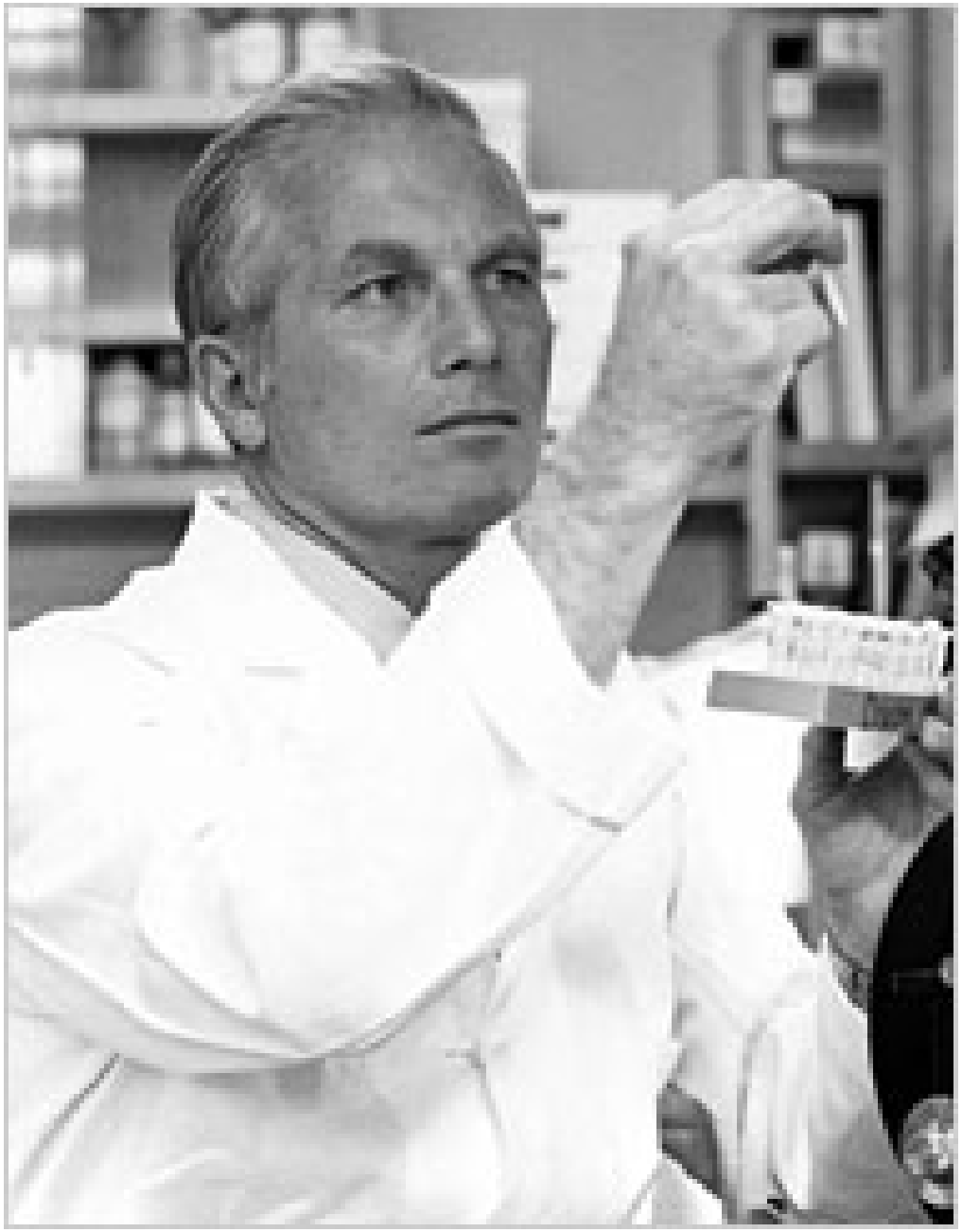
Frank J. Dixon, M.D.

1920-2008



Dixon was born in St. Paul, Minnesota in 1920, and completed medical school at the University of Minnesota (AOA).

After brief stints at Harvard Medical School's department of pathology and the Washington University Medical School in St. Louis, he spent 10 years as the professor and chair of the department of pathology at the University of Pittsburgh School of Medicine.

Dixon FJ, Moore RA

Testicular tumors; a clinicopathological study.

Cancer. 1953;6:427-54.

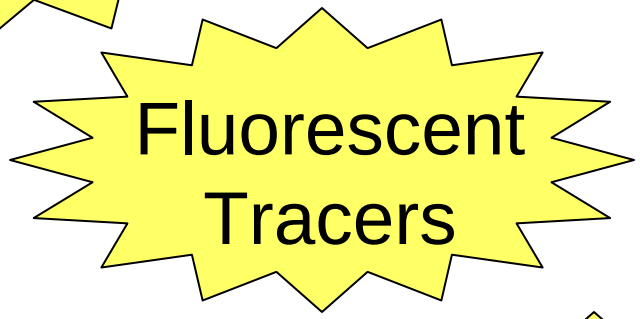
A classic in urologic pathology

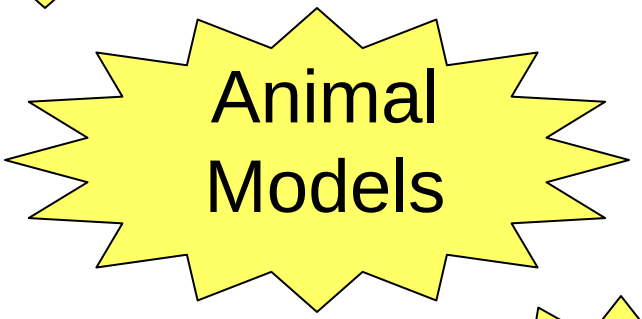
In 1975 he received the Albert Lasker Award (the “Americal Nobel Prize”) for Basic Medical Research:

“For his outstanding contribution to the creation of a new medical discipline, immunopathology.”

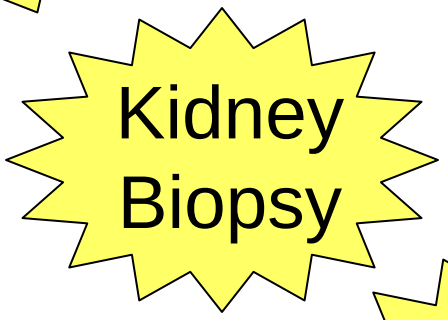
(in particular immunonephropathology)

1950s and 1960s

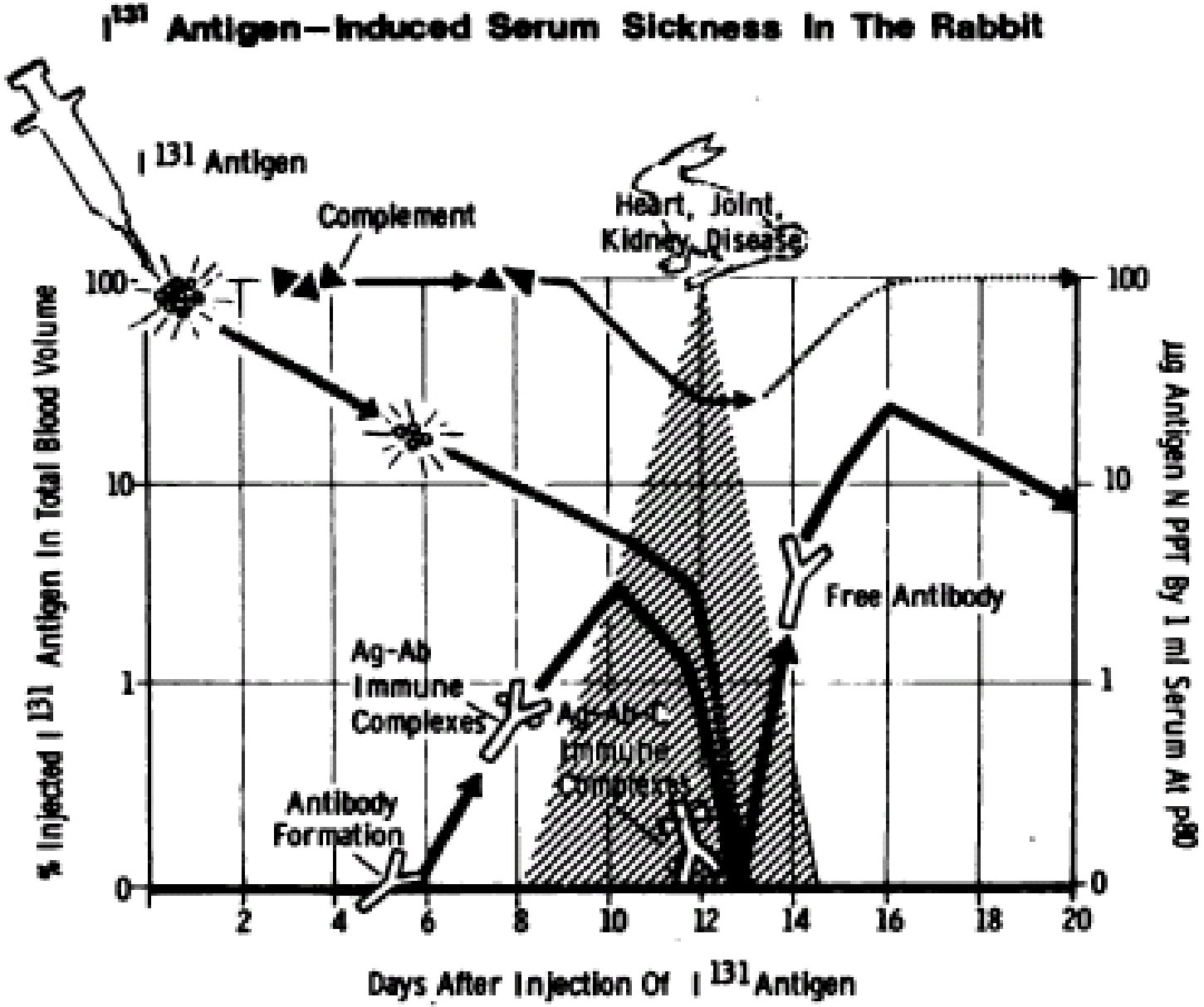




Stars were aligned for the study of immune mediated glomerular disease



The classic paradigm of immune complex mediated disease pathogenesis derived from multiple seminal studies by Frank Dixon and his associates



One of the many publications by Frank Dixon and his associates

That established the classic paradigm of immune complex mediated disease pathogenesis:

Pathogenesis of serum sickness

Dixon FJ, Vazquez JJ, Weigle WO, Cochrane CG

AMA Arch Pathol. 1958 Jan;65(1):18-28.

In 1961, Dixon and four colleagues moved to La Jolla and established the Department of Experimental Pathology at Scripps Clinic and Research Foundation, which was the nidus for the Scripps Research Institute, now with ~300 professors, 800 post docs, and 1,500 laboratory staff.

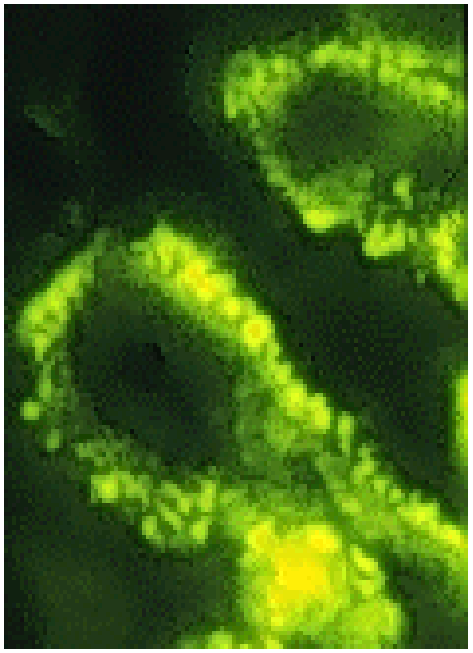
He was Director of Scripps Research Institute from 1961-1987.

Dixon's groups elucidated the basis not only for immune complex disease but also anti-GBM disease:

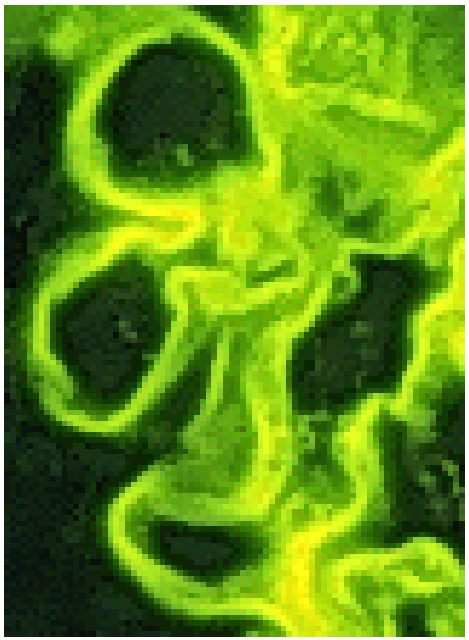
ANTIBODY MEDIATED GLOMERULONEPHRITIS

"Lumpy Bumpy"

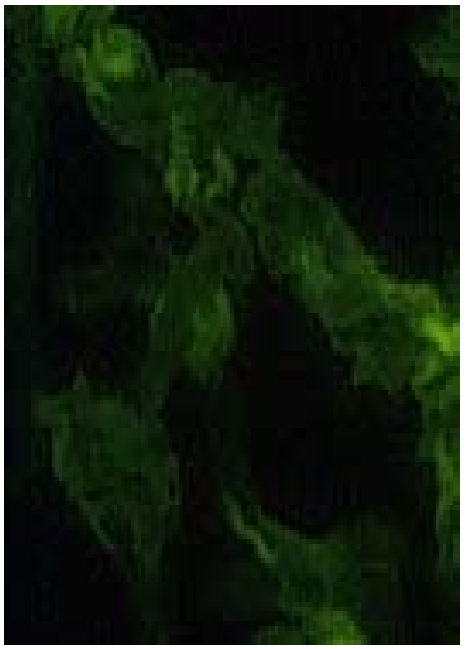
Granular
glomerular IF staining
for immunoglobulin



Linear
glomerular IF staining
for immunoglobulin



Paucity
of glomerular IF staining
for immunoglobulin

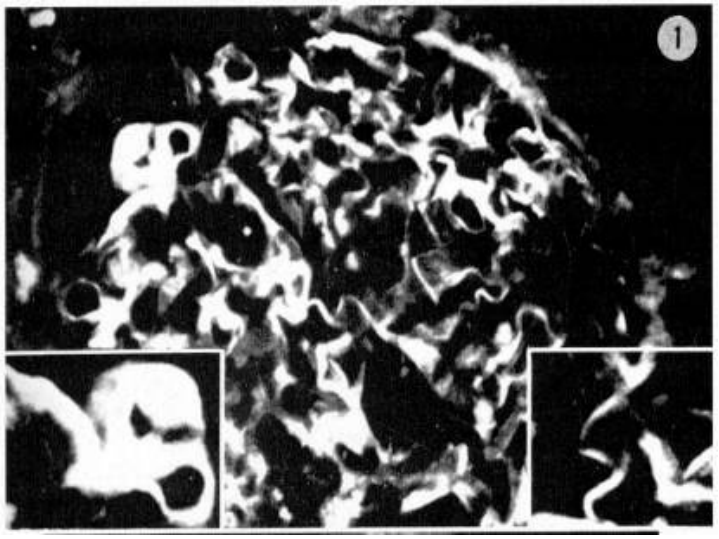


The Role of Anti-Glomerular Basement Membrane Antibody in the Pathogenesis of Human Glomerulonephritis

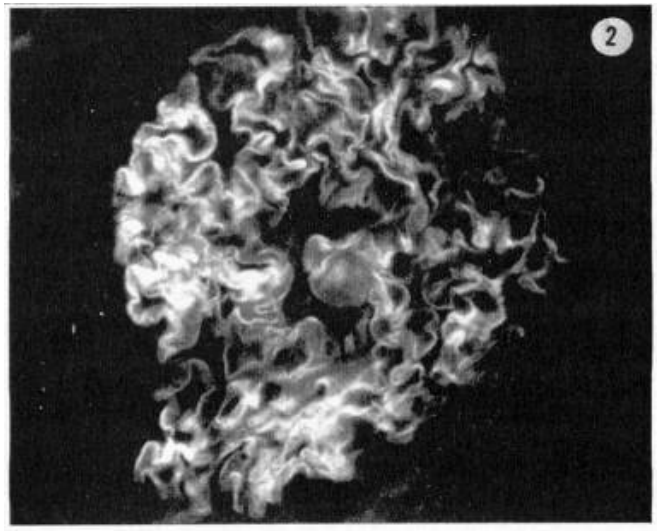
R. A. Lerner, R.J. Glassock, Frank J. Dixon

J Exp Med 126:989-1004, 1967

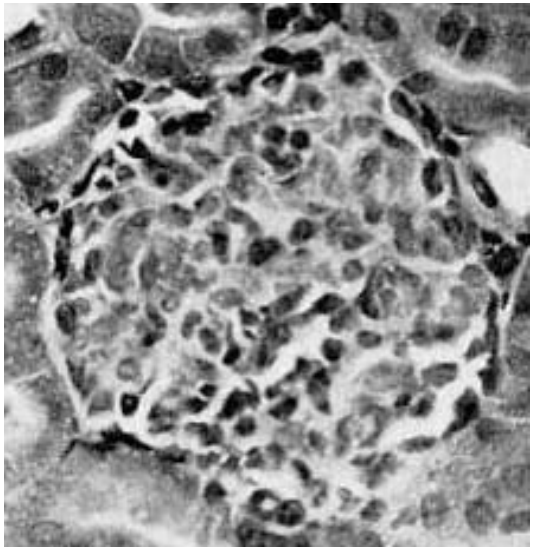
Direct IF of patient tissue

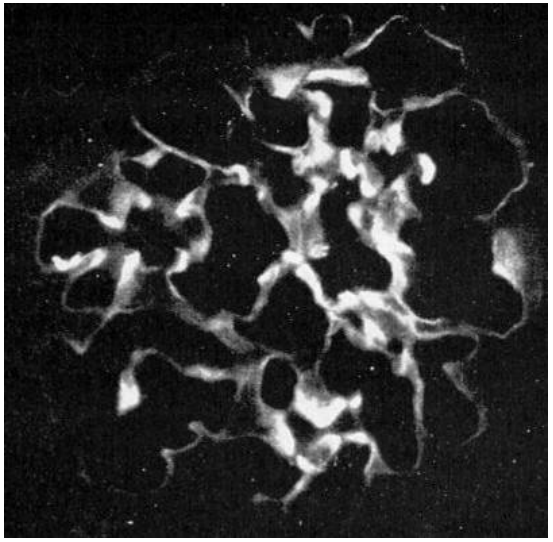


Indirect IF of normal tissue using eluate



Monkey kidney 24 hours after injection of eluate

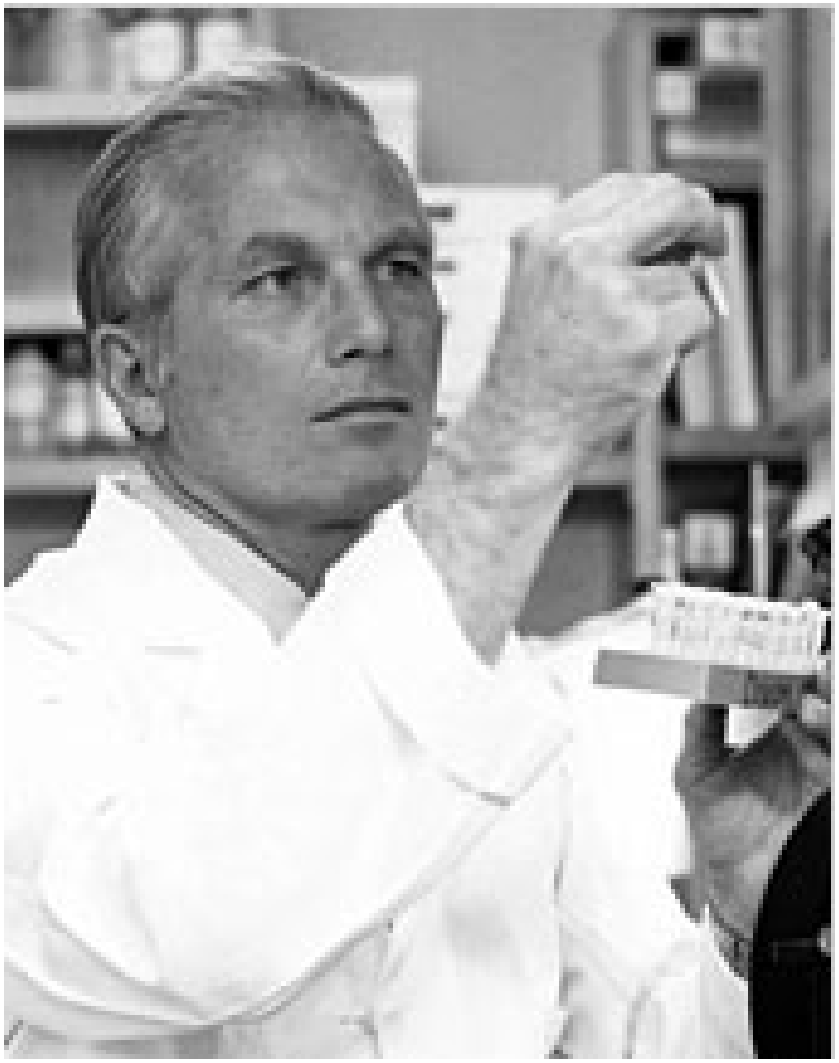




In his 1979 presentation of the Rous-Whipple Award to Frank Dixon, Barry Pierce described the excitement of Dixon and his colleagues while discovering new knowledge about kidney disease:

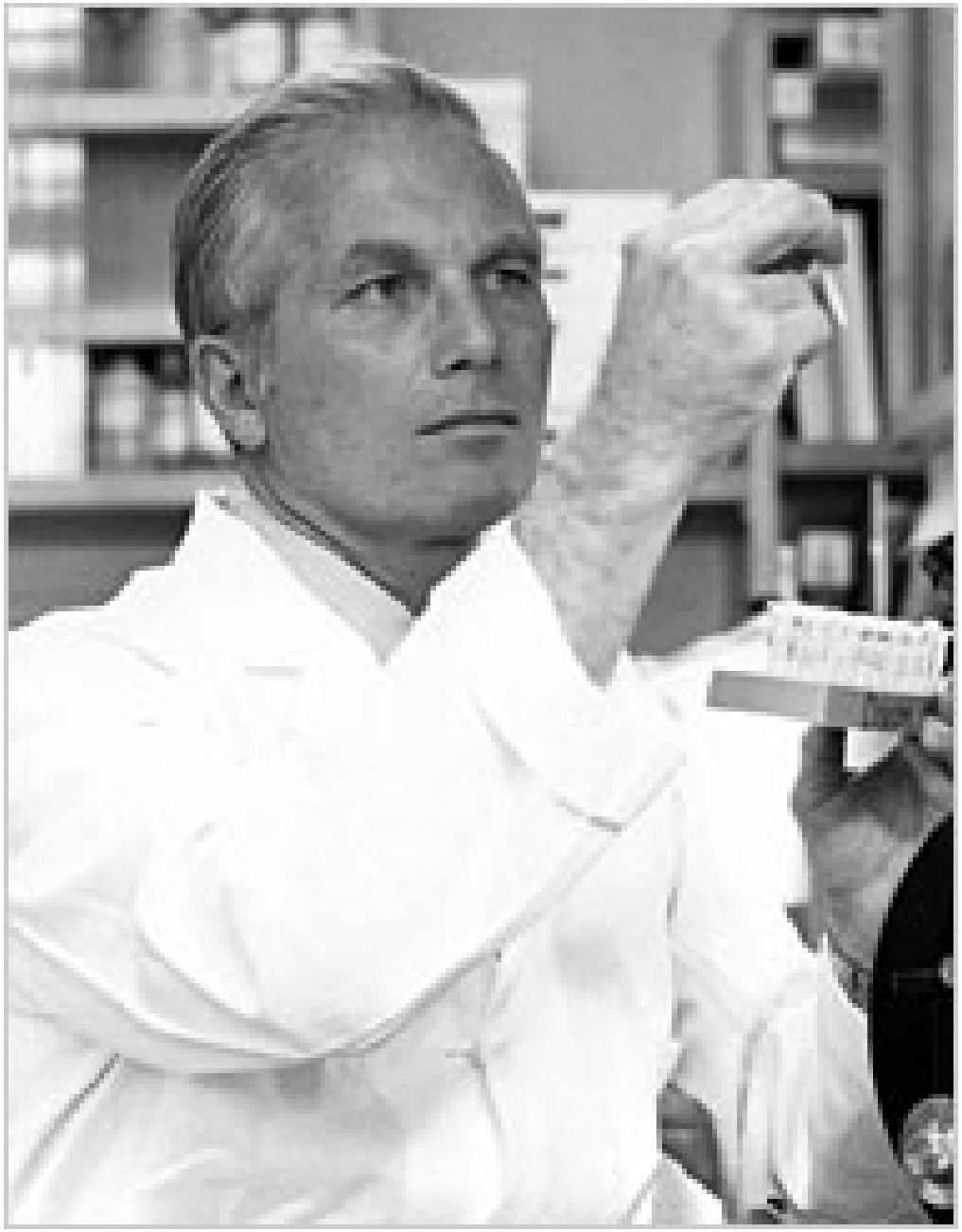
"For a time, these men knew something that no one else in the whole world knew."

"They had laid the scientific basis for the understanding of clinical nephritis."

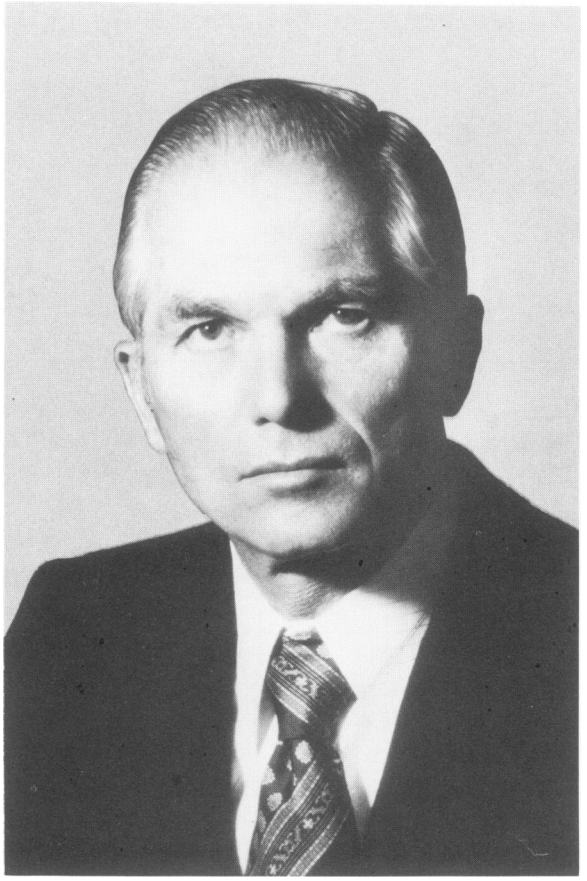


Frank J. Dixon, M.D.

1920-2008



ROUS-WHIPPLE AWARD



FRANK J. DIXON, MD

The American Journal of PATHOLOGY

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Presentation of the Rous–Whipple Award to Frank J. Dixon

1979

THE COUNCIL of the American Association of Pathologists decided, two years ago, that the Rous–Whipple Award, which is the highest scientific honor of the Association, should not be just another memorial, but in keeping with the tradition exemplified by the long careers of Dr. Rous and Dr. Whipple—a lectureship awarded to a distinguished working scientist. The Rous-Whipple Lecturer of the American Association of Pathologists this year is Dr. Frank J. Dixon, Jr.

It is an honor to make this presentation to my former mentor. I spent six exhilarating years, first as a fellow, and then as a junior faculty member on his staff, in the Department of Pathology at the University of Pittsburgh. Retrospectively, this experience provided me a vantage point from which to view a remarkable scientist in his formative years.

It was 1955. Professor Dixon was just 35 years old, had been chairman of the department for three years, had been the first to apply isotopic techniques successfully to the study of immunology, specifically immune elimination of antigen, and had studied the world's largest number of cases of testicular tumors, evolving the working classification most widely used even today, a quarter of a century later. In spite of these triumphs, Dr. Dixon was fully recognized by neither immunologists nor pathologists. In case there is any misunderstanding among us today, let me assure you quickly that the identity problem was theirs, not his.

The power of isotopic techniques, when added to the specificity and sensitivity of immune reactions, led to a mushrooming of information in immunology. The impact of this accomplishment was unanticipated by classical immunologists, and the powerful new technology was viewed with suspicion. These suspicions were overcome by the sheer weight of scientific evidence provided by Dr. Dixon, Dr. Talmadge, Dr. Maurer, and Dr. Weigle.

Presented at the Annual Meeting of the American Association of Pathologists (FASEB), April, 1979, Dallas, Texas.

0002-9440/79/1010-0003$01.00

Acceptance by traditional pathologists was also relatively slow in developing, even though Dr. Dixon had an impeccable professional pedigree. He was graduated with an MD from the University of Minnesota in 1943, at the age of 23. After service in World War II, he served a residency with Shields Warren. His first faculty position was with Robert A. Moore. The Armed Forces fascicle entitled "Tumors of the Male Sex Organs," coauthored with Dr. Moore, is a brilliant account of the natural history of the disease. They made postulates in it about germ-cell tumors that were ridiculed at the time, but all of them have been verified by direct experimentation. This classical clinical research has opened a new approach to neoplasia.

Recognition in pathology came quickly when Dr. Dixon, Dr. Vazquez, and Dr. Cochrane adapted Coon's technique to bridge the gap between the rapidly developing science and diagnostic pathology. The fuse of the immunologic bomb had been lit, and the explosion that was to take place occurred largely in departments of pathology, primarily as a result of Dr. Dixon's leadership.

His was a unique department of pathology! It had a single objective; a single deity was revered. That deity was Excellence. There was no room for mediocrity. If you jogged, played squash, caroused, played hookey from teaching assignments to go to the baseball games, or even did experiments or practiced pathology—you struggled for the maximum attainable. It was taken for granted that everything would be done with grace and excellence.

I remember when Bill Weigle and Charley Cochrane first induced nephritis by perfusing rabbits with immune complexes. What joy! What joy in accomplishment! Then Dr. Dixon proved that neither good antibody producers nor nonproducers developed nephritis; rabbits that developed nephritis were poor antibody producers in constant antigen excess. For a time, these men knew something that no one else in the whole world knew. They knew it because they had asked a question, they had developed a protocol, and they had done the hard work themselves. They had laid the scientific basis for the understanding of clinical nephritis.

A premium was never placed upon ideas in Dr. Dixon's department. The trick was to determine which idea to prosecute, which to put on the laboratory bench. Dr. Dixon's scientific taste and judgment were unequaled. If you wanted to show scientific discipline to the medical students, you didn't assign library reviews, as is so commonly done; you walked into a laboratory, any laboratory, and discussed the operative protocols. There was no secrecy. A premium was placed on the individual with the courage to do the critical experiment.

The physical plant was abominable. For example, Joe Vazquez did his fluorescence microscopy in the dark well under the staircase. Competition was keen; camaraderie was high. Friends joined forces and worked late into the night to help each other in the solution of problems. Interesting and important things were learned, some reflecting the inadequacy of the old building. For example, Bill Weigle learned, late one night, that the vessel serving as a soup tureen by day could also serve, for someone else, as a urinal by night.

It was a rollicking, freewheeling group, constructed in the image of Dr. Dixon. Fun was a keynote. The marvelous parties, especially those in the gracious Dixon home with Marion presiding, reflected the closeness of the friendships. And the picnics, baseball, kickball . . . I remember vividly the day Dr. Dixon hit a single and stretched it into a double with a victorious slide into second base. A rainbow formed quickly in the sheet of muddy water that sprayed into the sky. Unknown to Dr. Dixon was the fact that second base was a hole filled with water. After 22 years, the true facts can be revealed. Joe Vazquez placed second base in the mudhole.

Back to the laboratory. Dr. Dixon and Dr. Feldman designed a most innovative, problem-solving course for the sophomore students in pathology. The magic of this course centered around the main question ever asked in the department—"Why?" The students loved it, and the faculty vied for the opportunity to teach it. Residency training was not forgotten. Dr. Moran, Dr. Totten, Dr. Fennell, Dr. Fetterman, Dr. Sherman, and Dr. Fisher formed the outstanding faculty. Dr. Dixon led the weekly gross conference. His powers of observation and deduction, and the clarity of his presentation, held audiences in awe. People cringed when he showed a lesion and queried, "Now what do you make of that?" It meant that an error was about to be rectified.

By 1957, recognition flowed rapidly. Dr. Dixon was the first recipient of the Parke–Davis Award; nine of his students and associates have been so honored subsequently. His students form a veritable "Who's Who" of distinguished immunologists, pathologists, and clinicians. He was one of six charter members of the Pluto Club, a serious reaction of young scientists to the establishment. It was an effort to recognize and promote excellence in pathology. With Bob Good, Sam Bukantz, and Dan Campbell, he started the Hagfish Society, a reaction to problems of academic life. The Hagfish Society met annually during the "Federation Meeting," and in a spirit of conviviality, conducted business in not too typical academic style, but with typical results.

The harassment and restraints of academe eventually became overriding; and in 1961, Dr. Dixon moved to the Scripps Clinic and Research

Foundation, where his genius as a leader and developer of young scientists could be expressed. Scripps became a scientific Mecca. There, his successes in studies of autoimmunity and cancer are well known and require no reiteration. His personal honors are many, but the greatest of these is not named, has neither a medal nor a certificate. It is that special feeling that we, his F_1 and F_2 generations, carry in our hearts for him.

We gather today to salute you, sir, and present you with the highest scientific honor of our Association. You do us honor in accepting this Rous–Whipple Lectureship.

G. Barry Pierce, MD

Acceptance of the Rous–Whipple Award

Frank J. Dixon, MD

Let me say that this is indeed a happy occasion for a number of reasons. First, because this recognition comes from members of my own discipline and you have chosen to recognize the efforts of our laboratory now a second time. Second, because of the names of the eminent pathologists Peyton Rous and George Whipple, which this award bears—particularly Dr. Whipple's name, since it was his pioneering work on protein metabolism that provided important leads for us in our early attempts to analyze the metabolic aspects of the immune response. Third, because the presentation is made by one of my first research associates, Dr. Barry Pierce, whose comments were perhaps too kind but appropriately nostalgic. At the age of recipients of this award retrospection can be both pleasant and even profitable. Fourth, because it gives me an opportunity to recognize those who got me started in this business. Drs. Olive Gates, Shields Warren, Bill Meissner, and Robert A. Moore gave me the background and the tools necessary to begin a career in investigative pathology. Also, the late forties were good years in which to enter research. The research program you are recognizing began during the period of NIH growth, when research funds were in a happier state than they are today. Finally, and most important of all, I would like to recognize a series of extraordinary research fellows and scientific cohorts who have been associated with our laboratory over the past 30 years. These people have applied their brains and energy to the problems at hand and have created a lively and stimulating atmosphere in which, for me at least, it has been a pleasure to work. These, then, the teachers, the supporters, and the associates, are the essential elements in any successful scientific enterprise.