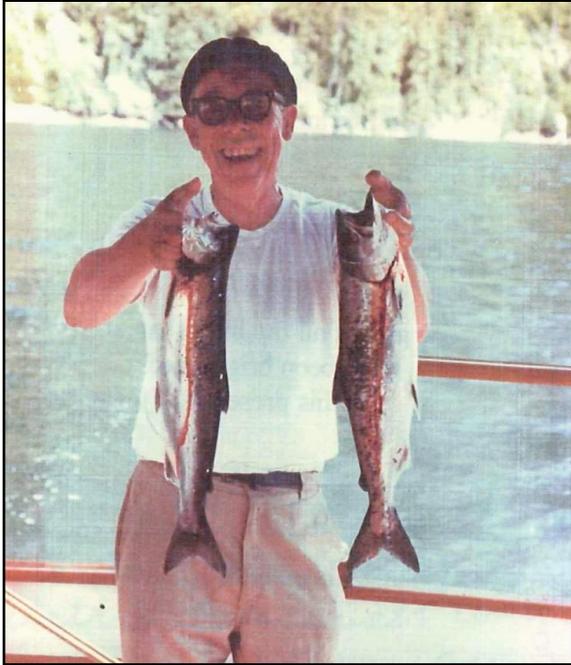
contracted influenza and was sick in bed for nearly two weeks. Of course he was unable to smoke during this period and to his (and my) pleased surprise he did not feel any urge to resume smoking after he recovered. That happy event likely added several years to his life.

Pat's Patented PBio Picnic Productions

When Pat retired as chairman in 1983, I was invited to give a short paper at the symposium in his honor. I barely remember the subject of my presentation but I do vividly remember a tribute to Pat given by one of the newer faculty members. I did not know him nor do I remember his name but he gave an inspired tribute to Pat's role in PBio picnics. He passed over the usual frolicking and feasting as routine and then waxed lyrical about the magical moment when Pat picked up his guitar and started singing. He sang old folk songs, sad, happy, funny and dramatic: "The Golden Vanity", "Oh Dear What Can the Matter Be", "Casey Jones", "John Henry", "Rye Whiskey", "Frankie and Johnny" and many others. It had been nearly fifteen years since I had attended a Bio picnic and the vivid tribute brought up many pleasurable memories of Pat's singing long ago.

The Arkansas Traveler

Sometime in the late 1960s or early 1970s, I got a letter from the University of Arkansas inquiring if I thought Pat was a good candidate for an Honorary degree, from his Alma Mater. I thought this an excellent idea and wrote a detailed, enthusiastic letter of recommendation. I'm sure that Ted Ruch and others got similar letters of inquiry and wrote similar letters. The upshot was that he was indeed awarded an Honorary Degree. He complained that he didn't deserve and didn't want to go to Arkansas to get it but I am satisfied that he was secretly pleased. I know that he fully deserved it.



ONE HAPPY FELLA

Farewell to Pat

I didn't get back to Seattle as often as I intended after we moved to Salt Lake, perhaps a half dozen times, but it was always a pleasure to visit with Pat and the others when I did. The festivities in connection with Pat's retirement in May 1983 were of course the best of all. The outpouring of love and respect for Pat was a pleasure to participate in. I was shocked and saddened when Bertil Hille called me in early June of 2002 to tell me of Pat's death and of the Memorial Service for him. I did not hesitate to tell Bertil, "I loved that man and I must come." When I got there Barbara Patton honored me with a request to give a one minute tribute to Pat, ending with a limerick. Here is an updated and slightly expanded version of my remarks:

Pat's happy, generous, caring nature nourished the relaxed, easy friendship we had for each other. His creative talents extended to writing amusing, clever and insightful limericks and these sometimes stimulated me to respond in kind. For ex-

ample, one sunny spring morning fifty or so years ago I came late to work and found the following limerick written on the small blackboard in my office. There was no signature but the delightful physiological wit left no doubt as to the author. Unfortunately, grasping why this limerick is funny requires fairly specialized knowledge of how nerves generate and conduct impulses. At the very least, the rhythm and rhyme are pleasing.

**Don't censure sluggish old Walt;
His axons are chockfull of salt.
His sodium pump
Can't clean out the sump
And cathodal block* ain't his fault.**

No clever limerick should remain unsurpassed and I struggled in vain to do so. My best was none too snappy and deserves oblivion but here it's anyway:

**At writing fun lim'ricks old Pat
'S an expert, he should be at that;
His single cortical unit --
He's unable to tune it --
Turns out lim-ricks rat-a-tat-tat!**

When the Saints Go Marching Out

The memorial service was a love fest; it was clearly evident that everyone loved Pat! His instruction for the ending of the Service were pure Pat: We all "marched" out of the chapel, to the strains of "When the Saints go Marching In" played very loudly on a trumpet with piano accompaniment! It was not a typical end to a memorial service but then Pat was a special guy. For me, it captured the essence of Pat and I enjoyed every second of the march, chuckling all the way at this voice from the Pat. The trumpeter was an old friend of Pat's who flew in from a large city in the Midwest (St Louis?). I think he

*Blockage of nerve impulse conduction that occurs when a nerve cell runs out of fuel.

was a professional trumpeter and very good.

My final tribute to Pat says it all for me:

It's sad that no more we'll be seeing

Our Pat, a sweet kind human being

I state clearly that

I love you dear Pat.

I'm sure that all here are agreeing.

I should have added: "Farewell, dear friend from J Wally Wally.

MEMORIES OF THE OLD

TIMERS

Loren Carlson.

I liked but had few interactions with Loren. He was calm, competent and gave well organized, relevant and interesting lectures. He was an insightful, productive applied physiologist who was a leader in his field. My favorite memory is the way he ended his lecture on weight regulation. He gave a vivid description of a strenuous workout intended to produce a loss in weight. He would then point out that a single candy bar eaten afterward more than made up for the calories burned in the workout. This illustrates how really hard it is to lose weight!

Julia Skahen

Julia taught physiology to the students of nursing quietly, faithfully and effectively for many, many years thereby fulfilling an essential service obligation of the department. My impression is of strong, quiet, caring competence.

Vahe Amassian

Vahe was recruited from England a year or two before Allen Scher and I arrived in the autumn of 1950. Ted assigned the three of us to teach the physiology course for dental students. Shortly after we moved to Seattle, Vahe bought a new Oldsmobile convertible with an automatic transmission, a rarity in those days, and offered to take Al Scher and me for a ride. It was a

mild, rainless autumn day with nary a cloud in the sky and it was great fun to ride about in an open car enjoying the grand weather and the view which was all new to me at that time. Later on Vahe provided me with my first experiences with British pronunciation. I found out that the letter "Z" is pronounced "Zed" not "Zee" and that in England, transmission of the nerve impulse in myelinated nerve fibers is "sal tat'-ory" whereas in the USA transmission is "sal'-ta-tory "

Robert Rushmer

Bob was bright, pleasant, eager, enthusiastic and ambitious. He pioneered the field of recording cardiovascular-respiratory parameters such as heart rate and blood pressure in awake, normal dogs at rest and during exercise. It is my impression that some of his findings were surprising, requiring modification of the conventional wisdom but I don't remember what. Rushmer later separated from PBio and helped define the field of bioengineering. He later collaborated with Orville Smith in seminal studies of cardiovascular regulation in behaving monkeys but I don't know the details.

Allen Scher



Allen M. Scher, PhD

Al and I arrived in Seattle at about the same time in mid September 1950. We worked together harmoniously for several years teaching the dental physiology course. He collaborated with Allan Young to obtain a detailed description of the spread of excitation through the dog heart. This seminal work provided one half of the information essential for developing a quantitative description of the surface electrocardiogram. Al was invited to summarize these findings in a chapter "Excitation of the Heart" in the 1962 edition of the Handbook of Physiology, Section 2: Circulation, an official publication of the American Physiological Society. One of my memories of Al makes me smile. At random moments he would rattle off a mildly mocking phrase about the jargon of the field of electronics: 'Ding-dong, click-clack, flip-flop, bang-bang, cathode follower'. Believe it or not, the last three are descriptive names for useful electronic circuits.

**Those great early days with Al Scher,
When nothing our dreams could deter.
I remember when Allen
And I were a-palin' –
If things could be still as they were!**

Allan Young

Al made seminal studies of the dynamics of respiratory gas exchange in the various lobes of the lung. I think he was one of the first to measure the variation of the ventilation/perfusion ratio with height in the lung. This type of information had immediate clinical applicability. He also invented a "volume clamp" for measuring the purely mechanical properties of the lung in intact animals. For two or three years in the mid-1950s, Al and I met two times a week to work through mathematical analyses of various types of non-linear oscillators; we both learned much and found pleasure in the process.

Arnold Towe

Arnie pioneered extracellular recording of single cells in the sensory cortex of cats and deduced the probable circuitry.. He was quiet, pleasant polite and thoughtful. Many years ago a monkey escaped from the Primate Center and trashed Arnie's office. The scope of the disaster is indicated by Philip Bard's definition of a monkey as "an evil spirit smeared with feces." Bard was a prominent mid-century neurophysiologist,



ARNOLD L. TOWE, PhD

**This Careful Researcher, A. Towe
Through Cortical Units Did Plow
And When Good Old Arnie --
Without Any Blarney --
Deciphered Their Code, He Said, "Wow!"**

I have tried to indicate the seminal scientific and other contributions of the original faculty members and something about their personal characteristics. Unfortunately this is all I remember about them that is worth repeating. I regret that it is so little when so much more needs saying. Essays on later faculty are needed but beyond the scope of this addendum

NOTE: Attached at the end are several photographs that I took at the Memorial Services for Harry D. Patton on 8 June 2002. The first one is of a group of "Pbio: Old and Not So Old Timers.", Loring Rowell, Allen Scher, Orville Smith, Walt Woodbury, Arnold Towe, George Brengelmann, Marjorie Anderson and Bertil Hille.

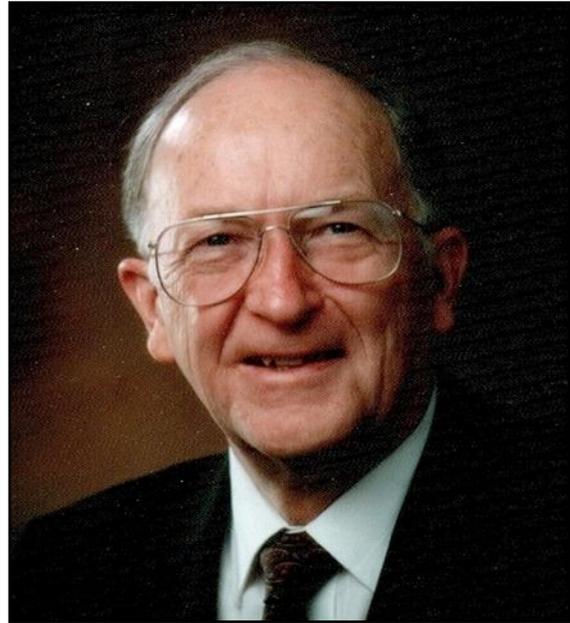
Thelma Kennedy

I need to mention Temy because of her central role in the success of the graduate program. **Temy** joined the faculty in 1958 after doing a postdoc under Pat. She immediately took charge of our rapidly growing Graduate Training Program. A large room full of cubicles and desks housed most of the graduate students and was, of course called the Bull Pen. Equally obviously, Temy was the Pen Mother. Temy was well qualified for this responsibility by temperament, talent, training and desire. She cared deeply about people was a good listener and advisor and had a sympathetic ear for the problems of the students both personal and academic. She kept morale high and undoubtedly persuaded wavering students to continue the pursuit of their goals. There must be a lot of stories about the her good deeds and it is a shame that some were not recorded. She was a good, happy person who fitted in well and was a great asset to the department.

J. WALTER WOODBURY

Research

1950-1972: Most of my experimental research used the technique of measuring transmembrane potentials of single cells using glass micropipette electrodes inserted into the cell's interior. I have published papers on the resting and/or action potentials from cells in the hearts of several species of vertebrate animals; from striated and smooth muscle; from neurons in spinal cord and sensory cortex of cat; and from thyroid and liver cells of rat.



J. WALTER WOODBURY, PhD

1952: Woodbury & Patton: As mentioned above, I gave a paper at a Cold Spring Harbor Symposium on the Neuron describing our intracellular recordings from single neurons in the spinal cord. Of particular importance were the first recordings of burst firing of interneurons in response to a stimulus to a dorsal root. Even today, intracellular recording from interneurons is not routine.

1953- 1954: Discovered that a nerve fiber can be excited by a passing a current through a microelectrode impaled in the fiber, opening up one method of stimulating and recording from the same cell. Shortly before that, Eccles and colleagues did the same thing using a double-barreled electrode, one barrel for stimulating and one for recording. The two methods complement each other.



WAYNE E. CRILL, MD

1956 Invented the flexibly mounted micropipette electrode for intracellular recording of resting and action potentials from moving tissues such as heart and smooth muscle.

1961 Woodbury and Crill showed that there are low resistance electrical connections between adjacent cardiac muscle cells. This discovery led to the isolation and characterization of the connexins, a class of normally open ion channels that reside in the membranes of two adjacent cells thus providing a path for current flow.

1964 Showed how to use micropipettes to measure pressures in capillaries.

1970: Showed how Eyring rate theory can account for the puzzling shapes of current-voltage relationships of ion channels and for the gating characteristics of voltage gated ion channels.

Teaching

How the Hodgkin-Huxley Theory Became My Theme. Soon after moving to Seattle Ted Ruch assigned Al Scher, Vahe Amassian and me to teach the dental physiology course. Somewhat to my surprise, I found that I liked teaching and have an aptitude for it. Indeed, teaching, training and mentoring occupied a substantial fraction of my time and effort throughout my academic career. I loved to teach but most of all I loved teaching the Hodgkin-Huxley theory of nerve action potential generation and propagation; it was always my number one teaching priority.

I first met Hodgkin at the 1952 Cold Spring Harbor symposium on “The Neuron” described above. Hodgkin gave a brilliant summary of **the** five Hodgkin-Huxley Nobel Prize papers which were in press at that time. I was indelibly impressed with the elegant simplicity and incredible explanatory power of their theory. It easily explained a wide range of puzzling phenomena. Their theory was an order of magnitude jump in our knowledge of nerve function and laid a rock-solid foundation for the elucidation of the functions of the entire nervous system.

Unfortunately, the theory contains quite a few difficult new concepts and full understanding of the theory requires a grasp of non-linear partial differential equations, putting it out of reach of most neuroscientists. Since I have a BS in physics, I was able to master the HH concepts and was thus in a good position to develop simplified approaches for explaining their theory to the neuroscience community. As explained below, it turned out that I played a predominant role in spreading HH abroad. This was one of my most important contributions to neuroscience.

The difficulty of the HH papers is illustrated by an annual ritual indulged in during the 1950s by a fellow nerve biophysicist, Gordon M. Schoepfli, of the physiology department at Washington University, St Louis and me. When we met at the annual FASEB meetings in Atlan-

tic City, NJ every April, we counted the number of scientists in the United States who understood HH. At first the fingers of one hand were sufficient but it took two hands near the end of the decade. This slow growth shows the urgent need for more accessible versions of the Hodgkin-Huxley, sodium-potassium theory of nerve action potential generation

Advanced Membranes Course (3 cr.) I thoroughly studied the HH papers as soon as Hodgkin sent me reprints in the autumn of 1952 and was eager to spread the word. Thus when Allan Brady – my first graduate student -- showed up in about 1953, I guided him through the HH papers. This process soon developed into a formal course consisting of a thorough, detailed study of the HH papers under my tutelage. I taught it at least once each year thereafter through 1972 and about ten times after I moved to Utah. Enrollment was typically 3 with a range of 1 to 6 students. After a few years, we formalized it by giving it a number and listed it in the course catalog. I never got tired of teaching this class.

Baby Membranes Course (3 cr.) Temy Kennedy named this course which was a simplified, qualitative version of the HH theory. It took much time and effort to develop effective ways of presenting the difficult underlying concepts and its popularity grew with the clarity of my explanations and the number of graduate students. My recollection is that this course was required of all PBio graduate students and many other students also took it. Enrollment was typically 8 or so but I can remember at least two years when the enrollment was more than twenty, perhaps thirty. I taught this many times in many places and with greater or lesser detail. Like Advanced Membranes, I never tired of teaching this course.

Control Systems Course Master. In the late 1960s I was a member the School of Medi-

cine committee that converted the curriculum to the organ systems approach. My “reward” was appointment as the Course Master for the newly created “Control Systems” course which was mainly about the physiology, biochemistry and pharmacology of nerve, muscle, synapse and reflex. I continued in this position until I moved to Utah but I was more than ready to move on to other teaching duties.

Mentoring and Training

Graduate Students: I mentored 12 graduate students who completed graduate degrees, 10 PhDs, 1 PhC and one MS. I include Wayne Crill, MD because he earned the equivalent of a PhD. I mentored him as if he were a graduate student; he took a year off from to Medical School to work in my lab and of course his research was superb. At least seven of my PhD students went into academia. Innumerable medical and other students spent their summers in my lab.

Post-Doctoral Fellows. Rather to my surprise, fifteen post-doctoral fellows, went through my lab. Three of these, Albert Gordon, John Conrad and Ted Kehl stayed in PBio.

Teaching Materials: Spreading HH Abroad

The Textbook, 1960 The audience for my Hodgkin-Huxley spiel went worldwide in 1960 with the publication of “**Medical Physiology and Biophysics**”, edited by T. C. Ruch and John F. Fulton. At Ted Ruch’s invitation I wrote a two chapter exposition of the HH theory. This was the first appearance of HH in a textbook which quickly became a best seller. This was in part due to the HH section because in 1960-61, I got letters from Sir Bernard Katz, a 1970 Nobel Prize awardee and from Stephen Kuffler of Harvard (who should have shared David Hubel's and Torsten Weisel's half of a 1981 Nobel Prize)

thanking me for my “much needed” presentation of the Hodgkin-Huxley theory.

My chapters provided a model of how to present HH theory with a minimum of mathematics and was bound to be copied. For example, in 1963, I edited Ernst Florey’s version of membrane biophysics for his text book of general physiology. This general approach was soon a standard part of all textbooks of physiology.

I have good personal evidence that the textbook was in wide use. Over the years, particularly at national meetings, somewhere around 50 neuroscientists approached me to let me know that they had had the painful pleasure of struggling through my HH chapters. They acted like it was a badge of achievement and it was.

H and H AGAIN and AGAIN. In 1962 I was asked to write a chapter on **Cardiac Electrophysiology** for the Handbook of Physiology, Section 2, Circulation, Volume 1, an official publication of the American Physiology Society. (My chapter preceded Al Scher’s chapter on the excitation of the heart.) I accepted with the proviso that I start with an exposition of HH because the chapter on nerve physiology in Section 1, Neurophysiology, barely mentions HH.

In 1969 I wrote an HH chapter “Biophysics of Nerve Membrane) for a book, “The Basic Mechanisms of the Epilepsies”.

Principles of Body Acid-Base Regulation In the course of writing a chapter on acid base regulation in humans for the 1965 edition of the Ruch-Patton textbook, I found and corrected a significant conceptual error in the well established and generally accepted theory of diagnosis and treatment of acid-base unbalances. Fortunately, this error had negligible clinical consequences but caused a fair amount of confusion. It also led me to some new insights that facilitated diagnosis and treatment. I also added a new section that extended the same principles of acid-base regulation to the fluid inside cells. The

timing was fortuitous because research on the determinants of intracellular pH was expanding rapidly at that time and my chapter became the standard reference source for both new and old researchers in this field.

Nominating Hodgkin and Huxley for the Nobel Prize

In late 1962 or early 1963, I received a letter from the Nobel-Committee of the Royal Institute, Stockholm inviting me if I wished to make a nomination for the Nobel Prize. Of course I interpreted this to mean that they wanted me to nominate Hodgkin and Huxley for the Nobel. I was thrilled at the invitation because I thought that they were long overdue for the Prize. The subsequent events are summarized in the last paragraph of a cover letter, dated 28 January 1963, that I sent to the Nobel-Committee nominating H and H.

“Recognizing the similar envelopes received by some colleagues in this department, I realized that they too had been asked to make a nomination for the Nobel Prize. It has long been felt by my colleagues and me that Hodgkin and Huxley merited the award. Consequently, it was agreed that I should prepare a nomination, and that they would make short nominations in a separate letter and sign the more detailed presentation enclosed.”

The nominating letter consisted of 9.5 pages of text and 2 pages of references and was signed by J. Walter Woodbury, Theodore C. Ruch, Harry D. Patton, Allan C. Young and Allen M. Scher. I have no way of knowing but feel reasonably sure that our letter was a fairly large factor in the successful nomination. Hodgkin and Huxley shared the 1963 Prize for Physiology or Medicine with John C. Eccles. It was not until 40 years later that I found out that nominations for the Nobel are by invitation only. Thus the very fact of the invitation to make a nomination shows that the committee was

seriously considering HH and invited me because I was internationally known as the first and only person who had put the Hodgkin and Huxley theory into a text book and thus promoted its wide acceptance..

LINC to the Future: 1963 - 1971

In 1963 NIH funded the Laboratory Instrument Computer (LINC) evaluation program. The principle aim was to demonstrate that digital computers can facilitate experimental neurophysiological research. In the LINC, built in A/D converters digitize and store the data as it is generated according to a protocol stored in the computer. The investigator programs the protocol in simple machine language instructions. I applied for and received one of the 24 LINC's to evaluate in my laboratory. I asked Albert Gordon who was my post-doc at the time to be my co-investigator and in August, 1963 we spent a very busy month in Cambridge, Massachusetts assembling the LINC and learning the principles of operation, programming and repair.

It was my policy from the beginning to make the LINC available to students, post docs and faculty and it quickly became immensely popular, particularly with graduate students. William H. Calvin made the first entry in the LINC log-book on 2 June 1964, He used it for four hours, presumably learning how to program the LINC. I made the last entry on 9 March 1971 when I shut the LINC down for repairs and a major upgrade. During that seven year span, there were 3,174 entries in the log book made by 107 people with a mean usage time of 2.7 hours per entry. The most frequent users were graduate students. Other frequent users were maintenance techs, post docs and faculty. The biggest user was one of my graduate students, William L. Hardy. He used the LINC to collect the data for his thesis research. In the 15 month interval from June 1966 through October 1969 Bill used the LINC an average of 5 hours per entry a whopping 537 times! The grand total is 2683

hours, that is 112 days! Thus he used the LINC for 3.6 months in a 15 month period.

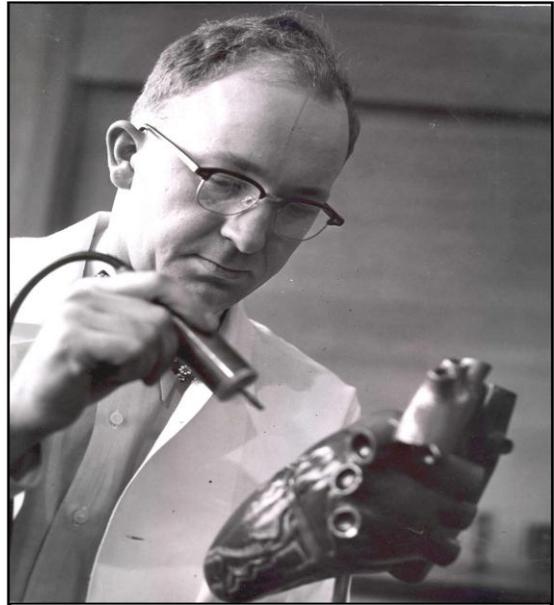
This statistic illustrates the huge role the LINC played in our graduate training programs. It introduced them to the basic concepts of computer operation and programming and was a powerful tool for obtaining and analyzing data.. It was simple, useful, user friendly and easy to program in machine language. Indeed, for a few years, there was a required hands-on graduate course on using the LINC. A workable program for performing some useful function was required for a passing grade. Our students got a jump on the computer revolution because of the availability of the LINC. A short time later, a departmental computer was available for doing serious data analysis but practicing on the LINC was an excellent way to learn the basics of computer use.

Public Recognition

1957 Intracellular Recording From Human Ventricle. John Lee, a medical student who worked in my lab for several summers urged me and Al Brady to try to record from a human heart at open heart surgery using a floating electrode attached to a hand held probe. It took considerable effort to design an input probe that could be sterilized in an autoclave. K Alvin Merendino MD gave us one chance to try during open heart surgery and against all my expectations John Lee succeeded in recoding an intracellular action potential from a cell in the left ventricle of a human with Al Brady and me at the controls. I thought that this feat was newsworthy and told the UW PR people. The outcome was a picture and article in the Seattle Times and a one minute appearance on KING-TV's morning news show. I demonstrated how we did it by holding the probe near a full sized color model of the human heart as shown in the picture.

1971 Organized the Membrane Biophysics Group. In 1970, the Biophysical Society authorized the formation of specialty groups upon presentation of a petition from at least five percent of the membership. A few months later, I thought it desirable to form a Membrane Biophysics Group. It was easy to collect more than the necessary 200 signatures. The Biophysical Society Council approved our application and the proposed By-laws in February 1971. I chaired the first Group meeting, held in February 1972 and persuaded K. S. Cole to make a substantial donation to establish the annual K. S. Cole Award. The Group has prospered for nearly 40 years and there are now several Groups that have split off from the original Group.

**This puttery professor named Walt
Has rhythmical limerickal fault
His head's a hot store
Of lim-ricks galore
Hence no hair on his cranial vault**



J. W. Woodbury holding probe used to record an action potential from a single cell in the human heart, 1957

FAREWELL TO PBIO

I loved PBio and the people in it, the University of Washington, Seattle and the State of Washington. I enjoyed every minute of my stay there and was extremely well treated. In turn I performed to the best of my ability and was grateful everyday that I earned a good living doing what I wanted to do. My decision to move to the University of Utah was for purely personal reasons and it was the most difficult decision I ever made. I treasure my memories of the 22 happy years I spent in the Department of Physiology and Biophysics, School of Medicine, University of Washington. I am proud that I was a member of the team that built up a great department and greatly impressed and pleased that it has continued to grow and prosper since then.

J. Walter Woodbury,
Salt Lake City, UT
7 Aug 2010

Acknowledgement

I thank Douglas G. Stuart, PhD, Regents' Professor Emeritus of Physiology, University of Arizona, Tucson AZ for editing this work and for our many conversations and email interactions about it. ,

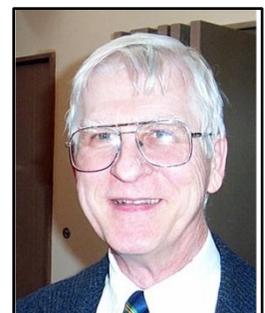
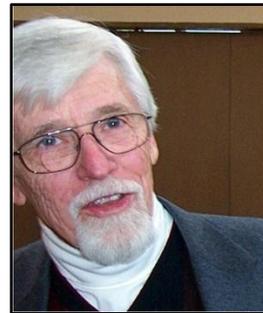
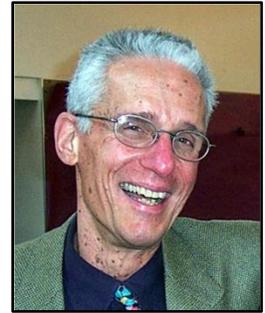
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Front l to r. Loring Rowell, Allen Scher, Orville Smith, Walter Woodbury, Arnold Towe. Back. George Brengelmann, Marjorie Anderson, Bertil Hille.



Walt and four graduate students: l to r: Michael C. Mackey, Milan J. Heath, J. Walter Woodbury, Steven H. White, William L Hardy. Home of JWW 19 Dec 1968



L to R alphabetical: **2nd row:**Marge Anderson, George Brengelmann, Stan Froener, Al Fuchs. **3rd row:** Ted Kehl, Barb Patton, Orv Smith, Chuck Sterling.

Right: Photo of the J. Walter Woodbury Memorial Window.at the northwest corner of 4th floor of the H-wing Bob Godt put the label there to commemorate the many hours I stood at this window looking at the construction of the T-Wing. The image of the outside of the H-wing is a reflection from the windows in the T-Wing.

All except the photo at upper right were taken at the Memorial Services for Pat in Seattle on 8 June2002. Photos by JWW

