REFLECTIONS: by Victor J. Freeman

The following autobiographical notes focus on only one of the three careers I have had, namely that of bacteriologist. My first career (clothing salesman) was very much out of character for me. The reason I persevered with it was because, after losing two brothers to the legal profession, my father was not about to give up hope on his youngest son becoming the heir to his men's clothing business. Fortunately for me, father also wanted all of his sons to have a college education!

From the very outset of my work at the clothing store, I knew I was no entrepreneur. Although my feelings for that phase of my work life were distinctly negative, the experience was maturing and valuable. The incessant energies spent organizing, shelving and tabulating clothing inventory must have disciplined my compulsive character in a direction that probably facilitated the research work I conducted in my second career (bacteriologist). In any event, at college I soon became enamored with the biological sciences, and decided to major in bacteriology. During the summer just prior to my final undergraduate year, instead of returning to my job as salesman, I chose to break the research ice by studying infections of goldfish. Although my father's disappointment was very great, I believe I recovered some esteem in his eyes when, the following year, I applied to medical school.

My decision to study medicine was motivated by advice given me by my head professor at the University of British Columbia in Vancouver. He convinced me that my future in bacteriology depended on having an M.D. degree. Consequently, after obtaining my bachelor's degree in 1941 with honors in bacteriology, I entered the University of Toronto, School of Medicine at Toronto, Canada. Following graduation from medical school in February, 1945 (a speed-up program because of the war), a year's general rotating internship in Vancouver, and most of 1946 with the Canadian Army (instructing French Canadian soldiers in the subject: Hygiene), I finally was able to return to my real interest which was to do teaching and research in bacteriology.

My first postgraduate assignment was as an instructor in bacteriology at the University of British Columbia. I did some research with gas gangrene-causing anaerobes, but in that first year, much time was required of me in the teaching area. Before that year (1947) was over, an opportunity presented itself at the University of Washington in Seattle, in which I was able to set up a research laboratory of my own in the newly-established Department of Public Health &

Preventive Medicine of the University of Washington Medical School. The School had just expanded from a two to a four year program, and as a result had new buildings with adequate space and facilities. My career as a researcher in bacteriology finally was fully launched!

What determined my choice of research subject is a question I as yet cannot recall. I am certain I discussed choices with the very cooperative members of the Department of Bacteriology in the Medical School, but somehow I believe I was influenced by the persistence of clusters of cases of diphtheria in Seattle and King County, in spite of extensive vaccination programs. In any event I set out to investigate ways of typing strains of C. diphtheriae isolated from cases of diphtheria, and carriers of the C. diphtheriae bacillus. In addition to utilizing the standard typing methods involving the use of biochemical tests (e.g. fermentation) and serum agglutination, I decided to investgate the possibility of phage-typing the C. diphtheriae strains based on the established Salmonella typing scheme. To this end attempts were made to isolate diptheria bacteriophages, in order to analyse their specificity for use in culture typing.

Efforts to isolate stable diphtheria phages from my collection of field cultures proved unsuccessful. Consequently the only phages available for specificity testing were those obtained from Australia and Canada. No practical scheme resulted from the use of those bacteriophages, possibly in part due to the fact that the adapted phages that were produced, also were found to be unstable. I had to settle for producing a modified scheme for typing strains of C. diphtheriae without the use of phage (see my third paper of the "Serological Studies" series in The American Journal of Hygiene, 1952, 55: 74-82).

In the course of conducting in vitro toxigenicity tests of the C. diphtheriae cultures we had collected, a number of our proven avirulent strains produced positive arrowhead precipitates. This was observed prior to doing the bacteriophage investigations. At that time I believed there was "no good evidence that these strains are able to become toxigenic for animals (Pub. Health Reports, 1950, 65: 875-882)."

SERENDIPITY!

While carrying out investigations with the Canadian bacteriophage, I discovered it had marked lytic activity on several of the avirulent cultures. In view of this, and the result previously observed of positive precipitates on in

vitro testing, I decided to check on the possibility that a dermal necrotic endotoxin may have been released by the lysis. The phage lysates of the avirulent cultures indeed did reveal a dermal factor toxic for guinea pigs. Further testing proved the toxic substance to be true diphtheria toxin.

When the phage-treated avirulent cultures were tested for the presence of bacteriophage, all of the newly-rendered toxigenic strains proved to be lysogenic. Initially upon this discovery, I postulated a phenomenon of virulence conversion that resulted from the spontaneous development of toxigenic mutants, with selection by phage lysis. But subsequently I leaned toward a different hypothesis. I suggested in my final paper ("Further observations ....." J. Bact., 1952, 63:407-414) that "the bacteriophage may make possible the toxin production through some as yet undetermined association with the metabolic processes of the bacterial cell." In other words, that the conversion to toxin production of the avirulent C. diphtheriae strains, was due directly to their acquired lysogenicity.

Initially I had had definite doubts about the validity of our results in producing conversion to toxigenicity. But after confirming the conversion phenomenon with the use of single cell isolation technique, I was convinced. However, convincing my colleagues in the Department of Bacteriology was quite another matter. One die-hard professor persisted in questioning my work even after the single cell isolation results were in. I feel certain he was not won over until my research was replicated in the laboratories of independent researchers.

About the time the validation phase of the phage conversion research was completed, I received an offer from the Director of the Seattle-King County Health Department to become their epidemiologist. It was arranged that I would continue with my teaching role in the Department of Public Health & Preventive Medicine at the Medical School. I gladly accepted, since it was an opportunity to broaden my knowledge and experience in the Public Health field. I already had in mind that I would pursue postgraduate training in Public Health, a plan that met with the approval of the County Health Director.

Accepting the position of epidemiologist necessarily brought to a halt, the further pursuit of basic microbiological research. My decision in this regard was not a labored one. I knew that if I were to elect to continue studying the conversion phenomenon I had discovered, I would have had to undergo postgraduate training in biochemistry. My long-

standing wish to do my graduate training in Public Health prevailed. Furthermore, organic chemistry had not been a favorite of mine in medical school! I extrapolated to conclude that postgraduate biochemistry would not appeal to me.

MY THIRD CAREER

During my service as epidemiologist, only one instance of "biological" significance stands out now. That was an epidemic that was known popularly as "the Green Lake itch." Actually it was a mild, but annoying skin infestation that was caused by a schistosome, contracted by swimmers in the lake. It seemed that otherwise, most all of my work involved problems of a mental health nature. So many of the calls I received from the Public Health nurses turned out to be only indirectly infectious disease issues. The significant problems were psychiatric ones. A graphic example is one I shall never forget. The nurse had been called by a neighbor, complaining that a four year old child with chickenpox was running around outside (there were quarantine laws at that time!). At first I was puzzled that the nurse wanted me to accompany her on a routine quarantine violation for a minor infectious disease. But when we were admitted into the child's home, it became very clear. The dining room was lined around all four walls with gallon jugs of chlorine water. The child's mother was a seriously disturbed obsessive-compulsive who was so engrossed in sterilizing her furniture, she could not take the time to supervise her little son. During the approximately eighteen months I served as epidemiologist, there seemed to be an unending number of such problems.

As a result of such experiences, I soon was convinced that I needed to arrange for a minor in Mental Health while I was undergoing my postgraduate training in Public Health. However, when I discussed such plans with my Director, and also the Director of the State Department of Public Health, they both strongly urged me to change my plans, and instead to go for a residency in Psychiatry.

The idea of abandoning my plan to train in postgraduate Public Health did not sit well with me (I had already been accepted at the School of Public Health in the University of Toronto). Consequently I explored alternatives and came up with an apparent compromise. The former Surgeon General of the U.S.P.H.S. had just started a new School of Public Health in Pittsburgh, PA. With his cooperation, I was able to arrange a joint program to include training both at his school and at the Psychiatric Institute there (or at least so I thought!). When I moved out there (with wife and child), I soon learned that the psychiatry professor in

charge of the residency program was not about to cooperate with the joint arrangement. He insisted that so long as I was one of his residents, I could not take any formal courses in Public Health. I believe the reason I acquiesced to that dictum was because I had no viable alternative. Thus was launched my third career: Psychiatrist.

Initially in Pittsburgh I tried to keep up with some of the literature in the phage conversion area. However, the requirements of the residency soon became so demanding, particularly with reading assignments, that I lost sight of most everything outside of psychosocial subjects. I confess I became very fascinated with the psychodynamics of human behavior.

On completion of my psychiatric residency, I ended up serving two years in the U.S. Public Health Service at San Francisco's Marine Hospital, where I was Assistant Director of Medicine, in charge of the Neuropsychiatric Unit. On discharge from the service, I accepted an appointment as Associate Professor of Public Health Mental Health at Pittsburgh's Graduate School of Public Health. Even though I did not have my postgraduate degree in Public Health, I thought that at last I had attained the joint arrangement I had sought. I looked forward to working with the graduate faculty as their consultant in Mental Health. After all, the Directors of the State and County Health Departments in Washington State had assured me that once I had my psychiatric residency under my belt, I would be in great demand by public health officers generally.

I experienced great disappointment at the Public Health School. Instead of working jointly with the faculty to help them utilize psychodynamic principles in their areas of teaching (e.g. Maternal & Child Health), I was expected to isolate myself in a distant corner and research schizophrenia. I thought I was a public health professional. That self-concept was dealt a serious blow. After three very difficult years, I left the Public Health School to retreat into the role of practicing psychiatrist. That was 1960, and since then, until 1982, I have been in private practice.

In my third career, I went an additional step beyond that of general psychiatrist. I completed training as a psychoanalyst. The latter program, although fascinating, was arduous, as evidenced by the fact it took eight years of seminars and supervision before graduating from the Pittsburgh Psychoanalytic Institute in 1968. Although I still am an active member of a Psychoanalytic Institute (San Francisco), I always have been a maverick insofar as psycho-

analysis is concerned. I believe that is because I always have been, and always will be biologically oriented. Although I believe strongly in the process "unconscious", I nevertheless am convinced that intrapsychic conflict cannot be worked through in the absence of interpersonal involvement. To attempt otherwise would be like trying to solve the toxigenicity conversion phenomenon without dealing with the bacteriophage interaction!

So much for my reflections. Presently I am largely retired, doing only a minimum of industrial psychiatric consultation. I have been wondering how I might once again become involved with things "biological." It has occurred to me that I could go back to school and take some courses in genetics that might help me catch up on developments in the field of lysogenic conversion phenomena. It sounds intriguing, and thanks to Dr. Lederberg's incessant curiosity, I have been aroused!

Victor Freeman