Peter Martin Biggs

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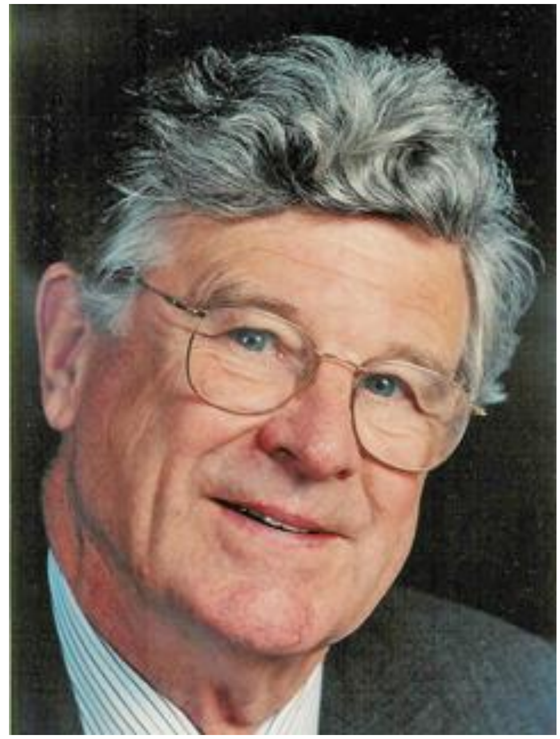

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Peter Martin Biggs

Boyhood

I was born in Petersfield, Hampshire UK on 13th August 1926. When I was five the family moved to Dartington Hall in Devon where my father was Director of Music at which time I went to the Dartington Hall Junior School. Soon after that we moved to Totnes and I was educated at home in a small school my mother ran until I was eight when I went to Totnes Grammar School. I do not remember much of this period except that the preparatory school for the senior school was a single multi-age class and one was seated according to achievement. On entering the school one was placed at the bottom of the class. This was a challenge and I was determined to make my way to the top! After two years this was achieved and I moved on to the senior school. I left Totnes Grammar School when I was eleven and went to Bedales School, a progressive independent school, in Hampshire as a boarder where my father was part time Director of Music. The family returned to Petersfield in 1939 which was about 20 miles from the South Coast. In the Summer of 1940 after the military evacuation at Dunkirk my parents were convinced that England would be invaded. At that time children were being evacuated overseas to Canada, Australia, South Africa and the USA. My parents asked me whether I would like to be evacuated to the USA. After some serious thought I said “yes” considering it an unrepeatable opportunity. So at the age of just 14 I was evacuated in late September 1940

to Massachusetts, USA where I was 'adopted' by a family and sent to the Cambridge School, which also was a progressive private school, from which I graduated in 1944. I then returned to my parents in England who were living in London. I learned my way around London on a bicycle amidst V1 bombs ('doodlebugs') and later V2 rockets.

Throughout my childhood I always had an interest in natural history with particular interests in birds and butterflies. I also spent much of my time both in England and the USA working on farms and in particular with farm animals which in those days included working horses.

Service in the RAF

I joined the Royal Air Force in the autumn of 1944 as a trainee for aircrew and spent the first six months of this service at Queen's University, Belfast studying physics and engineering together with climatology and geology on what was called a 'RAF University Short Course'. During this time I was a member of the University Air Squadron where, in addition to the university work we were tutored and examined in appropriate subjects for flying which were usually taken at the Initial Training Wing. Following this I was sent to an Aircrew Reception Centre in Torquay for seven weeks induction course during which VE Day occurred. I then went to grading school where one learnt to fly in the initial trainer, the De Havilland Tiger Moth. On ones performance during 12 hours of flying one was graded pilot, navigator or bomb aimer. I was graded pilot having flown solo after four and a half hours flying training. Soon after this the war ended with VJ Day and aircrew were no longer required. However I was not released but re-mustered to a trade in ground crew. I was eventually demobilised in March 1948.

Decision making

On leaving school my intention was to become an aeronautical engineer. However after three and a half years in the Royal Air Force starting with a university course in engineering while doing the initial aircrew training, gave me some experience of engineering and more importantly time for reflection. The result was a change of interest in engineering to biological subjects. In particular I became interested in the ecology of wildlife. I was by this time very keen and interested in research. I had already obtained a place to return to Queen's University to study Agriculture. But now my interest in research moved in the direction of zoology and I made tentative enquiries to Cambridge University. However, on further reflection I realised that if I did not make the grade for a career in research the alternative was likely to be school teaching! Although my mother was a teacher I did not feel attracted to this profession and considered I had no vocation for it. I then thought again and with my background of working with animals on farms I considered a veterinary training. I would be happy working with animals in veterinary practice if I did not achieve the grade for a research career. So I applied to enter the Royal Veterinary College in London and was accepted to start the five year course in October 1948.

Veterinary College

Three major academic factors during my veterinary course influenced my subsequent career. The first was an inspirational teacher, the second a stimulated interest in viruses

and in cancer and thirdly a book. The teacher, Dr A.S. King (Tony), gave a most stimulating series of lectures and discussions on the embryology, anatomy and physiology of the domestic chicken during my second year at the college. This not only stimulated an interest in the chicken and avian species but also confirmed my inclination that I would like to spend my career in research. The second factor was an attraction to the study of viruses and cancer arising from the course in pathology which included infectious disease. Finally, while working in the library and scanning possible useful texts I came upon the book by Ellerman and Bang entitled "The leucosis of fowls and leucaemia problems" published in 1922,. being a report and discussion of studies done by them in the first decade of the century. This stimulated my interest in virus induced leukoses of the fowl.

I had known Jan Molteno, the daughter of great friends of my parents, since early childhood. However, I had not seen her since I left for USA until soon after I was demobilised from the RAF when I invited her to join a group of friends on a sailing holiday. From that point I knew she was the girl for me. At the end of my second year at college we got married and she has been an invaluable support in so many ways in my life and career.

Further education

On graduation I wished to study for a PhD on some aspect of viruses and cancer but preferably avian leukosis. The next challenge was to get sponsorship to enable me to do this. Most avenues were closed because I was married. However, with the advice and help of Tony King, who had in the meantime moved to the department of Veterinary Anatomy at the new veterinary school at Bristol, I was encouraged to apply for a two year Research Assistantship in the Faculty of Medicine at Bristol. I was the first veterinarian to make such an application so I was not optimistic about my chances. It was my good fortune to be successful in my application and following a very short time in practice I took up this post as a PhD student in the Veterinary Anatomy Department at the Veterinary School in the University of Bristol. My first wish had been to do a PhD in some aspect of viruses and cancer but this was not possible, therefore I settled for a subject in anatomy. I was correctly advised that the subject of a PhD programme was less important than the training in research which was part of all science PhDs. I chose as my subject "Lymphoid tissue in the endocrine glands of the domestic fowl: its significance in health and disease" which provided good experience and background for my later studies on the pathology of the components of the avian leucosis complex. Although Tony King was not officially my PhD supervisor he became so in effect. We also did some research together on respiration in the fowl and he taught me the importance of rigour in scientific research. He was my mentor during this period.

First job

At the completion of the two years in 1955 I had to find a job. I applied for a post at the Animal Virus Research Institute, Pirbright to work on foot and mouth disease virus and other exotic viruses. But for an administrative "error" I would have taken that job and my future career would have been completely different. Fortunately, my interest in avian tumours and viruses appealed to the Head of the Bristol Veterinary Schools's Field

Station at Langford, Professor Blakemore, who earlier in his life had done some research on "fowl paralysis". He offered me a post as a Lecturer in Veterinary Clinical Pathology which allowed me to complete my PhD and pursue my interest in the avian leukosis complex. Amongst other subjects I lectured on virology and immunology where major advances had and were being made in the 1950s. During this period I gained experience of the rapidly developing poultry industry (which had just illegally imported eggs of broiler breeders from the USA) and in avian pathology and haematology and cell culture. It seems surprising now that in all the disciplines I was involved with in my research I was largely self taught. I don't believe this would be possible now. During the four years I had at Langford my research time was spent on completing my PhD and progressing my interest in the avian leukosis complex.

During my first year at Langford, because of the difficulty progressing leukosis research largely due to the lack of suitable animal accommodation, I developed an interest in transplantation, immunological tolerance and in particular the graft versus host reaction, all of which were topical interests of biologists and immunologists at the time. During this period L.N. (Jim) Payne, who was a final year veterinary student, had shown an interest in research and, in particular, in avian viral tumours. On graduation he joined me in the pursuit of my interests in the avian leukosis complex and graft versus host reaction. This was the beginning of a close collaboration that was to last for over 20 years and an association and friendship to this day. Some studies were undertaken in an attempt to establish a transplantable lymphoid tumour but they were unsuccessful. Looking at the results of these studies in retrospect it is likely that the procedures used had transmitted Marek's disease. Because of limited accommodation for experimental studies using the domestic chicken and the absence of isolation accommodation much of the time was devoted to studies of the graft versus host reaction. Three papers were published, including one in Nature, on subjects related to the graft versus host reaction. During this period techniques were established that were invaluable to later research. Two in particular were the intravenous inoculation of chick embryos and techniques for the study of avian chromosomes. I also gained invaluable experience in avian haematology, pathology and the handling of avian chromosomes.

Houghton Poultry Research Station

My ambition to do research on viruses and cancer were fortuitously enabled by the decision of the Agricultural Research Council to fund a programme at the Houghton Poultry Research Station (HPRS) on the "Avian Leukosis Complex". This was considered to be of major economic importance to the rapidly developing poultry industry. The Council and HPRS advertised for someone to lead this programme and it was my good fortune to be the successful applicant. I moved from Bristol to HPRS in the early summer of 1959. My first year was taken up with three activities. Firstly, designing laboratories and animal isolation accommodation for the Leukosis Experimental Unit (LEU). Secondly, undertaking limited studies on the avian leukosis complex that did not require isolation accommodation for experimental chicks. Lastly, a fruitful three month trip to the U.S.A visiting many laboratories involved with viruses and cancer.

The LEU, consisting of a laboratory block, which included a specially designed laboratory for tissue culture, and two wings of isolation rooms for experimental animals was completed and opened in January 1962. (Additional animal isolation facilities of greater sophistication were built and opened in 1965 and 1975.) This was followed in March 1962 by the opening of the Leukosis Production Unit about ten miles away for holding the breeding stock of lines of chicken the progeny of which were used for experimental studies at the LEU.

The studies I undertook over this period were mainly pathological. Using a range of techniques, I hoped to be able to provide evidence that Marek's disease was different and distinguishable from lymphoid leukosis. These studies were limited by having only field cases to work with. However, differences were noted. Some of the pathological studies used the developing chick embryo in a graft versus host assay to examine the immunological potential of cells from tumours of Marek's disease and a transplantable lymphoid leukosis tumour (RPL 12). They were not very productive but for one experiment where a cell suspension from Marek's disease tumours was injected intravenously into ten 16 day-old developing chick embryos which were allowed to hatch. By 14 days of age 5 of the ten had died with lesions of MD and by 39 days 9 of the 10 had died of MD. Although there were no controls this result convinced me that the disease could be transmitted by using tumour cells as the inoculum and that good isolation facilities were required for future transmission studies.

The third activity was a profitable three month tour of laboratories in the USA that were working with tumours and their viruses in the spring and early summer of 1960. First I attended a symposium held in New York on tumour viruses which was notable for the first report by Harry Rubin of the Resistance inducing Factor which he suggested was an avian lymphomatosis virus. It was at this meeting that I first met Ben Burmester. After visiting a number of people working with mammalian tumour viruses including R.E. Shope, Charlotte Friend and Ludwig Gross I moved on to the Regional Poultry Laboratory East Lansing where Ben Burmester, after a very short acquaintance, kindly asked me to stay with him and his family for the three weeks of my stay in East Lansing. This was the beginning of a long friendship between our two families. All the staff at the RPL spent a lot of time with me and I learned a great deal. Amongst a number of other laboratories I visited were those of Joe Beard and his staff at Duke University, Ray Bryan and others at the National Cancer Institute at NIH, Bethesda, and Fred Hutt and Randy Cole at Cornell. All the visits were invaluable but I particularly treasure the visits to RPL East Lansing and Cornell where I spent valuable time in their post mortem rooms and discussing the pathology of the condition with which each laboratory was working. It was clear to me at the end of my visits that the former was working with lymphoid leukosis and the latter with Marek's disease. This explained the differing results and disagreements there had been between the two laboratories.

One other experience which left a deep impression with me was when Ray Bryan took me out to the rapids on the Potomac river, a place of fascination and tranquility. What stays in my memory and influenced me was Ray Bryan saying that this was where he came to think. He taught me by this one act that one needs peace and time to think.

I recommend a tour of this nature to anyone starting up in an area of research which is novel to them. One not only benefits from the expertise of individuals whom one visits and the workings of their laboratories but also from the friendships and valuable contacts that can be made.

In November 1960 the first Congress of the newly formed World Veterinary Poultry Association was held in Utrecht, Holland. It was at this meeting that I suggested a classification for the avian leukosis complex and introduced the name Marek's disease in a paper discussing one given by John Campbell on the classification of the avian leukosis complex. I chose Marek's disease because at the time there was confusion over the pathological terminology used for Marek's disease and lymphoid leukosis and for this reason I chose a term that implies a disease and not a pathological entity. What better name than that of the man who first described the disease! My suggestion of Marek's disease was adopted in a resolution of the Congress.

I recall this congress vividly and pungently because not only was it my first presentation to an international audience which included so many veterinarians distinguished in the poultry disease field but also because I experienced what every presenter of a paper fears. As I have already said my paper was on the classification of the avian leukosis complex and fowl paralysis following an opening paper on the same subject given by Dr John Campbell. My paper ended up with the suggestion of a classification of the diseases into two groups the details of which were in my last slide. But when I called for it I was told by the projectionist that there were no more slides! After some discussion with the projectionist to no avail, I was forced to explain my proposed classification without illustrative support. The irony of the situation was revealed when I went to collect my slides at the end of my talk I found the slide in the projector!

Marek's disease and lymphoid leukosis

Jim Payne joined me at Houghton in 1961 and soon after this we moved into the recently completed Leukosis Experimental Unit in January 1962. We immediately set about attempting to transmit Marek's disease and lymphoid leukosis. We were successful at both and by the end of the year had serially transmitted Marek's disease through six passages and had isolates of lymphoid leukosis virus in stocks in the freezer. The success with Marek's disease was the result of using whole cell preparations initially of tumour material and subsequently whole blood. The latter was prompted by the studies of Durant and McDougal published in 1945. Isolation facilities and, serendipitously, the use of a highly susceptible strain of chicken were also important. We then concentrated on studies on the pathogenesis of Marek's disease and the elucidation of its causal agent. There was a need to establish whether Marek's disease and Lymphoid leucosis were different manifestations of the same disease or were separate diseases with different aetiologies. A comparison of the properties of the causal agent and the pathology of Marek's disease with those of lymphoid leukosis provided strong evidence to show that they were different diseases with different aetiologies. This was presented at an International Conference on Avian Tumor Viruses held at Duke University USA in 1964 at which I became reacquainted with most of those investigators I had visited in 1960. It was at this

meeting that I first met Bruce Calnek and over the years a lasting friendship developed between our two families which continues to this day.

At about this time Jim and I divided our responsibilities and I pursued the aetiology of MD and Jim its pathogenesis. Up to this point detection of the presence of infectivity was based on the presence or not of the disease in infected chicks 10 weeks after inoculation. To progress the studies on the aetiology of MD and to best utilise the animal isolation accommodation available it was essential that a shorter term assay for infectivity was developed. Pathogenesis studies had shown that histological lesions of MD were present in nerves and gonads by 14 to 21 days post inoculation. From this I developed a standardised short term quantitative assay which could be done in a single isolation room because there was no evidence of lesions of MD in control chicks kept in contact with infected chicks over this period. This assay allowed a series of experiments which established the cell associated nature of the causative agent of MD.

During this early period the infectious agent could only be maintained by passage in young chickens. Both out of interest and necessity several strains of chicken were used. It was noted that the incidence of MD varied according to the strain of chicken used. This stimulated an interest in the possibility that genetic selection could help to control the disease. We discussed this with the Breeder F&G Sykes, for whom Dr F.B.Hutt was a consultant, and they were interested in a collaborative study to examine the practicality of a genetic selection approach to controlling MD which went ahead with Fred Hutt's blessing. The results of a large scale study suggested that an experimental challenge procedure was both feasible and practical. However, soon after these studies were completed the attenuated MD vaccine developed by the group became commercially available.

At the same time this work on MD was going on parallel studies were taking place on leukosis and sarcoma viruses. The availability of the inbred Reaseheath lines of chicken, originally developed by Michael Pease at Cambridge, allowed studies of the genetics of the control of infection with sarcoma and leukosis viruses using chick embryos and cell culture. These studies led, together with anomalous results with the COFAL test, to work on endogenous leucosis viruses. The choice of these studies was influenced by the long time scale of studies of MD in particular because the infectious agent could only be handled at that time in vivo. The experiments with Rous Sarcoma Virus (RSV) enabled achievement while continuing the work on MD which was necessarily slow at that time. The work also led to studies with F&G Sykes on the feasibility of the use of progeny testing using Rous sarcoma virus challenge of the chorioallantoic membrane of developing embryos to detect for genetic resistance to infection with leukosis virus. It proved to be a practical approach and the results showed promise but the development of techniques for eradicating leukosis virus took precedence.

During this period a number of people were recruited to the group notably Roger Chubb in 1963 to provide immunological expertise and in 1965 Jim Payne exchanged with Graham Purchase of the Regional Poultry Laboratory, East Lansing USA for a year. During this period I concentrated on studies of the nature of the causative agent of MD.

By this time I knew it was avidly cell associated and all attempts to grow it in chick embryo fibroblasts were unsuccessful. I consulted Michael Stoker, an expert in tumour viruses and in particular SV40, who was little help because he had no experience of a cell associated infectious agent. Much later Peter Wildy and I became friends. If only I had consulted him in those days with his experience of herpesviruses we might have made more rapid progress.

In late 1966 Tony Churchill was recruited to provide much needed expertise in virus isolation in cell culture systems. Soon after arriving Tony registered for a Ph D and the valuable work he did over the three years he was with us formed his thesis. During this period he built on the knowledge that the agent of MD was highly cell associated using cells to seed cell cultures. I was away for three months for FAO in the Lebanon in 1967 at the end of which I attended with Tony a meeting of the European Tumour Virus Group in Sorrento, Italy. It was at this meeting he told me he thought he had isolated a virus from MD material in chick kidney cell cultures. The plaques produced by the virus contained syncytia and intra nuclear inclusions characteristic of herpesvirus infections. At that time we did not have an electron microscope so I asked Bob Dourmashkin of the Imperial Cancer Research Fund laboratories at Mill Hill whether he would be willing to examine material. This he did providing excellent electron micrographs showing clearly the presence of a herpesvirus. The results of these studies were submitted to and accepted by Nature however, Bob Dourmashkin did not accept our offer of co-authorship. Soon after the letter to Nature was submitted I was to attend the AAAP meeting at Dallas Texas and en route I visited as usual the RPL at East Lansing only to find that they had similar results using cultured duck fibroblasts but they had yet to submit them for publication. On hearing that we had a paper in press with Nature they decided that they would present a preliminary report at the AAAP meeting and that there should be a press release immediately afterwards. As I was an invited guest speaker at the AAAP meeting I felt morally obliged to present our preliminary results at the meeting and contribute to the press release despite Nature being very unhappy about disclosure of our results prior to publication. This forced the release of a press release in England subsequent to which was a briefing meeting held for the press and industry.

Although evidence was provided that this herpesvirus was the causative agent there was much skepticism in the minds of some, in particular Bob Heubner of the National Institutes of Health USA, with whom I had a long correspondence. Following these dramatic developments there was a lot of work done on both sides of the Atlantic to provide a wealth of evidence that this herpesvirus was the causative agent of MD. However, this provided only circumstantial evidence because the herpesvirus was so strongly call associated. Conclusive proof had to await the isolation of cell free virus by Calnek and colleagues at Cornell and the development of an attenuated vaccine at HPRS.

Both Roger Chubb and Tony Churchill left in 1969. Roger left for Australia and returned to his interest in infectious bronchitis virus. Tony left to start a company manufacturing and marketing the attenuated virus MD vaccine developed at HPRS by him and others. Subsequent to this laboratory work I organised, with the collaboration of two broiler breeders, field trials which showed that the attenuated vaccine was effective under field

conditions. This was a difficult time because up to that point I had thought that Tony was primarily interested in science and its application. We worked together well and happily. But sadly he left to produce and market the vaccine. Norman Ross, Judith Frazier and Prafulla Pani were recruited in 1970 and Patrick Powell in 1971.

On reflection 40 years on, the progress made from the identification of the disease, its transmission and the elucidation of its aetiology to the development of an effective vaccine in less than a decade seems remarkable. This is especially so considering the confusion between MD and lymphoid leukosis in the past and the number of people who tried to transmit the disease without conclusive or convincing results over the previous 40 years. The right tools, people and commitment of funds enabled us at HPRS to go from convincingly transmitting the disease to isolating the causative agent, despite problems posed by its cell associated nature, to producing a vaccine and running successful field trials all in the space of nine years!

I continued to work on MD and, with a number of collaborators, investigated the biological properties of MDV isolates and the epidemiology of the disease. It was shown that it was possible to characterise MDV isolates by the morphology of plaques produced in chick kidney cells. Later, with Vicco von Bülow, MDVs were shown to be of two serotypes and HVT a third. Serotype 1 comprised pathogenic viruses, including their attenuated derivatives, serotype 2 apathogenic viruses and serotype 3 HVTs.

Anecdotal information from the field indicated that the incidence of MD could vary between houses on the same site and between pens within a house. This was intriguing and unlike other infections with which I had had experience. Epidemiological studies were carried out in the field with the invaluable and fruitful collaboration of a broiler breeder in an attempt to resolve this conundrum. These studies suggested that it was likely that immunisation with viruses of little or no pathogenicity could occur naturally and that an interplay of viruses of varying pathogenicity could be responsible for the unexpected variation in the incidence of MD.

One of the rewards of working on Marek's disease was the community feeling amongst all those involved with such research around the world. It was therefore a great pleasure when Bruce Calnek , Jim Payne and Dick Witter joined me as a Marek Medalist when they were presented with the Joszef Marek Memorial Medal at the WVPA Congress in Budapest in 1997. The four of us had crossed paths so often that our families were well known to each other. For this reason we took this opportunity to form the Marek Medalist Society with a membership of ourselves and wives with the eight of us meeting whenever paths crossed.

Lymphoproliferative disease of turkeys

During the first year or two I was at the HPRS I was sent samples of lymphoid tumours from turkeys, a condition which at the time was of concern to the turkey industry. However the condition became sporadic and I was not approached again until about 10 years later. In 1972 I was approached by a turkey breeder and fattener who was experiencing a high incidence of lymphoid tumours in male fatteners. This condition was similar to Marek's disease but investigations negated a herpesvirus as its cause. Further studies implicated an unique oncovirus as the causative virus and I named the disease a Lymphoproliferative disease of turkeys (LPD)

Directorship of Houghton Poultry Research Station and the Institute for Animal Health

At the end of 1973 Bob Gordon retired and I was appointed his successor as Director of the Houghton Poultry Research Station. I took with me a small group from the LEU and we continued work on LPD and vaccination and vaccine breakdowns in Marek's disease.

During my 12 years as Director I tried to ensure the work of the Station was relevant to the needs of the poultry industry, was sound science and used appropriate modern scientific and technological developments. The usual honeymoon period for a new Director soon became a dream with horrendous inflation. Oil and its products became increasingly costly and salaries also increased for which the funding bodies did not compensate. Even so we were able to bring molecular biological expertise and skills to much of the work by new appointments by making savings elsewhere. I also initiated new programmes such as immunogenetics. However, continuing attrition of funding, partly due to a political change of view over the importance of agriculture, but also questions of who should pay for the research supporting agriculture and the view of the funders that infectious disease was over supported made it difficult over the years to maintain the facilities and resources of the Station. Even so research output was maintained. After several years of uncertainty, in 1986 it was decided to amalgamate the four animal disease research institutes into a single institute under one Director and that HPRS would be closed with the remaining staff being moved to one of the other sites.

I was appointed Director of the new institute to lead the science which necessitated visiting all four sites frequently. I had also to put in place an appropriate administrative structure which was time consuming. This was complete by the time I retired. There were considerable problems over the two years I was Director. The major one was attempting to consolidate the new institute when funding was incrementally being reduced. This was aggravated by the poor state of the physical infrastructure on the two major sites for which there were no funds for their improvement. Never the less it was an interesting two years although not very rewarding. I retired from formal employment in 1988.

WVPA and Avian Pathology

I was on the fringe of the formation and early days of the World Veterinary Poultry Association (WVPA) because my Director, Dr R.F (Bob) Gordon, was closely involved in the creation of the Association. I remember him recalling the trials and tribulations of this period and eventual triumph of success. My personal involvement started when

invited to present a paper at the first Conference of the WVPA held in 1960 in Utrecht, The Netherlands on the "Classification of the Avian Leukosis Complex and Fowl Paralysis" already referred to.

I was to become much more directly involved with the Association in 1971 when I was asked to take on the Secretary/Treasurer post which I held for ten years .In 1981 Jim Payne took over when I became President for a four year term. The following four years I was Senior Vice President following which I had the honour of being made an Honorary Life President. I have continued with my interest in the Association but from a more distant point of view.

At the fourth Congress held in Belgrade in 1969 Professor Klimeš, who was the Head of the Poultry Department and later Dean of the Veterinary Faculty in the School of Agriculture in Brno in what was then Czechoslovakia, proposed that a working party should be formed to examine the feasibility of establishing a European journal of avian diseases. There was much discussion but not much enthusiasm for the suggestion. Never the less it was agreed that the possibility be explored of launching a WVPA journal. This was done and at the meeting of the Bureau held during the World Veterinary Association Congress in Mexico in 1971. Professor Klimeš was appointed Editor-in-Chief with an Editorial Board of which I was a member. The first number of the new journal called Avian Pathology was published in the autumn of 1972 which formed volume one. Soon after this Professor Klimeš was unable to continue with the editorship due to ill health. I was asked to take over the duties of Editor-in-Chief temporarily. At the Bureau meeting held in Munich in 1973 I was confirmed as Editor-in-Chief. I decided to step down at the end of 1987 when I considered the journal to be well established and secure. I passed on the job to the capable hands of Jim Payne.

There was a lot of hard work, particularly in the early days, but I had the valuable support of Jim Payne as Assistant Editor-in-Chief. There were frustrations but also much satisfaction.

Retirement

On retirement from formal employment I made the decision that I would like to keep an involvement in science and its pursuit, particularly with veterinary and poultry matters. I feared I might not have enough to do but in reality it turned out to be the reverse! Some of my activities continued my interests, others broadened my experience. I was fortunate to be appointed a Professor-at-Large at Cornell University attached to the Department of Microbiology, Parasitology and Immunology. This appointment was for six years and entailed a visit for two to three weeks each year living on campus. This was a rewarding appointment and incidentally allowed me to keep in touch with, amongst other things, the Department of Avian Medicine and its research and Bruce Calnek and family. At the same time I became involved with the Institute of Biology which was the professional body for biologists in the UK. Biologist was meant in its broadest sense as it included medical, veterinary and agricultural scientists as well as biologists and teachers. I was appointed to the Executive Committee and became the Institute's President for a two year term. Over this period I also became a Vice President of the British Veterinary

Association and a member of the Council of the Royal Society. In addition to these activities I was a consultant to a pharmaceutical company and chaired a number of bodies related to the poultry industry.

I was elected a Fellow of the Royal society in 1976, the oldest scientific academy in the world founded in 1660 and therefore in 2010 it celebrates its 350th anniversary. There are 1300 Fellows coming from all aspects of science, medicine and engineering. Becoming a Fellow changed my life in many ways but particularly by the increased recognition and respect afforded by other scientists, a change in attitude, I believe, quite unwarranted. I also became a Founding Fellow and Council Member of The Academy of Medical Sciences which was founded in 1998.

Among many committees and bodies of which I have been a member during my retirement I found interesting was membership of the Royal Society Committee on Scientific Aspects of International Security particularly its Sub-Group on Scientific Aspects of Control of Biological Weapons which published two reports. I also found interesting being the Royal Society representative on the Inter-union Commission on the application of Science to Agriculture, Forestry and Aquaculture of the International Council for Scientific Unions.

Despite these activities retirement has enabled more time with the family and in particular with my wife Jan. We have had holidays we never dreamed of before and these have taken us to many fascinating and interesting parts of the world.

Reflection

Reviewing my research career there are a number of things which have been important. I hope it is not too presumptuous to believe that they might be helpful to others. Firstly, reading the literature! Secondly, the results of experiments designed to answer a question or test a hypothesis usually pose several more questions; it is the choice of which of these to follow that is important. Thirdly, keeping contact with other workers in the field and being as open about your work as possible is rewarding and accelerates progress. Fourthly, keeping up with new developments in science and technology. Fifthly, new appointments and long term visitors bring fresh blood and valuable ideas to the laboratory.

Looking back on my life I have few if any regrets. It has been a happy and rewarding life and I would not hesitate to do the same again if I had the opportunity of a second bite of the cherry. The work has been stimulating and rewarding and throughout my career I have met so many wonderful people and made so many friends and visited much of the world. What more could one ask for?

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

Additional biographical materials may be available from the AAAP Historical Archives located at Iowa State University. Contact information is as follows:

Special Collections Dept. & University Archives
403 Parks Library
Iowa State University
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Phone: (515) 294-6648
Fax: (515) 294-5525
WWW: http://www.lib.iastate.edu/spcl/index.html