American Association of Avian Pathologists
Biographies of Professionals in Poultry Health

Bruce W. Calnek

1932-

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The Life of Bruce Calnek

Early Years: I would never have guessed, as a youngster, that I would become involved in avian medicine as a lifelong career. My birthplace (on January 29, 1932) was a small farmhouse, literally in the shadow of Cumorah Hill (the birthplace of the Mormon religion) between Manchester and Palmyra, New York. I was the second of four children. My father, Otto, had emigrated from Nova Scotia, met and married my mother, Lucile Wixson, who was from the Southern Tier in New York, and they raised their family in central New York. Being of very modest means, my father worked as a farmhand for a while, and then learned the trade of papermaking to provide steady if meager cash income; both of these turned out to be important in determining my future, albeit in less than obvious ways. My first contact with chickens came when I was in 3rd grade and we moved to a small farm on the outskirts of Clifton Springs. We had 52 acres which my father worked in addition to his 7-days/week job in a nearby paper-mill. In addition to a couple each of cows, pigs, and horses, the family raised 100 chickens (mail-order chicks from Sears-Roebuck or Montgomery Ward) for slaughter twice yearly to pay the mortgage, and kept a small flock for eggs which my mother sold for “pin money.” Little did I know that the occasional chicken that died with a big liver, or became paralyzed, would portend the central theme of many years of research.

The fact that my father worked as a farmhand when I was quite young was a determining factor in a strange way. When I was 3 years old, we moved to a farm with a dairy herd. On our first day there, my 5-year-old brother and I were exploring the barn and came across an old cow-clipper in which a crank and a set of gears powered a cable with clippers at the end. I managed to put my left index finger into the gears (in retaliation, because my brother would not give me a turn with

the crank) and so I lost the end of the digit. I now consider that event to have been a stroke of luck, for a very strange reason. It seems that my father was a music lover and an avid violin player. He would have liked for me learn to play the violin, but with a short left index finger it just wasn't going to work. He encouraged me to learn to play the flute because it goes well with a violin.

The second piece of the story is related to my father's need to find a new job, which turned out to be in a paper-mill located in Ithaca, NY. Thus, at the beginning of my junior year in high school, we moved from the farm in Clifton Springs. During a 6-month period before finding a house in Ithaca, we lived in a rental apartment in the village of Trumansburg. I joined the school band and soon made friends with another flutist, Neil Moore. We often played duets in the evening and so I met his family. Another stroke of luck! It seems that Neil's father, Dr. Earl N. Moore, was on the faculty of the Veterinary College at Cornell University where he specialized in diseases of turkeys. When we finally moved to Ithaca, it was to Forest Home, a small residential community near the university. Dr. Moore offered me a job washing glassware Saturday mornings at his research laboratory on Snyder Hill (a of couple miles off campus). Soon I was doing the weekend chores on the research farm, and then he encouraged me to help with minor tasks related to his research on coccidiosis in turkeys. In due course, he suggested that I consider veterinary medical research as a career and the rest is history. Incidentally, the same Earl Moore later served in a US program in India, and was largely responsible for steering Dr. Syed Naqi into a PhD program in the United States, which in turn led to his richly productive research career.

An early introduction to the field of avian medicine (age 16-17) resulted from a job working for Dr. Earl N. Moore at Cornell's Snyder Hill poultry research laboratory

So, I consider (i) the finger loss; (ii) flute playing; (iii) the move to Ithaca via Trumansburg; (iv) meeting Dr. Earl Moore; and (v) working in a poultry disease research lab to be important sequential and interdependent events that resulted in my career choice. At the tender age of sixteen I knew exactly what I wanted to do with my life. How's that for luck?

College Years: In my senior year at Ithaca High School (1949), I applied to Cornell University with an expressed interest in the pre-veterinary medicine program in the College of Agriculture. After acceptance in the Spring, I was invited to join the Cornell Big Red Marching Band, with the enormous thrill of being able to practice with the band even before finishing high

school. It meant two years of playing piccolo at all football games (home and away), basketball games, track, etc., providing a wonderful diversion from studying.

During the two pre-vet years before my acceptance in the Veterinary College, I continued to work in the research laboratory at Snyder Hill, under the supervision of Dr. Moore and his assistant Jesse Brown, who in turn were part of the poultry disease team led by Dr. P. Philip Levine. We worked on turkey coccidiosis and turkey bluecomb, now known as coronaviral enteritis of turkeys, and I was gradually given more and more responsibility in the projects.

In 1951, I entered the College of Veterinary Medicine at Cornell. Probably my expressed interest in poultry diseases was double-edged with regard to my acceptance. Most of the faculty did not view chickens as being a particularly important part of "veterinary medicine." Yet they probably also thought that it did no harm to take an individual with such an interest once in a while to show they weren't prejudiced (much as they accepted a woman every year of two!). Further, I'm sure that Dr. Levine's letter of reference did me no harm.

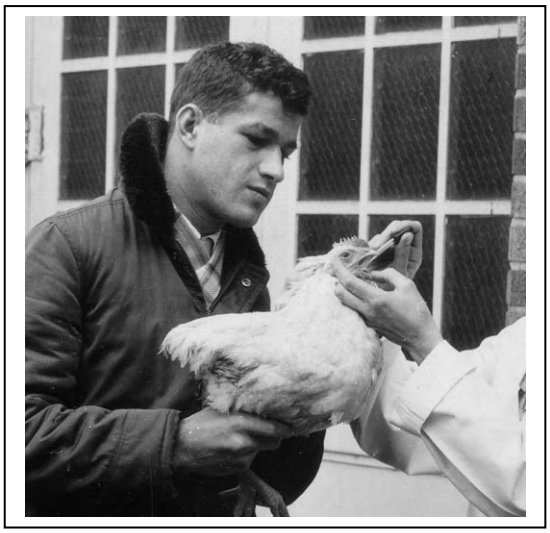
Throughout my 4 years in the College, I spent every possible free moment at the research lab. In 1951, Dr. Moore left for a position at The Ohio State University and he was replaced by Dr. Clyde I. Boyer, Jr., whose charge was to continue studies on diseases of turkeys. Upon his arrival I boldly informed him that he had inherited me as a student technician and so I was allowed to help with further studies on coccidiosis, turkey bluecomb and erysipelas in turkeys.

I had lived at home for my two years in the pre-veterinary medicine curriculum, being less than a mile from the center of the campus. However, as I was ready to enter the Veterinary College, my parents were forced to move again and I was left to fend for myself because they did not have the means to give me any financial help. At that time, the Colleges of Agriculture and Veterinary Medicine at Cornell had no tuition, and fees were very low. Thus, I had only to find my own room and board and pay for books and incidentals. I waited table at a fraternity for a few months, until it interfered with my classes, and earned some money from my job at Snyder Hill. However, it was not enough and half way through my 2nd year I was broke and going hungry all too often. My lucky stars came through again with the offer of a job at the Tompkins County Hospital doing emergency laboratory work nights and weekends, sharing the work with two other veterinary college students. The pay was room and board and twenty dollars per month. I was saved! Together we did all of the hematology, blood chemistries, cross-matches for transfusions, etc. that were required after regular hours. I bought my first car, a real clunker, so that I could work both at the hospital and at the research laboratory on Snyder Hill. Cornell University loaned me the $350 needed for the car, which turned out to be my only debt when I finished my education, and perhaps it was the only used-car loan that Cornell ever made.

Working at the hospital was a marvelous experience with a huge bonus. It was there that I met Mary Jeanne Trudeau, a dietician on the staff. After a courtship that often was simply having evening coffee at the cafeteria of the hospital, we became engaged and subsequently married on July 4, 1955, at the end of my third year in Veterinary College. We both continued working at the hospital, my ability to do so was confirmed when we found an apartment only a couple of blocks away.

While still a student, I was twice given responsibility for running the poultry diagnostic laboratory when Drs. Levine, Fabricant, Peckham and Boyer were attending the Northeast Conference on Avian Diseases (NECAD) meeting in June. This was my first dose of real responsibility, albeit with Dr. James Gillespie looking in at the end of each day. Luckily, being summer time, coccidiosis was the most common diagnostic challenge and my laboratory experience with this disease stood me in good stead.

After receiving the DVM degree in 1955, I entered graduate school with the aim of earning a Master of Science degree. Dr. P. P. Levine was my mentor and my project was on mycoplasma (then known as PPLO) infection in chickens, with particular attention given to egg transmission and viability of the organism under various environmental conditions. Working on an Assistantship meant that 50% of my time was spent helping in the poultry diagnostic laboratory, conducting weekly infectious bronchitis clinics, carrying out serologic tests, *e.g.*, hemagglutination-inhibition tests for Newcastle disease, etc. Also, on one day's notice, I was asked to teach the course on poultry diseases offered to students in the College of Agriculture, largely because Dr. Levine was concerned that the previous instructor had seen enrollment drop to an all-time low of 4 students and he thought the course needed new blood. It must have worked because enrollment went up more than 3-fold for the next year.



Assisting in the weekly infectious bronchitis clinic as part of the duties of the Graduate Assistant (1955)

I completed the requirements for the MS degree in 1956. Dr. Levine wanted me to continue for a PhD but I was itching to get going with my career, having worked in a laboratory gaining experience with research for seven years. He asked me to accept a one-year appointment as an Acting Assistant Professor to fill in for Dr. Julius Fabricant while he was away on sabbatical leave at the University of California in Davis. Thus, I had an additional year of solid research experience with ongoing projects on mycoplasma infection and infectious bronchitis, but it was clear that there were no permanent openings at Cornell so I had to find a job.

Professional Career: Two potential positions were of interest as I was searching for employment. One was at Virginia Polytechnic Institute (VPI, now Virginia Tech), where Drs. Bernie Gross and Charlie Domermuth were carrying out some exciting research. I interviewed for the position and was encouraged by their infectious enthusiasm and creative ideas. Soon

after, I was offered a position as an Associate Professor, much to my amazement. Apparently, I had hit it off with Drs. Gross and Domermuth because many years later, Bernie confided in me that he had instructed the Dean to "hire him before he leaves town." Anyway, as attractive as the offer was, I had to put them off because I had another job to interview for, at the University of Massachusetts (UMass).

The opening at UMass, in Amherst, was for the position that had been held by Roland Winterfield, and before him, Stephen Hitchner. This position had some particular attractions for me: (i) other faculty engaged in poultry disease work included Drs. Henry van Roekel, Glenn Snoeyenbos, Martin Sevoian and Kenneth Bullis, all well known for their contributions to the field; (ii) it seemed to me to be a better springboard for my career because of its reputation; (iii) they had a virus-isolation building for holding experimental chickens and there were excellent laboratory facilities; and (iv), the not trivial matter of being close to family (my parents at that time lived in Westfield, MA, a short distance from Amherst). For all of these reasons, I leaned toward the UMass, but after my interview I did not benefit from an almost immediate offer as I had gotten from VPI. Worse yet, VPI was pushing me for a decision. I tried to get Dr. Bullis, the Department Chair, to decide whether or not to offer me the job. Dr. Levine advised me to write a Western Union "night letter" essentially demanding an answer, and finally Dr. Bullis obliged by offering the position of Associate Professor. This was heady business for a 25-year-old just starting his career. At long last, I was going to have a chance for independent research and I could "try my wings." It was September, 1957 when we moved to Amherst for the beginning of a major chapter in our lives where we spent the next four years and where our two sons were born, David in 1959, and Douglas in 1960.

Research Activities: One of the first tasks when I arrived at Umass was to select a research project. I was told in no uncertain terms that mycoplasma research was off limits, for the obvious reason that Dr. van Roekel and his group were covering that field very adequately. Likewise, they were doing work with infectious bronchitis and Newcastle disease. It was suggested that fowl pox would be a good choice, but for reasons that escape me now, I could not get excited about working on that disease. Instead, I thought that avian encephalomyelitis (AE) might be a good prospect, in part because work in Erwin Jungherr's laboratories at the University of Connecticut had made some breakthroughs, particularly in developing methods for propagating the virus in antibody-free embryonating eggs. However, when I suggested AE as a potential research area, I was informed by Dr. Bullis that it, too, was off-bounds, because "Dr. van Roekel might like to return to that subject sometime in the future." This was a strange excuse for a new recruit unused to the idea of ownership of diseases. Persistence and stubbornness on my part caused Drs. Bullis and Van Roekel to relent and so I was off and running.

AE turned out to be a lucky choice, indeed, given the Connecticut work on embryo-infection techniques. That breakthrough offered viable approaches to new studies on the disease. Luck once again presented itself when it was found that a flock of chickens maintained by Olga Weinack (nee Olesiuk), a colleague of Dr. van Roekel's, was free of AE antibodies and therefore their egg embryos supported the growth of the high-chick-passage van Roekel strain of AE virus. With help from my first assistant, Hubert Jehnich (a veterinarian from Latvia), a serum-neutralization test was quickly developed so that immune responses to infection could be easily monitored. That work resulted in my first truly independent research paper which was published

in *Avian Diseases*. My first research grant (I believe it was $1,500 per year) was provided by Dr. John R. E. Taylor from the DeKalb Agricultural organization, and this paid for the many eggs I needed from the Olesiuk flock. During a four-year period at UMass, with help from Hubert Jehnich, Patricia Taylor and Martin Sevoian, a total of 5 papers on AE were published; the most important of these provided the first description of the pathogenesis of AE virus infection and, very importantly, the development of the natural-strain vaccine which is still in world-wide use. These break-through studies hinged on the use of natural, non-adapted field isolates of AE virus rather than the highly adapted van Roekel strain which was the virus of choice for most previous studies on AE. The latter had lost its ability to grow in the intestinal tract, the normal replication site for field virus. Patricia Taylor earned her MS degree under my supervision (although her committee chair was in another department) with studies on the isolation of a large number of enteric viruses, mostly adenoviruses.

With no responsibilities for anything other than research, my four years at UMass were a great substitute for a PhD, giving me that chance to "try my wings" and provide the springboard I had envisioned. Indeed, the success of my studies there were likely responsible for the offer to return to Cornell that came in 1961. I truly believed that ultimately I would be better off in a College of Veterinary Medicine than in a Department of Veterinary Science at a College of Agriculture and so I jumped at the opportunity. Thus, in the fall of 1961, I accepted Dr. P. P. Levine's offer of an Associate Professorship in the newly established Department of Avian Diseases at Cornell.

The faculty in 1961 consisted of the same people who were there when I had left in 1957: Drs. Levine, Fabricant, Peckham and Boyer. I was given the responsibility of interacting with three branch laboratories in New York State, but otherwise I was expected to spend full time in the research arena. Interestingly, my laboratory was in the same building in which I had gotten my start washing glassware 13 years earlier.

The summer before I started at Cornell, I attended the American Veterinary Medical Association (AVMA) meeting in Detroit. It was there that I heard Dr. Harry Rubin talk about his ground-breaking work on the so-called "RIF" test for the detection of avian leukosis virus. Dr. Rubin, who, incidentally, received his only degree (DVM) from Cornell University, entranced me with his presentation and when I arrived at my new job I knew where the fertile ground for research in avian diseases was going to be. I excitedly told Dr. Levine of my first choice for an area of research, but to my great disappointment he informed me that the funds that were available for research were largely earmarked for mycoplasma studies. Thus my initial projects were in that field. I developed methods for cultivating mycoplasmas in cell cultures and assisted in the ongoing project of using egg-dipping (in Tylosin) for the eradication of infection in breeder flocks.

A few months later, Dr. Levine called me into his office and told me about the very exciting prospects for research on avian leukosis, following Rubin's work (as if I had never said a thing about it before). I kept my mouth shut about my earlier plea and let him convince me that I should enter the field of tumor research. I was ecstatic! He encouraged me to apply for a grant from the National Institutes of Health, which I did. The 3 years of support which were awarded began what was to become a 29-year period of studies supported by successive grants from the

National Cancer Institute. I took on my first *bona fide* graduate student, Robert Giordano, as an MS candidate soon afterward.

Dr. Richard Witter was finishing his MS degree program under Dr. Levine in the early 1960s. One evening, upon excitedly returning from a meeting that dealt with techniques for working with "RIF," my enthusiasm spilled over in the form of a strong message to Dick. I told him that tumor virology was a most exciting challenge and I encouraged him to take up studies on avian leukosis for his PhD project. Dr. Levine suggested that I should take him on as my student, but I urged Dick to not give up Dr. Levine as his primary mentor because I believed that he was absolutely the best! As a consequence, I served as his "thesis research advisor" rather than the chair of his committee. History will confirm that this switch in direction for Dick was good not only for him but abundantly so for the field of avian tumor virus research.

The period starting in the 1960s, when Marek's disease became the subject of intense study by many laboratories was, without question, the most exciting time of my career. It was in the mid1960's that Lloyd Spencer arrived as my first true PhD graduate student, and Maurice Smith followed soon thereafter. During the time they were at Cornell, the main focus of my tumor studies gradually moved from retroviral-induced avian leukosis to Marek's disease (MD), which appeared to be of much greater importance to the poultry industry. Over the subsequent years, and with the help of numerous graduate students, my work focused on the pathogenesis of this herpesvirus-induced lymphoma. We concentrated on virologic, pathologic and immunologic studies which were carried out in experiments with single variables such as genetic make-up, virus strain, age, antibody status, etc. Ultimately, we developed a model of the pathogenesis of this disease that is still largely accepted and is often referred to as the "Cornell model."

An early goal was to identify the agent that caused MD. I spent about 2 years in semi-frustration on this project. This was a difficult task because the virus was known to be highly "cell-associated," that is, it was infectious only when whole cells (blood or tumor cells) were used as the inoculum. When those cells were disrupted, the material was no longer infective when inoculated into susceptible chickens. A first step was to try to grow the virus in cell cultures. I prepared cultures from virtually every organ system I could think of and inoculated them with live tumor cells. After varying times and passages of the cultures, I would inoculate them into susceptible chicks to see if they could cause MD. I had tantalizing results that suggested I was growing the "virus" but in too many cases, control cultures of tumor cells alone remained infective for chickens so I could not be sure that the infection had spread from tumor cell inocula to my tissue cultures. I used cultures derived from chicken embryos rather than chicks to avoid the possibility that my tissue cultures themselves were already infected with MD virus if the donors had been exposed (we did not have specific-pathogen-free flocks at that time). This turned out to be a big mistake; we later learned that tissues from chicks are much more susceptible to infection *in vitro* than are tissues from embryos. Even so, it appeared that I had probably succeeded with embryo kidney cells since they were much more highly infectious for chicks than were the tumor cell control cultures. The other mistake was that I subcultured the cells twice weekly and we learned later that it took longer than 3-4 days for focal lesions from MD virus infection to develop in cultures. The bottom line is that after 2 years of hard work with only equivocal results, my competitors in England and East Lansing, MI won the race. They had used chick kidney-cell cultures and cells from another species (ducks), respectively.

A more successful period started out with Lloyd Spencer's PhD study in which he developed reagents and immunofluorescence tests to detect MD virus (MDV) infection in various organs. I was on sabbatic leave in California at the time, but upon my return, and Dr. Spencer's completion of his work, I engaged in a study of tissue distribution of infection in both genetically susceptible and genetically resistant chickens. Stephen Hitchner (a colleague and our department chairman) and I independently wrote up lists of tissues to be examined to determine the pattern of infection from the time of virus exposure until tumors developed. Our lists were nearly identical and both contained more than 40 tissues representing all the major organ systems of the body, including all those we could think of that could potentially give access to the environment for any virus produced by them. Skin was among the latter. Each day, I would examine material from two birds using immunofluorescence tests on frozen tissue sections. The most frequent and persistent site of infection was in cells lining the feather shafts, *i.e.*, the feather follicle epithelium (FFE). This was pretty exciting! With the help of Donald Kahn, a graduate student in another department, material from that site was examined by electron microscopy where to our amazement, there were myriads of virus particles which had envelopes. This was a key observation given that a virus envelope is necessary for a herpesvirus to be infectious in the cell-free state and other organs known to be infected had only non-enveloped virus particles. We then determined that cell-free filtrates from sonically disrupted FFE cells were highly infectious, and that dried feather debris also was highly infectious when placed in isolator units with susceptible chicks. Thus it could be concluded that the enveloped virions that were shed with dander and dried cellular debris attached to molted feathers could contaminate the environment as infectious virus, which in turn was picked up by other chickens via the respiratory tract. A colleague at the USDA laboratory in East Lansing who was a competitor in MD studies (there is always competition among scientists working in the same field) was crestfallen when he learned of our findings; it seems he also had examined many tissues from infected birds by immunofluorescence but had dropped examination of skin because his reagents gave too much background reaction with that tissue.

So, while I was unlucky in the race to identify the etiologic agent, I was lucky in the one involving virus spread. Our findings had solved one of the true enigmas in the field of Marek's disease studies. Unraveling the mystery of transmission brought our research at Cornell a good deal of attention among other cancer research scientists. In part, this was because transmission of the disease using cell-free filtrates provided the first conclusive evidence that a herpesvirus alone could cause cancer, thus bolstering the belief that some human herpesviruses could be oncogenic.

Meanwhile, effective herpesvirus vaccines were being developed in England and at the USDA laboratory in East Lansing, MI, the latter following Dick Witter's isolation of a related but nononcogenic herpesvirus from turkeys. Tissue cultures infected with the herpesvirus from turkeys (HVT) were frozen and shipped to chick hatcheries in liquid nitrogen where they were then thawed and injected into day-old chicks. Stephen Hitchner had experience in lyophilizing viruses, and although the yield of cell-free virus that could be obtained from HVT-infected cultures was low, we decided to try lyophilizing it to at least facilitate shipping the virus from one laboratory to another. In this case, a bit of serendipity occurred. To obtain the small amount of cell-free virus from cultures, it was usual to subject the cells to sonic vibration to disrupt them

and release the virus. The plan was to then add various stabilizers that helped preserve virus infectivity during the freeze-drying procedure. To save a step, I suspended the infected cells in the stabilizers before sonication. To my amazement and initial disbelief (I was sure there was a mistake) this was the key. One particular stabilizer effectively protected the enveloped virus particles during the sonic disruption and the amounts of infectious virus that survived were so high that lyophilized HVT could provide doses sufficient for commercial application as a vaccine which could be stored and shipped without the need for liquid nitrogen. Our methods were patented by Cornell, and although used sparingly in the United States, royalties from sales of freeze-dried HVT vaccine in other parts of the world yielded significant monies to add to our NIH funding. The patent also covered other cell-associated herpesviruses, notably the varicella-zoster virus. The Japanese soon used our extraction method to produce the first effective herpesvirus vaccine against chicken pox in humans. Unfortunately, the Japanese government did not allow patent coverage for the human virus, and a similar vaccine developed by Merck in the United States (which was covered) did not receive licensing from the government until after the patent expired.

Dr. Karel (Ton) Schat became one of my graduate students in 1975 and after he completed his studies I was able to retain him as a colleague with whom I collaborated until my retirement in 1995. His PhD project was to find and compare strains of MDV that were of low virulence, with the aim of determining why some strains were more highly oncogenic than others. He mentioned his search for low-virulence virus strains to Dr. Randall Cole who, with Dr. F. B. Hutt, had developed the highly MD-susceptible S-strain, in Cornell's Poultry Science Department. Randy told Ton that he had a building on the Poultry Farm that housed some nearly mature S-strain chickens which, very strangely, had not had any losses due to MD. Ton isolated an MD virus which was named SB-1 (S for S-strain, B for the "B-house" on the farm, and "1" indicating it was the first clone obtained during purification techniques). Using methods that I had developed for enhancing the expression of oncogenicity with low-virulence strains of MDV, it was determined that SB-1 was entirely devoid of tumor-producing potential and, in fact, it could serve as a vaccine against virulent MDV. The strain was patented by Cornell but it stayed on the shelf for nearly 5 years until it was discovered in field trials that it provided a synergistic effect when combined with HVT (a phenomenon observed with other viruses by Dick Witter and his colleagues at the East Lansing laboratory). After that discovery, industry pressure forced licensure by the federal government and SB-1 became part of a so-called bi-valent vaccine administered to flocks around the world. The patent income from SB-1 provided yet more support for our work.

As mentioned, the central theme to much of my research on Marek's disease dealt with the subject of pathogenesis; we wanted to know the pattern of infection that led from the point of virus exposure to the development of tumors. And, we wanted to know what factors influenced that pattern. Thus, early work centered on first, establishing the sequential steps in infected chickens, and then on the effects of factors such as genetic constitution, age, maternal antibody, route of exposure, immune competence, etc. The first indication of a pattern was based on examinations of infected birds starting at two weeks post-infection, and it was determined that the infection was initially centered in lymphoid organs and it subsequently appeared in tissues of epithelial origin. The incidence and severity of tissue infection depended on genetic susceptibility. However, later studies showed that we were only seeing the second phase of a bi-

phasic infection, with the first occurring only in lymphoid organs but essentially equally so in resistant or susceptible birds. After several years of study our team had determined the important sequential events: B cells infection with cell death and inflammation; activation (with concurrent proliferation) and infection of T cells; immunosuppression; proliferation of infected T cells resulting in an occasional one transforming; and proliferation of transformed T cells into gross tumors. Many derivative studies were carried out to determine the important immune responses, the nature of age and genetic resistance, the characteristics of the tumor cells themselves, and a variety of other related subjects. For instance, a large number of studies from our laboratory (and from other laboratories) helped describe the nature of MD tumor cells through the development of many tumor cell lines established from lymphomas. Also, our group conducted very significant studies on the immunologic responses that were part of the pathogenesis of the disease as well as part of its control.

One of our research teams during the 1980s: Ton Schat (left) and Bruce Calnek (center) with three of their graduate students (Ken McColl, Dan Weinstock and Celina Buscaglia)

Indeed, the period from the mid 1960s to the 1990s was a remarkably exciting one that stretched from the first consistent transmission of the disease by Martin Sevoian at UMass, and independently, by the Houghton Poultry Research Laboratory in England, to the discovery of the herpesvirus etiology by the English workers and the East Lansing group, again independently, to the development of vaccines (first by the English workers) and the unraveling of the pathogenesis of the disease and the mechanism(s) by which MD viruses provoke normal cells to become malignant. Many laboratories worldwide participated in this research arena during these fruitful years and it was a true privilege to be part of the milieu. Ton Schat and I, along with our graduate students, had a marvelous experience in group research for a number of these productive years. The interactions, thrill of discovery, and the systematic way in which we team-tackled the subject of pathogenesis of a tumor disease made all of our careers exciting. That is not to say that we, like most research teams, did not have flops, disappointments, and slow periods, but those were certainly overshadowed in the overall picture.

One has to reflect on what makes for a successful research career. I think that in my case, it was my enthusiasm for my work, and my willingness to get through periods where nothing seemed to work. Perhaps most important for me was my determination to be a "bench person" who wanted to be directly involved (alongside my technical staff, and graduate students, of course) in all aspects of the research such as inoculating birds, doing necropsies, collecting tissues, conducting the culture work, and personally involving myself in gathering the results – I was never satisfied to delegate these sometimes routine tasks. Also, I believed that you see things that you recognize as significant but which might not seen as such and related to you by an "assistant." Thus, I had the opportunity to think about things while collecting results and to ponder the "whys and hows." This generated ideas of where to go next. Equally important, of course, is the team with which you are blessed. And one cannot ignore the interactions with, and help from colleagues. In addition to their direct contributions to a research project, they offer ideas and interpretations that are critical to a successful program. No one works alone in today's world, and therefore no one can take sole credit for their research success.

Part of my research success must be attributed to my presence at the bench where I could personally view and interpret the results of our experiments.

In addition to my avian encephalomyelitis and tumor virus studies, I also was engaged, albeit less intensely, in several other research areas including adenovirus classification, egg transmission by reoviruses, and pathogenesis of laryngotracheitis virus infection. The latter was done during a sabbatical leave in Australia. Other sabbatical leaves were at the University of California, Berkeley, and the Houghton Poultry Research Laboratory in England.

Administrative and other duties. Dr. Stephen Hitchner assumed the chairmanship of the Department of Avian Diseases upon Dr. Levine's stepping down in 1965. In 1973, he took a sabbatical leave in Melbourne, Australia, and I was asked to serve as Acting Chairman during his absence. Upon his return he announced he would limit his term to 10 years and Dean Edward Melby asked me if I would take over as Chair when that happened. I was reluctant to give up my research, and administrative work was not a goal of mine, so I declined. After an unsuccessful search was conducted by the Dean, he continued to press me to take on the job. I still was unwilling to do so until one day when two of my colleagues from other departments independently told me that Dean Melby was considering disbanding the Department of Avian and Aquatic Animal Medicine, as it was then named, if I was unwilling to do the job. That was

an untenable consequence for such a successful department, and I relented after Dean Melby convinced me that I should be able to continue in research for at least 50% of my time. Also, he offered me the best administrative manager in the College. It worked; I carried out uninterrupted research at our Snyder Hill laboratory in the morning (no meetings, no phone calls, etc.) and then went to my administrative office at the College in the afternoon. I snuck in a good deal of research record-keeping on many of my afternoons. This worked particularly well for the first 10 years when all of the faculty were experienced and needed little direction. However, as they retired, they were replaced by younger persons who needed considerably more attention as their careers were being established. Also, administrative work became gradually more demanding with increased oversight and rules to be followed.

As is not unusual, a second 5-year term as Chair followed the first. But after 10 years, when it is often appropriate to step down, there were no obvious choices for a replacement among the faculty of our relatively small department. So, I continued for what turned out to be an additional 9 years, until my retirement.

Perhaps one of the strongest challenges I faced during my tenure as Chair was to keep the department and its programs intact. We became a separate department (Avian Diseases, with P. P. Levine as its first Chair) in 1961, the year I returned to Cornell from Umass. The story I heard was that this was, in large part, to keep Dr. Levine from accepting an attractive offer elsewhere. In 1973, during Steve Hitchner's tenure as Chair, our department assumed the responsibility of developing a program in aquatic animal medicine. The Department of Microbiology had hoped to be awarded that "plum" by the college administration but the Department of Avian Diseases won out, largely because there were similarities between birds and fish in terms of the approaches which were suitable for disease diagnosis and control, *e.g.*, population medicine and preventive medicine. The addition caused a name change – to the Department of Avian and Aquatic Animal Medicine (with the lovely acronym of DAAAM). Very importantly for our small group of avian specialists, it beefed up our overall program to make it more visible and more viable in terms of size. Unfortunately, it also further pointed up the fact that we were the only species-oriented department in the college. Everything we did could easily have been absorbed by other discipline-oriented departments such as Microbiology or Pathology, and there were detractors within the College who felt that species-oriented departments were inappropriate. During my terms as Chair, this was always a problem that had to be faced. Our salvation, in part, was the success of both programs. Our avian studies were productive with substantial external financial support, and the aquatic animal studies filled a large need, both in the shellfish and fin-fish industries. Fortunately for me, because I knew very little about fish, we had a series of very good specialists in that field, starting with Louis Leibovitz who switched from duck specialist to shellfish specialist, and subsequently with persons specifically trained in the field of aquatic animal medicine. All of us participated to some extent; part of my contribution was to develop a couple of permanent fish cell lines useful in diagnosis of viral infections of fish.

Unfortunately, over the years and particularly in the late 1980s and early 1990s, a series of financial cut-backs in our State budget had to be dealt with. Personnel losses eroded our ability to maintain the level of activity we had reached, particularly with certain programs. Branch poultry diagnostic laboratories were closed, we lost professional personnel both on the Ithaca

campus and at the Cornell Duck Research Laboratory on Long Island (administered and largely staffed by our department). With our reduced faculty and overall personnel size, and reduced funding for administration, it again became problematic as to whether we could justify our existence as a small department that could easily be absorbed. The approach I took was to strongly emphasize to the administration that we constituted a "national and international resource" and that it was not in the best interests of veterinary medicine to break up a strong and productive program that had in place all of the requisites for success, *i.e.*, knowledgeable and experienced faculty and staff with strong reputations, animal facilities that were the envy of many other research units engaged in research on avian species and fish, specialized research laboratories and the supporting materials and equipment needed for studies on diseases of these species. This seemed to work with the two deans I was associated with while I chaired the department (Edward Melby and Robert Phemister) because they supported our agendas and kept us "alive." Indeed, when new facilities were planned for the college, space for the DAAAM was programmed as it was for all other departments. As an aside, I might note that when new college facilities were being planned in the early 1990s, the architects proposed tearing down the 41-unit Poultry Virus Isolation Building used for our poultry and fish disease research. I am sure the architects would not admit it, but I suspected this was because the isolation facility would be "shabby" looking next to the grand and lovely large animal hospital barns that would be adjacent to it. As Chair of the Central Planning Committee (see below), I told them it would come down "over my dead body." Their solution was to help the State find over two million dollars to upgrade and refurbish our Isolation Building so that it "looked like it belonged." The upgrade was needed for its own sake.

In the mid 1990s, just about the time we were scheduled to move to our new research laboratories, there were some major changes "in the wind" and my status was part of the story: 1) I had made plans to retire in 1995 which required a change in leadership, 2) a new dean was being recruited, 3) there was reason to believe that there would be a major realignment of departments in the college, and 4) some colleagues in the Department of Microbiology, and especially its Chair, Dr. Roger Avery, were lobbying hard to have us merge our two departments hoping to forestall uncertain but likely changes in the status of both departments. Thus, we amicably joined forces with our "sister" department. Our poultry disease group became a Unit of Avian Medicine within the newly formed Department of Microbiology, Immunology and Parasitology, with Dr. Roger Avery as the Chair. Interestingly, I had headed the search committee that brought Roger to Cornell in the 1980s from a position he had held in the Houghton Poultry Research Station in England. On the negative side, we could predict that there would likely be a gradual erosion of the emphasis on avian medicine without direct control over the programs and support in that field. Indeed, that has happened in subsequent years; as faculty in avian medicine retired, they were not necessarily replaced by persons with similar interests so that today (2007) Dr. Ton Schat remains as the only faculty member in the department whose specialty is avian disease research. This trend is not unique to Cornell University, but that does not diminish the disappointment associated with the loss.

Aside from research and administration, there were two major activities that were added to my "plate" in the course of my academic career. One started in 1975 when I was invited to become an Associate Editor for the so-called "Bible" in the field of avian medicine, a large volume entitled *Diseases of Poultry* published by the Iowa State University Press. The American

Association of Avian Pathologists (AAAP) took over editorial responsibility for the book when it was time for a 6th edition, and I accepted a role as Associate Editor overseeing many of the chapters on viral infections. Also, I had the privilege of writing the chapter on Marek's disease, with Dick Witter as co-author. This was a demanding job for a couple of years in each of the 6-year cycles of the book. Mel Hofstad who had headed the editorial committee, resigned after the 8th edition, and I was appointed to the much more demanding job as Editor for the next two editions. There were upwards of 100 authors to look after, along with four Associate Editors, and the job became more complicated when I oversaw the initial switch into the modern age with computer-assisted preparation. I also took over responsibility for the chapter on avian encephalomyelitis. I relinquished both the editorship and authorship of the Marek's disease chapter after the 10th edition was published two years into my "retirement."

My second major activity other than research started in 1987 when I was appointed by the Dean as Chair of the Central Planning Committee. This was to oversee the College's part in the planning for what turned out to be a $93 million addition of new teaching, research and library space and the complete replacement of the teaching hospital at the College. I was told by the Dean that it should take about 35% of my time, but it turned out to require up to 90% during some stages. I was sufficiently occupied that my colleagues had to help me with some administrative duties and I could not keep up my research commitments at a level sufficient for maintaining my long-time NIH research grant. However, I believed that I owed it to the college that had given me so much.

Professional student teaching was not one of my responsibilities, but I did teach the 3rd year course on Poultry Diseases the last year it was given, when my colleague, Dr. Syed Naqi, took a sabbatical leave.

During my career, I have enjoyed a number of honors. Examples include: the first recipient of a new Cornell University "Named Chair," The Rudolf J. and Katherine L. Steffen Professor of Veterinary Medicine; Gordon Memorial Lecturer, London; Bart Rispens Memorial Lecturer, The Netherlands; Conference Orator, 4th Asia-Pacific Poultry Health Conference, Melbourne, Australia; President, American Association of Avian Pathologists; U.S. Public Health Service Special Postdoctoral Fellowship; Eleanor Roosevelt Cancer Research Fellowship; P. P. Levine Research Paper Award (three times for best research paper in *Avian Diseases*); AAAP Special Service Award; Upjohn Achievement Award; Veterinary Research Award, American Feed Manufacturer's Association; Josef Marek Commemorative Medal for Research Achievements (conferred by the University Faculty, Budapest, Hungary); Outstanding Service Award of the New York State Veterinary Medical Society; Life Memberships in the American Association of Avian Pathologists, the American Veterinary Medical Association, and the World Veterinary Poultry Association; Diplomate, American College of Veterinary Microbiologists; Charter Diplomate, American College of Poultry Veterinarians.

Retirement Years: Finally, we come to the end of my tenure as a Professor. In 1995, at age 63, I succumbed to the attractiveness of a little less pressure and the opportunity to spend a bit more time on my decades-long love for woodworking, and so I took advantage of an early retirement offer. It was with the full expectation that, as an emeritus professor, I could continue working professionally to whatever extent I wished. Indeed, the next two years were largely

occupied with my responsibilities for the 10th edition of Diseases of Poultry coupled with some interesting research. I brought my long-time assistant (and friend and colleague), Ray Harris, out of retirement and we became enjoyably immersed in part-time research on MD and also in work with cell lines we had developed which were uniquely susceptible to chicken infectious anemia virus (a final patent resulted).

Happily engaged in clock-making following retirement

Retirement allowed my woodworking interests to get a great deal of attention and that led to a small business (now mostly clock-making) and a partnership in a 40-member craft cooperative with its own store in Ithaca. However, I still manage to get to my office at Cornell a few times each week where I share space with my long-time friend and colleague, Julius Fabricant. I am presently involved in assembling material with which to write a history of avian medicine at Cornell (a rich subject), and I recently joined the History Committee of the AAAP.

Our two sons now have careers of their own, which gives Mary Jeanne and me a good deal of pride. David, earned a PhD at Cornell and has been involved with genetic testing in a laboratory in Atlanta. Douglas, graduated from Syracuse University and is in a professionally challenging position with TIAA-CREF, a major provider of retirement plans for academics. Together, they and their wives (Caroline and Lynn, respectively) have given us 5 lovely grandchildren ranging from 21 years to a few months old (with one more on the way)

In summation, I would have to say that I have been blessed with a wonderful family (and a very understanding wife), many joys, and what I consider to be an abundance of good fortune in my career. Everyone should be so lucky!

Biography solicited by the Committee on the History of Avian Medicine, American Association of Avian Pathologists.