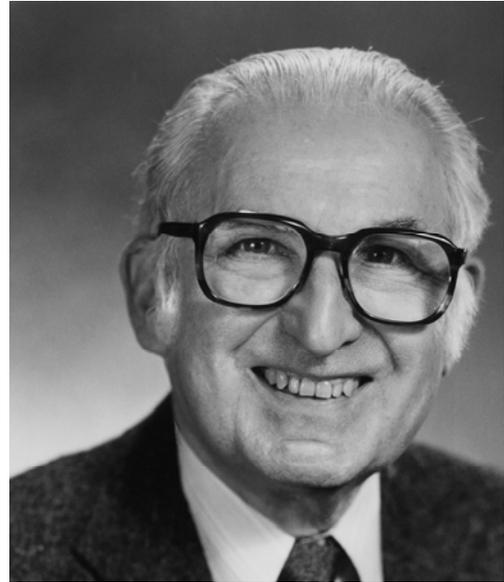


American Association of Avian Pathologists
Biographies of Professionals in Poultry Health

Julius Fabricant
1919-

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Date: 2007
Revised:



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I was born March 30, 1919 in Philadelphia, Pennsylvania. I grew up in the family of my maternal grandparents since my mother spent most of her adult life in tuberculosis sanitarium. As an only child, in fact an only grandchild and a sickly one to boot I was a pampered and precocious member of a large family. Since I had every childhood disease in my pre-adolescence I had every opportunity to develop a repertoire of educated T-cells which have probably contributed to my longevity.

I was educated in the Philadelphia city schools and graduated from Central High School, which in those days was a boys only school. I was fortunate during most of my childhood to live across the street from an extensive open area, Fairmount Park and therefore did not grow up entirely on paved city streets.

In 1936 I entered Pennsylvania State University and during my first semester decided to become a veterinarian. At the end of my second year at Penn State I applied and was admitted to the Veterinary College of the University of Pennsylvania in Philadelphia. I was able to live at home and commute to school by trolley-car. From 1938 to 1942 I was a somewhat diligent student of veterinary medicine. During this period my future career was significantly influenced by the professor who taught general pathology to the veterinary and dental students. Dr. Joseph McFarland M.D. professor of pathology gave a series of lectures that convinced me that my only satisfactory future lay in the area of experimental pathology.

By the time I graduated as a VMD in 1942, the United States was at war with Japan and Germany. Therefore I took a temporary position as a meat inspector with the U.S. Dept. of Agriculture. I was stationed in New York City where my first assignment was to a slaughterhouse situated where the U.N. building is now located. Over the next two years I circulated through all of the meat packers in Manhattan and Brooklyn. For a person with an interest in pathology there was much to be learned from this experience. For example, the presence of parasites or lesions was often insufficient evidence to make a significant diagnosis at a post-mortem examination. In 1944, I received a small inheritance, enough to enable me to go back to college. I enrolled at Penn State again to complete the few credits I still needed to get my B.S. degree. To help in my financial support I also worked part-time for the Veterinary Science department. Dr. W.T.S. Thorp who was then chairman of that department put me to work on several poultry diseases. This was my introduction to what would become my lifetime vocation.

During this period I was pondering the question of where to pursue future graduate work. After prolonged investigation I narrowed my choice to the University of California at Berkeley and Cornell University. My final choice of Cornell was based upon the absence of a long list of required courses and the emphasis on research for Ph.D. training at Cornell. This fateful choice influenced the rest of my life since once I migrated to Ithaca, New York I spent the rest of my life (except for sabbatic leaves) at the New York State College of Veterinary Medicine at Cornell.

I arrived in Ithaca in July 1945. The next day I went to the offices of the veterinary college. Dean Dr. William Hagan told me I would be a graduate student with Dr. Peter Olafson in veterinary pathology. Dr. Hagan took me in tow and introduced me not only to Dr. Olafson but to each and all of the faculty and graduate students at the time. One of these graduate students, Catherine Greci, was the graduate assistant in the laboratory of Dr. P.P. Levine, a relatively newly appointed Professor of Avian Diseases. This lovely young lady offered to show me around Ithaca and as a susceptible young bachelor I quickly accepted the offer. In three months we were engaged and in six months married. So quickly was my fate determined.

My first year as a graduate student was as a major in pathology with Dr. Olafson and a minor in poultry diseases with Dr. Levine. My research problem was a study of "Enterotoxemia in Sheep" which involved a series of unsuccessful attempts to reproduce the disease experimentally. On the other hand a field trial in prophylaxis of this disease by vaccination was a success.

When I completed the sheep studies and wrote a thesis for a M.S. degree, I switched my pattern and continued for a Ph.D. degree program majoring in poultry diseases and minoring in pathology. This was made possible when everybody agreed to let me take over my wife's assistantship in poultry diseases. She had already taught me how to use chick embryos for virus isolation as well as many other diagnostic techniques.

My research program on avian respiratory diseases soon narrowed down to its final form "The differential diagnosis of Newcastle disease and infectious bronchitis". Since this problem was of major interest to the New York poultry industry as well as my personal research I was furnished

with a laboratory technician as well as a caretaker for my experimental animals. The latter part-time post was filled by a veterinary student, Wilson Miller, who later became a full-time poultry veterinarian in Lancaster county, Pennsylvania and a well-known member of the AAAP. My second minor for my Ph.D. program was in poultry nutrition with Professor Leo Norris. This program broadened my exposure to general animal nutrition as well as biochemistry.

My research on avian respiratory diseases started out as an evaluation of the HI and SN tests for the diagnosis of Newcastle disease. A sudden and severe series of outbreaks of infectious bronchitis in laying hens in New York led me to extend my studies into techniques for virus isolation and SN tests for this disease. Thus did my Ph.D. thesis emerge as a study of differential diagnosis of these two respiratory diseases.

As I completed my graduate work I was offered an appointment as an assistant professor in poultry disease research. This was at the time one of only two full-time research positions in the veterinary college (previous incumbent Clifford Barber). This was just what I was looking for and I spent the rest of my working career in this slot. I was now responsible to Dr. P.P. Levine and we worked as a team for the next 20 years. Dr. Levine and I would periodically pick out a list of field problems from which we would select what we considered high priority items for study. Together we would organize research programs and with Dr. Levine's brains and my hard work try to solve the poultry industry's pressing problems. For many years our major area of interest was in respiratory diseases. Vaccination techniques and evaluation, egg transmission of disease, the carrier state and viral epidemiology were some of the areas studied. At this point it is important to acknowledge that our peers in this area, Drs. Delaplane, Beaudette, Van Roekel, Jungherr and Cunningham, were invaluable colleagues and competitors. Our communication over the years furnished valuable input to these studies.

In addition to these studies Dr. Levine and I were deeply involved in the early studies and description of virus hepatitis of ducklings. It was an enormous thrill in my early career to isolate a new virus since DVH produced characteristic changes when inoculated in chick embryos. We could, therefore, run SN tests and determine that the virus did not produce mortality except in young ducklings. However, older ducklings did become infected and produced SN antibodies in their serum. This enabled us to use serum collected from ducks at the time of slaughter for the passive immunization of susceptible ducklings. This was the control method until a procedure was devised for vaccination of breeder ducks to produce maternal antibodies to protect the ducklings.

The pattern of our respiratory disease research switched direction with the development of the broiler industry and the recognition of "chronic respiratory disease". My own exposure to this problem came when some poultrymen noticed that following bronchitis vaccination some flocks continued with respiratory symptoms for long periods of time. We had already established that infectious bronchitis (IB) was a relatively acute disease in which symptoms were not persistent. It was, therefore, concluded that we were dealing with a new infectious factor. My first successful attempts to isolate this organism were made using established techniques for cultivating psittacosis-like agents by chick embryo yolk sac inoculation. When we later learned we were really working with a pleuropneumonia-like organism (PPLo) we converted to a more suitable culture media. It quickly became apparent that chickens with this respiratory syndrome

harbored a multiplicity of different PPLO. Some of these were associated with disease and others seemed to be non-pathogenic. The problem now was to differentiate and characterize the pathogenic species. A great step forward in this research occurred when I was able to spend a sabbatic leave at the Veterinary College at Davis, California. Here I worked in a laboratory with Henry Adler and Richard Yamamoto. This collaboration did much to clear up the relationships between Mycoplasma species and disease. At this time two disconcerting facts emerged. First, the pathogen (Mycoplasma gallisepticum) was vertically transmitted from the hen through the egg. Secondly, essentially all of the major commercial poultry breeding stock was infected so that a test and slaughter eradication program such as was used with Salmonella pullorum was not possible.

The problem now became to find a method of producing Mycoplasma-free chicks from infected hens. Attempts at several laboratories to prevent egg transmission by antibiotic or other drug treatment of infected hens had not proven effective. Therefore, on my return to Cornell from sabbatic leave, Dr. Levine and I embarked on a concentrated study of the qualitative and quantitative patterns of egg-transmission using culture of individual trap-nest identified eggs of artificially-infected laying hens. This allowed us to study the duration and dynamics of egg-transmission and to gain evidence of how egg-transmission occurred. The bad news was that although egg-transmission could be reduced to a low level and the infection pattern was often intermittent, on a flock basis egg-transmission persisted throughout the entire laying period.

Our group investigated two different methods of preventing egg transmission of Mycoplasma gallisepticum (MG). My personal approach was prophylaxis by means of immunization of breeder hens. This could be accomplished by infecting the hens during their growing period (9-12 weeks of age) with a highly virulent live culture of MG. In this situation the breeders would recover from the infection before beginning to lay eggs. The eggs would be free of MG infection. This procedure could be consistently repeated but had two associated problems. First, it was difficult to produce an immunization inoculum except for immediate use since the inoculum could not be stored. Secondly, the inoculated birds were at high risk for secondary Escherichia coli infection during the period after inoculation.

On the other hand, Dr. Levine's approach was to treat the disease with effective antibiotic administration to the hatching eggs. This could be accomplished by "dipping" which entailed warming the eggs and then placing them in an ice-cold solution of antibiotic. The temperature difference between the egg and the solution allowed each egg to absorb enough antibiotic solution to effectively prevent infection in the hatched chicks. This method was effective and safer than immunization. Therefore, it was adopted for intensive study and field development.

At various times Bruce Calnek and Arnold Rosenwald participated in these laboratory and field dipping studies. Our field work was done on a large scale with the cooperation of Babcock Farms. Our first field studies involved treatment of parent breeders. When we proved that "dipping" could be effective at this level Babcock agreed to convert the entire breeding operation to MG-free status. Accordingly, after many meetings a detailed program was produced describing every step and eventuality involved. The success of this program was constantly monitored by extensive serological and cultural procedures. Interestingly, the essential cost of

the “dipping” and testing procedures was paid for by the improved levels of hatchability of the MG-free breeding stock.

During the same period my research program was also supported by NIH grants for basic studies on mycoplasma culture and characterization as well as the diagnosis and taxonomy of these organisms. These grants also provided facilities and support for graduate students working with avian and bovine mycoplasma. One of the major advantages of working with mycoplasma was the ability to be a pioneer in a relatively new and little understood area of research. It made it easy to do all sorts of original work and to quickly become an international expert in your chosen field of research.

Eventually mycoplasmas were no longer a major interest for NIH and funding in this area dried up. Fortunately, this did not occur until after we and other poultry research workers had managed to develop a successful eradication and control program for MG in poultry. It also became necessary to devise a method for prevention of egg production drops due to MG in multiple-age egg laying farms. This turned out to be possible by a modified immunization technique.

When my career as a mycoplasmaologist ended I entered a new career as a herpes-virologist. I joined Bruce Calnek’s group which was working on tumor viruses, at that time primarily on Marek’s disease virus. When Bruce went off on sabbatic I was assigned a fascinating project on the early pathogenesis of Marek’s virus infection. My collaborator on this project was Marius Ianconescu who was spending a sabbatic leave with our group. After this introductory plunge into the biology of tumor viruses I found my place in life as a co-investigator on my wife’s NIH project on virus-induced atherosclerosis.

My wife, Catherine Fabricant, had noticed, during her studies on the growth of a cat herpesvirus in tissue culture that the infected cell cultures produced characteristic crystals that she identified as cholesterol. This led Catherine to theorize that a herpesvirus might play an etiological role in atherosclerosis. A review of the literature supported this theory. She then put together an NIH grant proposal to test this idea. The model she selected was Marek’s disease herpesvirus infection in chickens. Since I was already trained in avian pathology and specifically also in the techniques for working with Marek’s disease virus, I became a co-investigator on this project. A second co-investigator was Dr. Richard Minick, a professor of pathology at the Cornell Medical College in New York City. He furnished the expertise in the analysis and description of the atherosclerotic lesions produced in our experiments. These lesions were identical by gross and microscopic pathology to those occurring in human atherosclerosis.

Previous attempts to demonstrate a relationship between viral infection and atherosclerosis had failed because of lack of suitable technical methods and inaccurate knowledge of the viruses involved. Our experiments had the advantages of clone-purified Marek’s disease virus (MDV) and specific-pathogen free chickens free of known viral infections. These chickens were of strains genetically selected for susceptibility to Marek’s disease virus and could be kept in suitable isolation facilities. All of these factors made possible the design of a carefully controlled experimental plan.

We were able to prove that MDV alone could produce classical atherosclerotic lesions in the anterior aorta, gastric, splenic and intestinal arteries and most importantly in the coronary arteries of infected chickens. Such lesions were not produced in uninfected control chickens or in uninfected chickens fed high levels of cholesterol in the diet. It was later determined that a significant part of the pathogenetic mechanism was alteration of patterns of cholesterol and cholesterol ester metabolism in the infected cells. This finding strengthened the hypothesis that a similar human herpesvirus might be an etiological factor in human coronary atherosclerosis.

With a few notable exceptions it took over 25 years for this finding to attract any notice from the human heart disease establishment. But before she died Catherine did receive recognition at an international meeting on infectious agents in atherosclerosis and had the resulting paper published in the American Heart Journal.

Although, Catherine and I retired in 1985 we continued these studies until 2000. When Catherine died in 2001 I still kept up my interest and some level of activity in the area of avian diseases.

Finally, I should perhaps comment on my reputation as a gadfly at various avian disease meetings. It was my custom only to ask simple informational questions or to question the validity of the methods used, the adequacy of control groups or the conclusions drawn from the data presented.

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