An Approach to the Study of the Pulmonary Circulation

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Thou shalt have no false gods before thee! Some of mine have been godlike, some stern, some even cruel, but none have been false. Although, like the great lawgiver, none actually walked upon the Canaan of the pulmonary circulation, whether wittingly or not, they led me step by step in an uninterrupted path to that study. First was my father, a Tulane-educated general practitioner, who was wont to remind me of my great disappointment with kindergarten “because they don’t teach no doctor stuff there.” Then there was his father, an old-world Hebraic scholar, litterateur, and moralist, who, with my father, had miraculously escaped the Odessa pogroms. He spent most of my childhood attempting to inculcate me with his classical concepts of faith, morality, and justice. Here, I first came to be electrified by ideas and abstractions. In college came a standard platter of American education, food but no feast—some classics, some literature, a bit of science. Then I was off to medical school.

The war with Japan and Germany had just commenced, and the pace of medical education quickened, leaving little time for concepts to germinate. Nonetheless, those 36 uninterrupted months were ample time to elevate new gods. Dr. Richard Ashman, chairman of physiology, delivered the first 20 of the daily physiology lectures. Electrophysiology was his major interest, and one of the several phenomenan he described still bears his name.

Sitting in on those lectures were Dr. George Burch, himself soon to become widely published in electrocardiography, and Dr. Sam Levine's nephew, Harold Levine from Boston, a well-known cardiologist in his own right. The curriculum was rushed, and for many reasons these electrophysiology lectures were complex. Moreover, we had not even studied cardiac anatomy. In one of Dr. Ashman's lectures I had failed to identify correctly the electrocardiographic appearance of atrial flutter. In a subsequent discussion, the great electrophysiologist was far more concerned with goading his errant student into formulating a concept that would explain how continuous atrial electrical activity could usurp sinus node function. With my guess that a faster irritable focus depolarizing at 250–300 times a minute could do it, Dr. Ashman was off with a host of experiments he and I could do to further test this widely accepted concept. He suggested that we go off to his laboratory—“Never mind the exam, there will be others and you’ll do well.” Many years later my wife pointed out that the intensity and dedication I learned as a child had not altered, but I had substituted medical science for theosophy and humanism, and the former had become my new “religion.” Electrophysiology, electrocardiology, and cardiac arrhythmias were challenges that came along in rapid succession while working with Dr. Ashman.

Further along, my time at medical school was largely consumed with new icons, each with special cardiovascular skills. Dr. Edgar Hull, the medicine chairman, was an unquestioned master of bedside medicine and cardiac auscultation. At Charity Hospital in New Orleans, patients took to him immediately because he knew their hometown physicians. In fact, he seemed to know every physician who practiced in every parish in Louisiana. He had taught most of them. His was a practical approach. He sought the correct diagnosis to the extent possible but got the patient back to his family and work as quickly as possible. He had an enduring but quiet concern for his house staff members, not only during their training at Charity Hospital, but also throughout their careers. He once postponed an important staff meeting because, as I learned years later, he had gone to northern Louisiana to help reconcile a former cardiac fellow and his wife. His student examinations were always oral, straight from Cecil’s textbook, and he enjoyed “helping you out a bit” when you faltered. There was also Dr. Robert Bayley, a former cardiac fellow under Dr. Frank N. Wilson of Ann Arbor. He, like Wilson, had a remarkable talent for mathematics and applied it skillfully and often to electrocardiology. He had devised what has come to be known as the triaxial reference system. Medicine “porch conferences” (porches had ceiling fans) were vibrant with clinical cardiology, electrocardiology, cardiovascular physiology, and the invariable “let’s see your data” or “can you give us the mathematical proof.” Somehow the other areas of medicine got much less attention.

Undoubtedly the foremost figure in my early progress was Dr. John Samuel La Due. He had done medicine at Harvard, internal medicine at Mayo, and a Ph.D. at Minnesota. He had only recently turned all of his enormous energy to cardiology, joining with Drs. Ashman, Hull, and Bayley. With the fortunes of alphabetic student assignments and a high class failure rate, John La Due was my rounding man all through medical school. One soon came to emulate his penchant for thorough historical inquiry, complete physical exam, and an unabridged write-up. Before case presentation, it was wise to spend the evening in the medical library acquainting oneself with current literature related to the patient’s illness. If all went well, the ultimate question was always, “Well, what have you read about this illness lately?” He wasn’t one to let you off the hook easily. Two examples are memora-
ble. As a second semester student visiting the wards for the first time, I was asked what the “Robinson-Power-Kelper test” was. “I would have to go to the library; it wasn’t yet in Cecil’s text.” La Due smiled, “Yes, that’s a good idea.” As we commenced discussion of another patient, he looked up at me rather surprised and shouted “Now!” On another occasion, Dr. La Due prodded a less than ardent medical student to study more and more thoroughly the blood of a patient with unexplained anemia. On the great day, Dr. La Due finally received all the hematologic data and asked for the student’s appraisal of the anemia. “I don’t know, Dr. La Due, but you’ve had me take an awful lot of blood out of her.” This was one of the few times John La Due was ruffled.

The experience I acquired over three academic years with John La Due left an indelible mark on my approach to medicine and science. “If you don’t know and it’s not in the library, you have a golden research opportunity,” he would say. Although I worked with him on the wards, in the clinics, and in his research laboratory, it was only years later that he expressed any appraisal of my work or productivity. Some seven or eight years later, I saw John in the lobby of the Los Angeles hotel hosting the American College of Cardiology meeting. He insisted that I come immediately to a private reception honoring him upon his election to the presidency of that group. At that reception, I was introduced to the cognoscenti of cardiology and the new specialty of cardiovascular surgery as “one of the best damn medical students I ever contended with.” The libations and atherogenic refreshments were lavish, and John had probably partaken heavily of both. With Dr. Ashman and Robert Bayley, La Due perfected the concept of electrocardiographic vector analysis, developed the ventricular gradient of F. N. Wilson, and from the QRS loops identified the effects of anatomic rotation of the heart on the standard ECG. Indeed, my first publication was with Dr. Ashman, demonstrating that the anatomic axis of the heart, as seen fluoroscopically, could be predicted from the ECG taken in the same position. La Due went on to New York, where, with Wroblewski and Karmen, he was the first to publish the use of the cardiac enzyme (SGOT) in the diagnosis of acute myocardial infarction.

Shortly before I received my medical degree, my father’s brother, a colorful gynecologist at Cedars of Lebanon (now Cedars-Sinai), Los Angeles, suggested I learn his specialty and join him in his practice. This digression was short-lived. My professor of obstetrics knew me well, having delivered me about twenty-one years before. He agreed to give me an obstetrics residency if I would first do a rotating internship at Charity Hospital. His conditions were clear: take as many specialty blocks as I could, except obstetrics, and if I still wanted the obstetrics residency at the end of that year, it would be mine. My first assignment was internal medicine, with Drs. Hull, La Due, and Bayley as my clinical staff and Louis Levy, another ardent follower of Dr. Ashman, as my resident physician. Professor Graffagino, Uncle Earl, Cedars, and Los Angeles became a pipe dream (and we used pipe cleaners in those days to project vectorcardiographic loops). It was clear, however, that before embarking on a training program in cardiology, it was necessary to complete that rotating internship and three more years of general internal medicine. My second rotation was onto Dr. Alton Ochsner’s surgical ward with a young resident from Philadelphia, Oscar Creech. There was a new surgery, tying off a patent ductus, or turning down a subclavian artery, to the pulmonary artery for Fallot’s tetralogy. Oscar was disappointed, seeing that I was much more interested in the cardiovascular and hemodynamic derangements than in the mechanics of surgical repairs. I did the forbidden obstetrics month, for the most part to see first hand
how a physiologic volume overload affects healthy people as well as those with rheumatic and congenital heart disease. Alcohol was the only pharmacologic diuretic known then.

The next year was spent at the University of Cincinnati. Dr. La Due had urged me to spend a year with Professor Marion Blankenhorn to learn a more disciplined approach to internal medicine. Dr. Blankenhorn had made many early observations on beriberi heart disease. His son, David, was an excellent fourth-year student on our wards, and we have been friends over the years. He went to the University of Southern California and became interested in atherosclerosis. About halfway through the first-year residency, I was invited to give a cardiology seminar on this new business of vectorcardiography. During the year-end holidays in New Orleans, Richard Ashman took me into his laboratory and his home and amply prepared me for the seminar. Noble Fowler, a senior resident, attended and showed interest. We talked many times during that year. Indeed, after a short visit to Bellevue, he started this “new thing,” cardiac catheterization in the heart station at the General. Soon I had learned what little was known about it in 1946. I returned to Charity Hospital for two more years of internal medicine residency. Shortly into that second year, a patient thought to have bacterial endocarditis on the pulmonary valve appeared. It seemed best in those days (1947) to culture blood drawn from the pulmonary artery. A cardiac catheter somehow appeared quickly. Since the only horizontal fluoroscope available was in the tuberculosis hospital unit, the catheterization procedure was done there. There were no strain gauges in those days. One of Dr. Ashman’s jerry-rigged smoked-kymographs with a calibrated rubber tambour measured pressures as the fluoroscope guided the catheter up. This was the first heart catheterization at Charity Hospital! The cultures were negative, but we established the diagnosis of pulmonary stenosis with the catheter and kymograph.

Somehow this seemingly minor event did not pass unnoticed in the staid community of Charity Hospital. I got a strain gauge! What’s more, on his retirement, Dr. Ashman gave me his prized double-channel Cambridge string galvanometer with the two quartz spare strings and rapidly moving photographic paper. He had had it uniquely designed to take not one, but two ECG leads concurrently. One channel was converted to use with the strain gauge, permitting measurement of one pressure change concurrently with one ECG lead. One channel was standardized against a column of mercury and the other with a DC voltmeter each day. It was large, about the size of the fluoroscopic table, but was a distinct improvement over the smoked kymographic drum and rubber tambour. On the other hand, its quartz strings broke easily, were difficult to insert between the magnet heads, and were expensive. My assignment in the internal medicine residency program somehow became concentrated on the cardiology clinic with the new bright-line fluoroscope (only 5.0 m.a.!), the ECG reading room with Dr. Ashman, and, of course, the catheterization procedure on selected patients seen in cardiac consultations or clinics. Dr. George Burch became interested, and soon we were occasionally studying Tulane patients as well. Dr. Nelson Ordway joined the Louisiana State University’s pediatrics service and introduced the Roughton-Scholander glass pipette to measure blood-oxygen content. Pediatric patients from both schools came along, and we grew busy. There were no standards, no procedural texts, no ECG oscilloscope, no defibrillation, no surgery standby. We flew by the seat of our pants, but fortune smiled, and no one was injured. Perhaps the exception was the hair on my hands, which was lost. I was able to
An Approach to the Study of Pulmonary Circulation

get a “bright-line” fluoroscopic screen. Still 5 m.a. T.V. screens, remote or otherwise were unknown!

In the heart station there were new and exciting sessions. Dr. Frank N. Wilson, who first described the precordial electrocardiogram, began coming quarterly to visit his daughter in New Orleans. Dr. Franklin Johnson, his close associate, sometimes accompanied him. Wilson’s student, Dr. Demetrius Sodi-Pallares, now chief of electrocardiography at the National Institute of Cardiology, Mexico City, also came. Sodi’s New Basis of Electrocardiography in 1956 was a classic. He was later to advocate glucose, potassium, and insulin for acute myocardial infarction. With Dr. Ashman reading ECG’s in the Charity Heart Station and Drs. Wilson, Johnson, Sodi-Pallares, Bayley, La Due and Hull doing commentary, those were some heady sessions for a medicine resident. Nonetheless, the new potential for hemodynamic studies began to occupy more and more time. Dr. George Burch, who had also embarked on a notable career in electrocardiography, invited me to the Tulane cardiology functions, and I came to know his cardiology staff. On the Tulane surgery service, Dr. Michael De Bakey and his resident Oscar Creech showed great interest in these cardiovascular conferences. The Taussig-Blalock procedure for Fallot’s tetralogy had been described at Hopkins, and eventually the first edition of Helen Taussig’s book on congenital heart disease appeared. Maude Abbott’s book was largely a pathologic anatomy treatise. Cournaud’s short book hadn’t appeared yet.

I came to know Dr. Burch even more in the third year of the LSU internal medicine residency. Once his secretary called me from Charity Hospital to see to him in his Tulane medicine department office. Something was surely wrong! He explained to me that a young instructor in English had transferred from the School of Arts and Science at Michigan to the one at Tulane. His wife, who had Fallot’s tetralogy, was under the care of Franklin Johnson and F. N. Wilson at Ann Arbor. Dr. Burch showed me the rather complete letter he had received from Ann Arbor and asked me to examine his patient in the elite doctor’s infirmary at Charity. I was impressed with this attractive young blonde, lying in bed, sparsely clothed, reading Tolstoy’s book. After my examination, I returned to Dr. Burch’s office armed with Taussig’s new text. I confidently assured him I would put a catheter retrogradely through a patent ductus. “A tetralogy has equal distribution of extremity cyanosis, but this young woman has differential cyanosis with pink fingers and blue toes.” Burch cautioned that x-ray documentation of the catheter in the ductus would be required. “But Dr. Burch, I’ve never seen such an attractive young blonde so scantily clothed in Charity Hospital.” The Catholic nuns have always maintained strict decorum. “What’s more, she was lying there reading Peace and War.” “Al,” he replied, “Tolstoy’s book is called War and Peace.” I paled, but his consummate smile told me a new icon would arise from an unlikely source, Tulane. The x-ray was dutifully brought to his office; he thanked me and kept it. Years later I came to learn that he had sent the film to Ann Arbor and gently chided the doctors there about bedside examination and the need for a university catheterization lab! Years after that I came to truly understand that the judge and the executioner never dine together.

After the rotating internship and three years of “internal medicine,” I was now preparing to undertake a cardiology fellowship. Dr. Ashman had me placed with Dr. Louis Katz at Michael Reese Hospital, Chicago, but he wanted most that I set up a catheterization laboratory, not a new experience for me. Drs. Langendorf and Pick were superb students of arrhythmia,
but that didn’t seem right. Dr. Hull wanted me to go with his friend Sodi-Pallaris in Mexico City, but there was a language barrier. I’d only learned Cajun French at Charity. Dr. Burch had another idea. He had a great interest in systemic veins, because “that’s where most of the blood is.” He’d written one of his famous primers on venous function, which had been well received. I had studied some of Professor John McMichael’s papers dealing with the concepts of digitalis affecting primarily the systemic veins but knew little about how veins controlled heart function. I asked Dr. Burch about this work, and he immediately suggested I go to London and study with Professor McMichael. In June 1948, Dr. Burch and I composed letters to him. About November, I had taken a week to cruise to Havana, and, while there, visited Dr. Castillano’s new angiocardiographic laboratory, one of the earliest in this hemisphere. As my boat moored on the bank of the Mississippi River, Dr. Burch’s secretary pulled me off and into his office. Professor McMichael had come to Tulane to deliver the John Musser Memorial Lecture and was sitting at Dr. Burch’s desk waiting to interview me. He explained that he had place for one American, and Dr. Epstein from Yale had been appointed. A problem arose regarding American Board of Internal Medicine credits for studying in London, and Dr. Epstein was going to stay in the States to complete those requirements. If I had no such constraints, he would accept me. Dr. Burch arranged one of the first NIH Traveling Fellowships for me. I was on my way to London in June 1949.

London may be part of the Old World to Americans, but to me Hammersmith Hospital was an exciting new world. Professor McMichael was surely an icon who strived to reach a balance between man and his instruments, who leaned heavily on experience and less on statistics, and who had the skill to use bedside medicine and keep advanced technology in proper perspective. He was eager for me to visit his own Edinburgh and other British and continental hospitals. “One doesn’t come to Europe every day,” he was fond of saying. At 8:00 a.m. on the first day, I went to the hospital, only to learn that the British start their laboratory at about 10 a.m. and carry on until 8 p.m. Professor McMichael took me from firm to firm (subspeciality sections) introducing me to the people with whom I’d be working, a remarkably talented group, each of whom was destined for greater tasks. Professor Paul Wood, an Australian, was the rounding man on the largest cardiac ward at Hammersmith. He was an excellent clinical cardiologist, a good teacher, somewhat dogmatic, and very self-assured. He was friendly from the outset and showed me the chapter dealing with electrocardiography in his forthcoming book *Diseases of the Heart and Circulation*. He was well acquainted with the work of F. N. Wilson, Richard Ashman, Robert Bailey, and the other vectorcardiology group. John Goodwin came the same year I did. He was interested in cardiomyopathy and devised a manual compression system to deliver radio-opaque material down a cardiac catheter. His publications on cardiomyopathy are widely known. He and Paul Yu edited an annual *Progress in Cardiology*, for many years. John grew in stature and became one of Britain’s renowned senior cardiologists. He was often an invited international speaker at the American Heart Association and the American College meetings. Professor E. P. Sharpey-Schafer was one of the most clever and at times the most acerbic member of the Hammersmith group. He and Professor McMichael had studied heart failure, cor pulmonale, and the effects of ouabain during the late war years.

Richard Bayliss, with whom I worked closely, was a very able clinician and close to the professor. One of Hammersmith’s greatest raconteurs, he was fond of the apocryphal tale of Sharpey and the chair
in medicine at Thomas’s. He was shortlisted, but a second round was announced. The story has it that Sharpey and Richard were at a pub on Duncane Road near the White Castle, having a beer before going home. Richard suggested that if Sharpey would obtain a fresh new suit and shirt and present himself as a candidate the second time, he would get the chair. Richard suggested a suit like the blue serge the man next to them was wearing. Sharpey, always direct, commenced a conversation with the man. Soon the two went upstairs and Sharpey emerged with the suit. Some say he bought it; others say he convinced the man to enter Hammersmith Hospital for treatment of his cough. No one will ever know how the transaction occurred, but a few days later, *Lancet* had an announcement that Professor E. P. Sharpey-Schafer had assumed the chair at Thomas’s.

Sheila Sherlock was one of the most colorful, clever young women at Hammersmith. She was interested in liver disease and studied hepatic metabolism each afternoon using a catheter passed into a hepatic vein. She always commenced her catheter lab studies promptly at 2 p.m. If Professor McMichaels’s firm—which included Dick Bayliss, Morris Ethridge, from Adelaide, and me—hadn’t finished in the catheterization laboratory by that time, we experienced the full spectrum of Sheila’s colorful nature. She has revised her book *Liver Disease* for many editions. She became a Dame, and Professor McMichael and Dick Bayliss were eventually knighted by the British queen. Sheila is now chairperson at the Royal Free. She is married to Jerry James, a pulmonary physician who was with Professor John Scadding at Hammersmith. Working with Sheila was Alex Bearn, who later studied copper metabolism in Wilson’s disease while at the Rockefeller in New York. He had the medicine chair at Cornell in later years. Sam Kaplan was a South African working in cardiology at Hammersmith. He was interested in going to the United States to do pediatric cardiology and eventually in setting up such a subsection. I urged him to look at Cincinnati, which offered many academic and intellectual advantages, including a marvelous children’s hospital, heavily supported by Proctor and Gamble. Besides, the meals there were superb. As a junior house-officer at the General, I arranged to read their ECG’s in the lunch hour because of the food differential. In later years, I was to interview quite a few candidates for the pediatric cardiology job at Tulane and two for the Established Investigatorship award of the AHA who had trained with Sam at Cincinnati Childrens. More recently, Bill Friedman, who joined the AHA Research Committee as my term was ending, told me he had lured Sam out to the pediatric department at UCLA. The professor was rightfully proud of the bright young people he brought together at the Hammersmith—not only British physicians, but also those from the provinces, South Africa, Australia, India, New Zealand, and Canada. (In spite of reminding him about a certain melee in 1776, I was always introduced as being from the American province!)

Finally I was conducted to a laboratory where thoracic blood volume was being measured by the blue-dye technique. There I met Harry Kopelman and a tall young physician who was introduced as “Grant Lee.” I smiled at the professor, who had me explain, “In my country, nobody is called both Grant and Lee.” There began a long and still cherished friendship that has lasted even through this most recent pulmonary conference outside of Denver.

Hammersmith Hospital was always frenetic with excitement—new ideas, new research initiatives, cardinal seminars, and penetrating, and at times, acerbic grand rounds. The professor was the obvious leader and the arbitrator among sharply circumscribed opinions from the
diverse firms. Hammersmith had newly been designated the Postgraduate Medical School of the University of London and was considered by British medicine to be a heterodox, not one that taught the skilled orthodox British tradition of clinical excellence. At Hammersmith, the position was that one had acquired those skills before coming. Nonetheless, as late as 1949 the other great teaching hospitals were not sure about this Hammersmith place.

This was only four years after the second Great War, and London had only begun to rebuild. The single catheterization laboratory was in a galvanized hut jerry-built onto the end of the hall. Dick Bayliss, Morris Ethridge, and I used it to study the effects of digoxin on the heart and circulation in different types of heart failure. Sheila and her group were studying liver function, and John Goodwin and his group were doing angiocardiography, looking at various forms of cardiomyopathy. When the London fog turned quite green, Hammersmith went out of control. The professor, Sharpey, Sheila Howath, and our group were looking at the effects of ouabain on acute cor pulmonale induced by that thick, soupy green fog. Because of the urgency and large number of patients, these catheters were floated in at the bedside with the help of a water manometer. Some nearby residents of East Acton actually came in to have the “arm-tube with the digitalis.” Watching the pressure level and the oscillations of the tip of the column of water was all that was needed. A mixed venous sample, a quick arterial sample, and, where possible, 3 min of collected exhaled air. A rapid Haldane blood oxygen analysis and an O₂ meter and we had pressure and flow—no balloons, no thermodilution, not even dye dilution. We were not sure of the salutary effects of ouabain in acute cor pulmonale, as opposed to other types of failure, but it often produced a remarkable pulmonary hypertension. We reported our data but were not sure of the mechanism of the enhanced pulmonary hypertension. The hypertension didn’t seem to be due to enhanced flow, and the patient’s oxygenation improved. I’ve concerned myself with it for many years and am still not sure. Ironically, last year in my laboratory we spent much time characterizing the electronegenic Na-K-ATPase pump in the pulmonary vascular bed of healthy cats. There is no doubt that, at elevated tone, activation of the pump induces vasodilation. When the pump is blocked with ouabain, a sizeable pulmonary hypertension quickly appears. When we go about writing up those experiments, we’ll quote that 1950 paper describing this strange aggravation of pulmonary hypertension by ouabain in cor pulmonale.

Many stimulating visitors came to Hammersmith. Werner Forssmann came from his small hospital practice in southern Germany. He had been a thoracic surgeon who had studied with Adolph Fick. He had searched for Fick’s mixed venous sample with the zeal of Jason searching for the Golden Fleece. Alas, his one publication on right heart catheterization for obtaining the sample was telling. He had used a ureteral catheter, which he had passed upon himself with a system of mirrors and a fluoroscope. But the x-ray he published clearly indicated that the catheter was too short and had stopped at the superior vena cava—right atrial junction. Nonetheless, he had come up with the idea, and he, with Cournaud and Dickinson Richards, who did get the mixed venous sample, were awarded the Nobel prize. Forssmann had apparently done no research since that x-ray was taken. He had had problems with other surgeons at their hospital and seemed to have gone to a small practice in a small German hospital during and after the war. He told us that newspaper reporters had found him in this small city and asked him how he felt having just received the Nobel prize. He said he thought for a moment and replied,
“Like a village priest who had just been elected pope.” Visiting the clinical research facilities at Hammersmith, he was indeed a village priest. With the advent of dye dilution, and now thermodilution, one wonders, like Jason, about the value of the Golden Fleece after it served its purpose. Indeed, no one knows its ultimate disposition.

Dr. Sam Levine came from the Brigham. At grand rounds he demonstrated his legendary auscultatory skill, correctly predicting the length of the PR interval by listening to the intensity of the first heart sound. I was elated at this demonstration that American physicians, too, had great clinical skills and could reach a balance between the bedside and the laboratory. Sitting in the back of the room with Paul Wood, I heard his quip, “Let him try that again.” Well he didn’t but he’d shown it could be done. Von Euler from Stockholm lectured on sympathetic control of cardiac inotrophy and attempted to relate that to Starling curves. The family of curves came later, as did Von Euler’s Nobel prize. Lenegre came from France and described fibrosis in the cardiac conducting tissue and heart block. Somehow he was rumored to have obtained his normal control specimens at the guillotine. I erred badly, with my weak command of the Cajun-French patois, when the French cardiologist Professor Heim de Balzac, a nephew of Honore de Balzac visited us. Thinking in English but speaking the patois, I referred to Lenegre as “l’homme guillotiné.”

Then Sheba came from the new medical school at Jerusalem and invited us to visit him. He had just been appointed Israeli minister of health. Some of us took him up on his invitation. The Hebrew University Hospital in 1950 was not the Hadassah Hospital of today. Nonetheless, it was fascinating to read the patient charts in a language un tarnished by 3,000 years of history but suddenly transformed and transliterated into scientific medical English. And one who had helped modernize Hebrew had been my father’s teacher Chiam Naham Bialik in Odessa. Several Russian delegations came to Hammersmith from time to time but left fairly promptly. The professor, like his prime minister, never trusted the Russians and their brand of communism. Borst came from the Netherlands and showed the ability of licorice to increase blood volume by a DOCA-like salt-retaining property. The value of the jugular venous pressure in accurately measuring right atrial pressure at the bedside was demonstrated, even if a tilt table was needed for low pressure. C. Heymanns came from Ghent and lectured on the carotid sinus function. He also received a Nobel prize. Andre Cournaud gave a superb lecture on the pulmonary blood flow and ventilation-perfusion matching. Bill Briscoe, who was in our Hammersmith group, eventually went to work with him at Bellevue. He spent much of his time calculating the distribution of ventilation and ventilation-perfusion ratios. Bill was a heavy smoker, but these were the days before smoking became an anathema.

One other perk appeared in London when postwar rationing was still necessary. The first month, my NIH check hadn’t arrived, so I went to our embassy at Grosvenor Square. No one had heard of this new NIH, but “if it has anything to do with the Public Health Service, you’re in luck because the European chief is here.” He hadn’t heard of the NIH either, but somehow I ended up with an embassy passport and Navy ship-store privileges. Like being at Macy’s in a ration-ridden London! The professor always enjoyed a wee bit of whisky (Scotch). To earn dollars, the Scotch was made in the U.K., shipped to New York, and back to the U.S. Navy ship stores in London by return flight. For errant young sailors preparing to visit the continent, ladies’ stockings were packed in special boxes, a single stocking each.

The professor was quite correct in his
appraisal of the remarkable effect of that digitalis on systemic vessels. Much later it was shown to be related to its blocking action on the electrogenic pump. We were all convinced that much, if not most, of its effect was on the heart, but the two are closely intertwined, and sorting out the differences was difficult in intact man. Nonetheless, it fell to me to present the group paper at the First World Heart Congress in Paris, September 1950. It was to be presented in French; I protested that others spoke fluent French. They explained that the English and the French speak each other's languages but won't. But my patois was insular and far from contemporary French; even my university cheering-song was "Geaux Tigers." Sure enough, before I had completed the presentation, my first ever, Dr. Cournaud was on his feet presenting a response in his staccato native French. I suggested that, because he was now an American, he would be good enough to repeat his comments in English. Only later did English become the lingua franca of science. At the meeting, I had an interesting discussion with Bill Milnor from Johns Hopkins and Tom Mattingly. Bill was interested in the electrocardiographic criteria for the diagnosis of ventricular hypertrophy. Tom, a general in the U.S. Army Medical Corp, was studying trauma to the heart.

Working back at Charity Hospital, I told Dr. Burch that we had finally convinced Professor McMichael that the primary effect of digitalis was on the heart. Without a hesitation, Burch replied "Well, I'm not so sure anymore!" He said that Tulane was undergoing changes. The esteemed Dr. Ochsner was about to retire as chairman of surgery, and Tulane wanted the cardiovascular surgeon Dr. Michael DeBakey to succeed him. Mike was a Tulane graduate and had been Dr. Ochsner's finest before going to Baylor. But Mike suggested that Tulane invite Oscar Creech instead. He had done a superb job with Dr. DeBakey at Baylor and should make an excellent successor to their mentor, Dr. Ochsner. Soon after Oscar assumed the surgery chair, he busied himself setting up a combined cardiology-cardiovascular surgery service. Dr. Burch asked me to come over to Tulane, set up the cardiology-surgery catheterization laboratory in the surgery department, and work closely with the surgeons. Dr. Burch, who never quite trusted surgeons, felt that a well-trained cardiologist in their midst would reduce their mortality rate. Neither he nor any in his group had the time or penchant for that work. The job carried a major hazard. I would be working directly with the chairman of the surgery department, but my own chairman, Dr. Burch, had very little interest in surgery, much less cardiovascular surgery. At later conferences, he was wont to point out that the major cause of death from ventricular septal defect was surgery. Nonetheless, he assured me that the need was great and my uneasiness ill-founded.

When Oscar walked into his newly activated catheterization lab and saw me, he decried, "Lord, not you again!" Oscar was one of the most princely men I've ever had the pleasure to know. He was an accomplished cardiovascular physiologist who happened to be a master surgeon. Work went well; new brooms sweep well. There were daily cardiac catheterizations, attendance at surgery and the intensive care unit, and presenting or standing in for Dr. Burch at cardiology conferences and at his student rounds during his important visits to the NIH, to other universities, and to Europe. The research seminars were replete with promising young people. Tom James was studying coronary arterial supply to specialized conducting tissue and electronmicroscopy of the sinoatrial and atrioventricular node, Ralph Lazarro was doing early electrophysiologic experiments, Lewis Thomas was involved in cardiac pathology, Thorpe Ray was studying sodium and tagged rubidium excretion induced with the new mercury diuretics.
Leo Horan was looking at mechanisms in atrial fibrillation. Victor Ferrans was finishing clinical cardiology training on my ward and doing his Ph.D. in cardiac electronmicroscopy, and Bill Love was estimating coronary blood volume by dye dilution. In physiology, Dr. Hyman Mayerson and Karlman Wasserman were measuring pulmonary lymph protein.

The ivory-towered top floor of the medical school was Burch’s Berchtesgaden. Here his hemetically sealed temperature- and humidity-controlled rooms had been constructed. In them, I did bedside cardiac catheterizations to measure pulmonary vascular pressures and Fick outputs. We were studying the effects of temperature and humidity on human cardiovascular function. New Orleans could get hot in the summer, and hospitals weren’t yet air-conditioned. Moving quickly to make measurements and keep the instruments functioning in those rooms often caused incapacitating weakness within 15 to 20 minutes. Dr. Burch himself had once experienced a syncopal episode in one of the rooms. Strangely, the volunteer, who lay quietly on a couch, sweated but had no real discomfort. Cardiac outputs were measured 35–40 L/min at the most humid and hottest temperatures.

Work had proceeded quite well until an unlikely incident changed all that. I had visited John Ross at the NIH and received instructions in his newly introduced transseptal catheterization technique. The transbronchial approach we previously used to cross the mitral valve was difficult for the patient, and twice the PE-50 tubing had knotted inside the left atrium before it could be passed through the mitral valve. Fortunately, both were in patients with severe mitral stenosis. In each, the PE-50 was drawn tautly against the patient’s lips, and Oscar Creech caught the errant loop during a finger fracture, closed valvulotomy. Back at the Tulane lab after the NIH visit, I was doing a transseptal catheterization on one of Oscar Creech’s patients. The catheter had gone across the atrial septum but entered the left inferior pulmonary vein. A plastic 3F was then passed out the transseptal catheter into a peripheral pulmonary vein, and pressures in the left atrium and large and small pulmonary veins were recorded concurrently as the patient lay comfortably on the catheterization table. This approach simulated to a great extent the continuous measurements of systemic venous pressure that Dr. Burch had studied. He had described the spontaneous fluctuations in pressure in these vessels.

At that moment Dr. Burch, who generally wanted little to do with cardiac catheterization, walked in and asked about the progress of the procedure. When I explained how readily one could measure pulmonary venous pressure in a man resting quietly, he was enthusiastic. He watched the pressure tracings carefully as he asked the patient to undertake complex mental tasks. After the diagnostic procedure was complete, he announced with great conviction, that I was to study pulmonary circulation. There was no need for me to do all of these diagnostic catheterizations; other people could be brought to Tulane to do that, but to study the intact pulmonary vessels was a more important undertaking. Responding to my reluctance to undertake the study of a circulatory system about which so little was known, he smiled again and said, “Can’t think of a better reason to do it.” After many years of clinical cardiology, electrophysiology, and about twenty-five hundred diagnostic heart catheterizations, I was now into the pulmonary circulation.

Many months of long evenings were consumed in the medical library, reading what was not quite “so little.” The relation of vascular flow to pressure, interstitial lung pressure to lung volume, hypoxia, gravitational effects, distention and recruitment, pulmonary capillary blood flow, pulmonary venous pressure, and pulmonary reflexes were among the major
topics of interest. Rewarding weekends were spent with Bill Milnor at Hopkins learning from his work on pulmonary venous and arterial pressures in open chest dogs. Bill had recently transferred from cardiology to physiology at Hopkins and was even then formulating plans for his unique book, *Hemodynamics*, published in 1982. It wasn’t that far from New Orleans, and my sister was there with her husband, who was on the child psychiatry faculty.

Dr. Burch advised me to make simple, direct measurements in intact dogs. I contrasted the spontaneous changes in large and small vein pressures, after taking elaborate pains to ensure that this was not pulmonary venous wedge pressure. Output and pulmonary blood volume were measured by dye dilution. We then looked at the effects of transfusion, of a cold environment, and of balloon occlusion. A pressure gradient between the large pulmonary vein and the left atrium was never identified. We found no support for the concept of large vein throttling pulmonary venous flow in the left atrium. Interpreting the data provoked interesting discussions. Measurements dealing with pulmonary hemodynamics are not interpretable in terms commonly used in the systemic circulation. I urged those near Dr. Burch who were reviewing my data with me to remember that this is a high flow, low pressure bed, modulated by Starling-type resistors not seen in the systemic bed; that flow and pressure are not always nearly linear; and that calculated resistance values must be interpreted with great caution. Moreover, although changes in small vein pressure are valid, the more the vein constricted, the more likely the size of the catheter was to interfere with exact calculations of absolute venous resistance.

Nonetheless, papers began to be published, and Oscar Creech urged me to apply for my own NIH grant. By then I was looking at pulmonary embolism and the possibility that sudden distention of a pulmonary vessel could induce reflex bradycardia and hypotension, with possibly reflex pulmonary arterial and venous constriction. Dr. Greene, from the NIH, was visiting Tulane, and Oscar arranged a meeting, where I learned for the first time how to apply and write a grant application. The application was submitted under Oscar’s aegis, and, to my complete surprise, the study section funded my project. Oscar was delighted, there was joy in Mudville, the first of this growing junior faculty had gained independent support. There was, however, an unforeseen problem, and my Icarian wings experienced the first rays of the sun’s heat. A member of the medicine faculty only applies for grant support from the medicine department, not surgery. The careful instructions from the NIH hadn’t covered this point. They saw my laboratory in surgery, my close clinical and laboratory relationship with the surgeons, and said that’s the way to go. In spite of the progress of the research, I could not bring any of these studies to readily provide for a definitive experiment. One had to differentiate between passively induced pulmonary hypertension from mechanical obstruction with embolization and actively induced vasoconstriction. Paul Yu from Rochester was chairing the American Heart Association session at which Abe Rudolph was presenting a paper suggesting that the pulmonary hypertension resulting from embolus was mechanically induced, and I followed with data suggesting reflex contribution. I had known Paul for many years. He was working in the American Heart Association and was soon to be its president. He spoke to us before the session and developed an excellent discussion during the session based on prior knowledge of the presentations. Abstracts were not published until later, if at all, in those days.

The implications of all these studies were becoming increasingly obvious. The pulmonary vascular bed could not be
studied by methods used in the systemic bed. Techniques were available to perfuse an excised lung or to perfuse it in situ at constant pressure or constant flow, or even in the intact chest to look at three points on a pressure-flow curve. However, what was needed was a technique permitting the study of a hemodynamically isolated lung lobe perfused at constant flow, and constant left atrial pressure, in an intact, spontaneously breathing animal. With such a preparation, the pharmacologic responses of pulmonary blood vessels could be more closely studied with respect to dose response curves, receptor sites blocking agents, and, later, transduction mechanisms. Moreover, reflex responses of the pulmonary vessels are better examined in intact chest animals. In the clinical arena, this was the time of a great thrust toward surgical pulmonary embolectomy. Working with Oscar Creech and his surgical group, we had done some but saw the unfortunate complication of a successful embolectomy followed rapidly by adult respiratory distress syndrome. Indeed, the sanguinous froth appeared in the endotracheal tube less than an hour after embolectomy in one memorable patient. The concept of reperfusion injury was not yet developed. Along with many others, I was trying to devise a large cardiac catheter method to extract larger emboli without thoracotomy and bypass, and then later to use thrombolysis with streptokinase. I soon learned that, with suction on the catheter, the pulmonary arterial wall quickly obstructed large catheters and that technique was not going to be effective.

Serendipitously, I was surprised to learn how large a catheter one could put into a dog’s pulmonary artery without causing any evidence of hemodynamic alteration, as long as one didn’t apply negative pressure. I could put a balloon on that large catheter, isolate the left lower lobe artery by balloon distension, and perfuse the lobe with blood through the large catheter with an extracorporeal pump. I had the transseptal technique for lobar vein pressures. In those days, one simply went to Glens Fall, New York, and showed U.S. Catheter and Instrument Company what was needed for research, and they developed the catheter. Marketability or patent rights were not issues. The company directed me to an unusual balloon designer in New Jersey, just outside New York City. He devised the balloon and showed me how to attach it and a length of PE-50 tubing to the catheter with thread and a quick-drying glue.

After introducing this catheter with the attached balloon and side PE-50 catheter into the dog’s left external jugular vein, one could guide it, under fluoroscopy, through the tricuspid and pulmonary valves by altering the distal catheter curve, simply changing the tension of the PE-50 on the distal curve. The left lobar artery was entered with the standard 0.035-inch guide wire. The left lobar vein was easily entered transseptally with a Ross needle and sheath from the right external jugular vein via the left atrium. The Cope adaptor permitted concurrent measurement of pressure in a large and small pulmonary vein in left lower lobe. The surgeons had now abandoned isolated coronary perfusion in favor of hypothermia for bypass surgery. Those pumps served well (and still do) for pumping femoral arterial blood through the catheter to the left lower lobe. The lobar artery was hemodynamically isolated from the pulmonary artery by balloon distension. The transseptal technique was developed with much trial and error. In man, the transseptal catheterization is done from the right femoral vein, but in the lower animals, the plane of the atrial septum is such that one must approach it from the external jugular vein. Fortunately, in these animals the external jugulars are very large and accommodate these catheters easily. Moreover, for those trained in cardiac catheterization before the percutaneous trans-
venous technique appeared, catheterizing the pulmonary artery from the superior cava was easier than from the inferior cava. Positive pressure ventilation during catheter insertion prevented air embolus.

The catheters and needles were improved and miniaturized for smaller animals as time went on. The cat is the sturdiest animal, remaining hemodynamically stable for 6–8 hours of experimentation. The dog is not as stable for long periods, often has heart worms (with or without a negative microfilaria blood test), is expensive, and requires large catheters. The sheep is too large, and its atrial septum is often very difficult to find fluoroscopically. The rabbit is a flower than wilts easily. It readily develops shock with pulmonary hypertension, both of which we found to be easily corrected with cyclooxygenase blockers. Its septum is soft and often tricky to puncture. We have used pigs and occasionally monkeys with some success.

The organizers of the Eighth National Symposium on the Pulmonary Circulation had invited a number of prominent investigators of this vascular bed to the Philadelphia meeting. Bill Rashkind had been interested in congenital anomalies of pulmonary veins, and he somehow asked me to show my data at the meeting. The concept of active changes in pulmonary venous tone was not readily accepted in those early days. Aviado’s group and Milnors’ group had data in open chest isolated perfused lungs, and now I had data in intact dogs with constant lobar flow. That afternoon session had three speakers. The first speaker, an internationally known authority, spoke about the physiology of the pulmonary circulation. I was chagrined to see his first slide, a comparison of the histology of a pulmonary artery and a pulmonary vein. He stressed the musculature of the pulmonary artery and by comparison the sparse musculature of a pulmonary vein. He also expressed wonder at “what Al Hyman was going to talk about for forty-five minutes.” Bloodied but unbowed, I expressed the angst of a relative novice in this subject but proceeded to present 45 minutes of data. A lively exchange followed (the first of many over the years), which was finally interrupted by introduction of the third speaker, Domingo Aviado. He was to speak on the pharmacology of the pulmonary system but spoke to the vascoactivity of pulmonary veins, measurements he had made in his laboratory. Domingo was obviously in pain, hobbling about the podium because the colchicine hadn’t relieved his acute gout. His comments directed toward the first speaker were more trenchant, indicating pedagogically to the lead speaker that “he hadn’t done his homework.”

Among the speakers the next day was Gil Blount from Denver, who spoke presciently on an interesting topic, “High Altitude Effects on the Pulmonary Circulation in Cattle and Man.” Bob Grover and Jack Reeves worked in his cardiac catheterization laboratory, and their later contributions to our understanding of pulmonary hypertension are well known. Gil was superb and showed us how things are done in Denver. In the freezing Philadelphia night, Gil came to Bookbinders with us wearing a light jacket, no vest, sweater, or top coat! Others in the group, Jesse Edwards, Art Sahshara, Ray Truex, Al Fishman, and Van Mierop, were more conventionally attired.

Having finally experienced relief the next day, Domingo was more placid and spent the afternoon with me in his laboratory. Soon after that session, he moved on to do toxicology for the army, a loss to the pulmonary circulation group. We waited many years until Chris Dawson, John Linehan, and their group in Milwaukee developed a better technique to assess pulmonary venous reactivity and in large measure showed more clearly what we
were trying to demonstrate. Only recently Chris was kind enough to have me to his laboratory. Their work is unique.

It seemed clear that a more detailed study of the pharmacology of the pulmonary vessels was a fruitful area of study. First the endogenous peptides. Bradykinin actually constricted the dog's pulmonary veins at resting tone, a finding recently reported at the New York Academy of Science by Paul Guth. I was able to find him and discuss the data when I learned he was a professor of pharmacology at Tulane. I began working more closely with the basic science groups, especially pharmacology. The pharmacology of pulmonary vessels was apparently not a topical research area. Although the grant renewal was funded, one of the pink sheets contained the query, “How many ways does this fellow intend to make the lung cry ouch!” A cartoon framed in my lab shows a lung with a megaphone and the inscription “ouch.” Many years later, a prominent pulmonary vascular investigator visited my laboratory, saw the cartoon, and laughed. “You know, years ago, I put that same remark on someone’s NIH grant critique,” but he couldn’t recall who the applicant was. Well enough. He didn’t believe in pulmonary veins either.

The Schuller Memorial Lecture is sponsored annually by the pharmacology department at Tulane. Sol Langer from Paris delivered a remarkable lecture dealing with subtypes of adrenoceptors and their pre- and postjunctional activities in the systemic vascular bed. But the pulmonary vascular bed functions at a lower level of tone, and virtually none of this data is available in the pulmonary bed. Following his lead, we looked at the neural control of the pulmonary bed as a short-term project. We found that the problem was more complex in the lung vessels because of altered response at various levels of tone. At low tone, stellate stimulation induces pulmonary vasoconstriction, but when tone is increased, stimulation produces a transient alpha-1-induced vasoconstrictor response followed by a longer vasodilator response, induced by beta-2 adrenoceptor stimulation. We also found the beta-2 adrenoceptor activity was enhanced at elevated tone. Moreover, agents such as epinephrine and phenylephrine, which have both alpha and beta adrenoceptor activity, reverse from vasoconstrictors to vasodilators as tone is increased. Several years later, I was giving a seminar on this topic at Sol Langer’s laboratory in Paris. I pointed out that the pulmonary bed was unusual, because in that bed, phenylephrine, which is the paradigm of alpha-1 adrenoceptor agonists, has clear beta-2 activity in the lung vessels. Both Sol and Icilio Cavero beamed as they pointed out that they had published similar data using right atrial muscle preparation several years before.

Our studies of hypoxia in the dog revealed only a modest vasoconstrictor response, quite similar to that reported earlier by Al Fishman’s group. The sheep gave a somewhat more vigorous response, and we studied hypoxia in this species. Bob Grover and Jack Reeves had reported a far more vigorous response in their dogs. We thought it might have been related to acclimatization to high altitude and exchanged a group of dogs via air freight to and from Denver. Sure enough, the first two of his dogs were vigorous responders in my laboratory, and the first of mine gave the weakest response they had ever encountered. The next was more responsive, and Bob asked me to check where the vendor got the dog. Probably a high-altitude area. “No, the only place flatter than New Orleans is Mobile.” That theory faded. We are still not sure of the reason, but the difference was real.

In clinical cardiology, Tulane was handicapped in those days. We used the huge and academically rewarding Charity Hospital, but there was no private Tulane
Medical Center, which only came later. More and more patients were being referred directly to Oscar Creech and placed in two large private hospitals. These hospitals now had catheterization laboratories, and Oscar asked me to study and follow them outside the Tulane-Charity system. Only those referred patients with more complex problems were admitted by special arrangement to Charity for diagnosis and treatment. As the university medical and surgical groups became more and more disunited, the inevitable happened. A departmental reorganization resulted in my faculty appointment being placed entirely in the surgery department, with the catheterization laboratory. My Icarian wings had now completely melted, and I was now at sea, treading research alone. Friends, advisors, and coworkers came from time to time, but, alas, the last icon had departed, and my approach to the pulmonary circulation became a complete immersion in my own research laboratory, with my own model.

I miniaturized the apparatus to study the cat, an animal almost free of heart worms (a nemesis in New Orleans). To do so, we lost the small pulmonary vein measurements because the lung is too small. We gained in sturdiness, reproducibility of responses, and ease of catheterization. My coworkers and I have devised experiments to study pulmonary vascular regulation by the sympathetic and parasympathetic nervous system, by the prostaglandins, by acid-base alterations, by hypoxic sensors in the arteries upstream to gas-exchange vessels, and by adrenergic and purinergic receptors. Most recently, a unique opportunity was presented to look at central regulation of the pulmonary bed. About twenty years earlier, Donald Richardson, a young academic neurosurgeon, had been studying pain and temperature fibers in cats in his laboratory next door to me. He grew in stature and eventually assumed the chair in neurosurgery. He was now implanting electrodes in man to stimulate opioid receptors to relieve chronic intractable pain. In the faculty parking garage we exchanged pleasantries, and he suddenly asked me how to treat ventricular tachycardia. He had inserted this electrode and stimulated; in lieu of producing relief of pain, the response was hypertension and ventricular tachycardia. “Don, there are 15 people in the cardiology department who can help you treat tachycardia as well as I, but where did you put that electrode?” He showed me in his cat model, in the supraoptical diagonal band of Broca. I spent the summer months that year relearning the neuroanatomy I’d forgotten since 1942. Function had been added to the bland memorization of sites and tracts. We combined techniques in my laboratory, using stereotactic electrode insertion and cardiac fluoroscopy for pulmonary catheter insertion. Alas, the stimulation induced the systemic effects he had already shown but no pulmonary responses.

For many years, I had been concerned with the effects of pulmonary vascular tone on responses. I had devised a technique infusing a thromboxane A$_2$ simulator U46619 which raised lobar pressure from 12 to 35 mmHg at constant flow with a stable baseline. We repeated the stimulation, and now we saw a vigorous biphasic response, a vasoconstrictor that commenced with 5–10 s of stimulation followed shortly by a vasodilator response that persisted 3–4 min. We were excited. Aubrey Taylor in Mobile suggested that the vasodilator response should be looked at more thoroughly. How did we know it wasn’t mediated by a circulating vasodilator rather than directly by a neurogenic mechanism? Well, we didn’t, but the introduction of a 4-min trap in the femoral artery–lobar artery pump clearly identified two separate vasodilator responses, one before the perfused blood from the femoral artery reached the lung, and one 4 minutes later, when that blood arrived. Although the earlier constrictor and dila-
tor responses were not affected by standard blocking agents, the late dilator response was clearly blocked by ICI 118551, a specific beta-2 adrenoceptor blocker, and by propranolol. Moreover, the serum epinephrine levels rose 5- to 10-fold with the stimulus. Our earlier work had demonstrated the tone dependency of responses to some phenylethylamines. At low resting tone, epinephrine is a vasoconstrictor, but a very active dilator with even small increases in pulmonary vascular tone. Jack Reeves was one of the referees selected by the journal editor. He wrote that he checked it out with the Colorado neurology department, and we were quite on target—he even signed his name to the reviewer’s comments.

The effects of tone on the pulmonary vascular responses have continued to fascinate and perplex us. At low resting tone, epinephrine, acetylcholine, serotonin, phenylephrine, adenosine, ADP, ATP, and bradykinin, to name a few, are vasoconstrictors, but at high tone, induced by U46619, hypoxia or selected vasoconstrictors, these agents become vasodilators. On the other hand, alpha-2 adrenoceptor agonists are greatly potentiated at high tone but prostaglandin F2 alpha, Bay K 8644, and angiotensin II are not potentiated as vasoconstrictors at high tone. This remains one of our areas of intense study. In the past three and a half years, since being joined by my young colleague, Howard Lippton, a pulmonologist, we have returned to looking at vasoactive peptides and their tone responses. Indeed, endothelins are also vasodilators at high tone!

Presently, I remain pleasantly at sea, paddling around in the pulmonary circulation. I still do my own experiments at the bench, an old curmudgeon myself now, but steadfastly refuse to be anyone’s icon, false or true. Moreover, I maintain an active interest in clinical cardiology, doing consultations and invasive procedures in the cardiac catheterization laboratory in the early morning hours, before going to my research laboratory.

References