

NATIONAL ACADEMY OF SCIENCES

WILLIAM BARRY WOOD, JR.

1910—1971

A Biographical Memoir by

JAMES G. HIRSCH

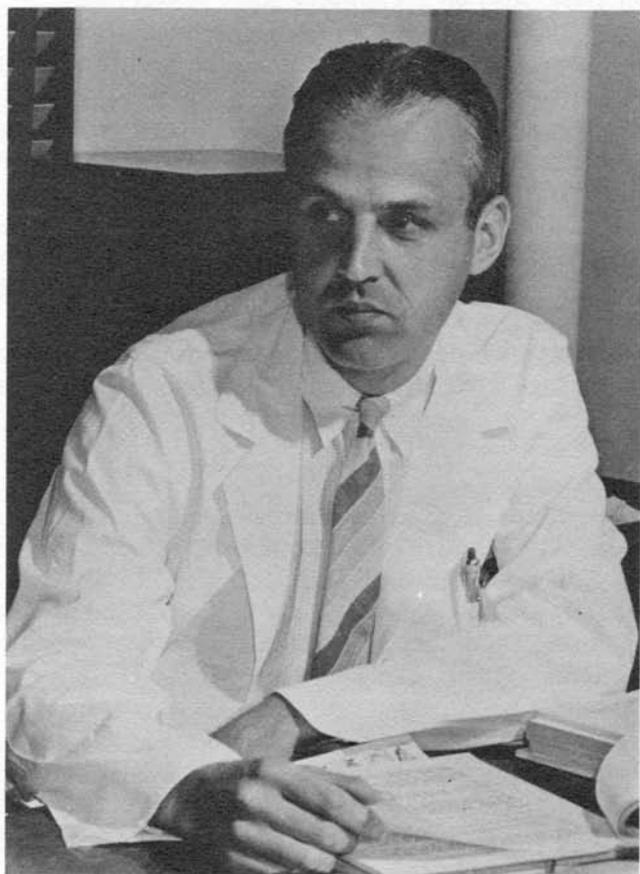
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Biographical Memoir

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WASHINGTON D.C.



Mr. Barry Wood Jr.

WILLIAM BARRY WOOD, JR.

May 4, 1910–March 9, 1971

BY JAMES G. HIRSCH

BARRY WOOD was born May 4, 1910 in Milton, Massachusetts, of parents from established Boston families. His father was a Harvard graduate and a business man. Little information is available about Barry's early childhood, but it was apparently an enjoyable and uneventful one; he grew up along with a sister and a younger brother in a pleasant suburban environment. He was enrolled as a day student in the nearby Milton Academy, where one finds the first records of his exceptional talents as a star performer in several sports, a brilliant student, and a natural leader. Young Wood had no special interest in science or medicine. He took a science course as a part of the standard curriculum his senior year at Milton and somewhat to his surprise won a prize as the best student in the course. This event signaled the start of his interest in a career in science.

In view of his family background and his prep school record it was a foregone conclusion that he would attend Harvard, but Barry was only seventeen years old when he graduated from Milton, and his parents decided he might profit from an opportunity to broaden his outlook and mature further before entering college. He was sent to The Thatcher School in California, an experience he recalled

later as highly enjoyable and successful, with much exposure to outdoor sports and activities.

The record Wood made at Harvard (1928–1932) was truly phenomenal, leading understandably to national fame at the time. He was a star athlete, winning nine letters in three major sports (football, baseball, and hockey) and a tenth letter in tennis. He was named to the All-American football team as quarterback in 1931 and was captain of the football team in his senior year. So much time was taken with his football play, his position as center of the hockey squad, and as first baseman of the baseball team, that he didn't participate in track, one of the sports he excelled in at Milton. He did, however, make time for a little tennis, achieved national ranking, and was chosen on one occasion as a member of the Davis Cup squad.

In the face of this record of athletic accomplishments, which obviously consumed a good deal of time, Barry somehow managed not to neglect his academic work. He graduated summa cum laude and did honors thesis research in biochemistry as I shall discuss in a moment. Barry was asked time and again in later life how he managed to do both sports and schoolwork and do both so well in prep school and in college. His answer was deceptively simple, namely that he devoted all of his time to these two activities and organized himself so as to avoid incursions on his time by anything else. This undoubtedly was true or nearly true, but nevertheless his record of accomplishments in both fields is likely never to be equalled.

Barry's experience in chemistry at Harvard was influenced by a chance encounter with James Conant, described by Barry as follows:

I can still remember vividly coming out of the chemistry library . . . walking down the hall one day, and Mr. Conant met me and asked me what I was doing in the biochemistry field, whether I was having a good time, and

whether I planned to write an honors thesis. I told him I really hadn't made any plans. . . . Mr. Conant was then chairman of the department, and I was just an undergraduate student . . . but as was so typical of Mr. Conant he still knew that I was there and that I was trying to concentrate in biochemistry. He said that he knew just where I ought to work on my honors thesis, with Professor L. J. Henderson. Well, I had read Henderson's *Fitness of the Environment*, which was a book that anyone concentrating in biochemistry would read, and I had also read his monograph on the blood, which was his great work as a scientist, and the idea of working with L. J. Henderson just seemed too good to be true. As a result of Mr. Conant's efforts, I was introduced to L. J. and went to work with him in the fatigue laboratory, which was located in the basement of the Harvard Business School across the river.*

Barry's description of L. J. Henderson was also of some interest.

He was not a laboratory scientist. As a matter of fact, he used to tell me that he was no good in the lab; he broke all the test tubes! He stayed in a room above the laboratory. L. J. took the data that came out of the laboratory, he would work with his slide rule and put it together and write it up. He really was one of the first theoretical biologists, who didn't do a lick of work with his own hands. And later he became a philosopher, interested in Plato. Of all the people on the Harvard faculty at that particular time, he had more influence over President Lowell than anyone else. Lowell went to him, consulted him about every major decision. To be allowed to work in his laboratory was a tremendous privilege.†

Barry's honors thesis work was selected to take advantage of his athletic as well as his academic activities. The laboratory had studied previously changes in certain physiological or biochemical parameters in athletes such as marathon runners. Henderson advised Wood to study changes in the white blood cell count during strenuous physical exercise. The study thus involved obtaining from his teammates blood sam-

*"Leaders in American Medicine," audiovisual memoir T/V2107, W. Barry Wood, Jr., 1971; Alpha Omega Alpha Honor Medical Society and National Library of Medicine/National Medical Audiovisual Center.

†*Ibid.*

ples for white counts before, during, and after the height of physical exertion in football or in hockey contests. The findings were impressive: white counts in a football game or after a period on the ice would go from a normal of 5000 to 24,000, a change as great as that seen in pneumonia or acute appendicitis. This thesis work, properly evaluated and written up, was published in a German physiology journal and was the first of a long series of publications by W. Barry Wood, Jr., nearly all of which had to do directly or indirectly with the same white cells that were the subjects of this college project.

Barry also described with some relish his experience in writing for publication this first research work, an experience strikingly similar to that recalled by many of us in similar circumstances.

The thing I can remember most clearly about this experience was that I had to write this honors thesis, and I worked very hard on it. I tried to make every sentence perfect. When I got through I thought I had a masterpiece to give to L. J. to read. Well, he called me back about three days after I had given it to him. It was there on his desk, and it was just *covered* with red marks—corrections—starting with the title. He began by pointing out that the title didn't say what was in the paper, that when you give a title to a paper it should tell the reader what is in the paper. He must have spent hours correcting the manuscript, every single word. My first reaction was one of anger. But it was a wonderful lesson to me in writing. I did the whole thing over again, taking into consideration all of his corrections. And then, of course, he liked it a little better. That had a lasting impression on me. I always tried later on to do the same thing, to help junior faculty or students write scientific papers properly.*

Two important decisions were made by Barry in 1932 when he graduated from Harvard. The first was the decision that he and Mary Lee ("Leal") Hutchins would be married. Barry and Leal had been close friends since childhood; the Woods and the Hutchins shared with several other Boston families a summer camp in southern Maine. Secondly, Barry

**Ibid.*

decided to enter medical school at Johns Hopkins. He reminisced about the choice between Harvard and Hopkins as follows:

I decided after college that it would be a good thing to get away from Boston. After all, I'd been there nearly all my life, and it seemed to me wise to go somewhere else. I had just been married, and my wife's father was a physician who had been trained at Johns Hopkins. I heard him talk about it, what a wonderful place it was. He had been there in the days of Osler and Welch and Kelly—he was Kelly's first resident on the gynecology service. I also looked at the Hopkins catalogue and noticed that they had a lot of free time for special studies and research. I was interested in the idea of doing research, so it seemed to me that I ought to go to Johns Hopkins. So I went to talk to some of the Boston Brahmin physicians. And they told me I was crazy! They said that John Hopkins was a second-rate place, that there was no medical school that could measure up to Harvard Medical School. But despite that advice I decided to go to Hopkins, and I never regretted it.*

Barry and Leal lived in a boarding house near The Hopkins. Leal went to Goucher and then to The Johns Hopkins School of Public Health, for her graduate work. Both Barry and Leal often spoke of these happy days, commenting that it was a great experience to go through graduate school together. They kept careful records of expenses; total costs, including tuition, room and board, and maintenance on a secondhand car, were \$1100 for the first year, for both!

The medical school curriculum was a demanding one, but Barry found some time to spend in the laboratory of W. Mansfield Clark, working in biochemistry and metabolism, in particular on pH and oxidation-reduction potentials. One summer during medical school was spent at the University of Wisconsin working in microbiology and visiting with Polly Bunting, a close Vassar College friend of Leal's who was a graduate student at Wisconsin. The following summer Barry took a clerkship at The Boston City Hospital,

**Ibid.*

where he was exposed to interesting clinical material and stimulating teachers: Soma Weiss, William Castle, and Chester Keefer. As Barry put it, he was "bitten by the clinical bug" that summer.

When graduation from medical school drew near, Barry found himself facing a difficult choice in terms of postdoctoral training. He was interested in everything ranging from clinical medicine through clinical research to the basic sciences and pure laboratory work. Mansfield Clark urged Barry to go straight into biochemistry and start his scientific career, but Barry finally decided, after considerable debate and soul-searching, to pursue his clinical bent. He went on to internship and assistant resident appointments at Hopkins on the medical service directed by Warfield Longcope.

Longcope encouraged each of his resident physicians to select a speciality for clinical and laboratory study in depth during their stay. Barry wanted the metabolism and biochemistry speciality, but it had been taken by someone else. Longcope suggested as an alternate the field of immunity and gave Barry recent issues of The Rockefeller Institute for Medical Research annual reports, directing his attention to the work of Oswald Avery. Wood was utterly fascinated with these reports. Longcope arranged for Barry to visit Avery in New York. Barry described his visit as follows:

I can still remember to this day going into Avery's office. He sat me down at a table. . . . He was a tiny little man, and he had on a long white coat, and he paced the floor. He told me the whole story of the pneumococcus capsule and the polysaccharides in such a way that I was just entranced by it, and I went back with great enthusiasm for getting into this infectious disease field.*

Barry had little time for lab research during his clinical residency, but as soon as this had been completed he returned to Boston as a National Research Council Fellow in

**Ibid.*

the bacteriology department of Hans Zinsser. Wood had made a choice between A. Baird Hastings in biochemistry and Zinsser and selected the latter because he was still captivated by the pneumococcus and wanted to do research on this organism. It is somewhat surprising that Barry did not seek a position with Avery, the acknowledged pneumococcus expert who had stimulated his interest, but there is no evidence that he considered this course of action.

Zinsser was in the terminal stages of leukemia during Barry's stay in the department, so arrangements were made for him to work with John Enders, who was then studying pneumococcal infections. Wood and Enders developed a laboratory model of pneumococcal pneumonia in rats, based on earlier work done by Nungester at Michigan. This model allowed them to study in a fruitful manner several experimental aspects of the infection. They demonstrated that leukocytes played a primary role in recovery from pneumococcal pneumonia and were not merely scavenger cells that cleaned up the damage after antibodies or other agencies had killed the microbes. They studied the effects of antiserum on the recovery process and confirmed the earlier reports that antibody promoted phagocytosis, although they also noted that some phagocytosis occurred before demonstrable antibody was present. These experiments laid the groundwork for continuing studies on relationships between pathogenic microorganisms and phagocytic cells, studies that occupied Barry for approximately half of his scientific career.

After only one year at Harvard, Barry returned to Hopkins, accepting a junior staff position in the department of medicine. It was wartime and he was busy with clinical and administrative duties, including service with a special commission on primary atypical pneumonia of the Armed Forces Epidemiology Board.

During his second year on the staff at Hopkins, Wood was

offered the position of professor and chairman of the department of medicine at Washington University in St. Louis. Barry was only thirty-two years of age. He was, in his own words, "flabbergasted" at the offer. He was only two years beyond house-officer training, and he felt a distinct uneasiness about his ability to handle the professional responsibilities. Furthermore, St. Louis was far outside the Boston-Baltimore axis he had been on his entire life, and all of his Hopkins friends and mentors advised against taking it because it would preclude any chance to develop a research program. Despite these negative aspects, the challenge and the attraction of the offer were too great to resist, and he accepted.

He started slowly in St. Louis, watching and learning the ropes from Harry Alexander and other experienced clinicians. His success in the early phases of the new job was helped in no small measure by his strict adherence to the admonition of his wife, Leal, that he never mention Johns Hopkins! Within a few years Barry had established one of the best teaching and research medical services in the world at Washington University. The service was small, by today's standards, with an unusual degree of intimate and stimulating contact between professors and house staff.

Wood was determined to continue his laboratory research in his new position. He enjoyed clinical medicine and teaching and was extraordinarily talented in both of these activities, but basic research was his first love. He devised an unusual plan for sharing the clinical and administrative leadership of the department with his close friend and colleague, Carl Moore. Each was the professor in charge of clinical and administrative duties for six months of the year, during which the time available for research was nil, or at best catch-as-catch-can, and each enjoyed six months of the year for full-time research, uninterrupted save by emergencies. Such a

plan can be expected to work only if the two men are completely compatible and trusting of one another; it worked very well indeed for more than a decade with Barry Wood and Carl Moore sharing the captain's role.

Although limited to half-time for research activities, Wood was remarkably productive during his thirteen years in St. Louis. His laboratory published during this period approximately thirty papers reporting new findings on mechanisms involved in the pathogenesis of diseases produced by the pneumococcus or closely related microorganisms. He studied the mechanisms by which the outcome of experimental pneumococcal pneumonia was altered by various agencies: coexisting influenza virus infections, various drugs and antibiotics, and serum antibodies.

Perhaps the most important contribution was the discovery of the phenomenon of surface phagocytosis. This discovery grew out of observations made on lungs of animals infected with encapsulated pneumococci early in the course of the disease, before the animals were able to produce antibody to render the microorganisms susceptible to phagocytosis. It was noted that considerable phagocytosis was occurring even at these early time points. Further study on interactions between encapsulated pneumococci and phagocytes *in vivo* and in several situations *in vitro* established the fact that the nature of the environment was important to the outcome. On smooth surfaces the phagocytes were unable to engulf the bacteria, whereas on rough surfaces they were often able to wedge the slippery microbes in a blind alley or a corner and accomplish phagocytosis. This phenomenon of surface phagocytosis contributed to understanding of the early events of infection; more important in theoretical terms was the demonstration that physical as well as chemical parameters influenced the phagocytic process.

Toward the end of his stay in St. Louis, Wood embarked

on a new line of research, leaving pneumococcal disease in favor of experiments dealing with what appeared to be quite a different area, the pathogenesis of fever. When asked to comment on the factors that accounted for this change in direction of his research, Wood began his reply with another comment on his visit, during his house-officer days at Hopkins, with Oswald Avery at The Rockefeller Institute.

When I went to visit Avery that time I mentioned earlier, he pointed out that there are two kinds of investigators. There are investigators who go around picking up surface nuggets, and wherever they spot a surface nugget of gold, they grab it and put it in their collection. And, he said, there is another kind of investigator who is not interested in these surface nuggets, but rather is interested in digging a deep hole in one place, hoping to hit a vein. Of course, if he strikes a vein of gold he makes a tremendous advance. Dr. Avery was such a wonderful example of this second type of investigator. . . . If you look at his bibliography essentially everything was on the pneumococcus—I think there was one paper on the streptococcus, which is a sort of cousin of the pneumococcus. And yet Avery was the father of modern molecular genetics. Avery made the extraordinary discovery that the molecule important in heredity is DNA, in the course of studying his dear old pneumococcus, which he stayed with all of his life.*

Wood continued,

Having been so impressed by Avery's doctrine and his career, I was very hesitant to leave the pneumococcus. But I rationalized by saying that I was just as much interested in the leukocyte that I started working with in college. This double interest also seemed justified because the leukocyte, after all, is the thing that destroys the pneumococcus and makes the patient recover. One of the things that commonly happens to patients sick with infections or other diseases is that they develop fever. And I was impressed as a clinician with how little we know about fever. This became particularly fascinating when it was found that the leukocyte is the cell that makes pyrogen, which is a hormone that acts on the hypothalamus to reset the body thermostat so that the temperature goes up. So it was really a logical progression to get involved in this area.†

**Ibid.*

†*Ibid.*

At this time Wood changed not only his primary field of interest, but also his job. He was offered the post of vice-president of Johns Hopkins, a challenging position in which he was expected to coordinate the medical school, the school of public health, and the hospital and to revise the medical school curriculum. The decision to return to Hopkins was a difficult one for him to make. His life in St. Louis had been a happy and gratifying one from both personal and professional points of view. He and Leal enjoyed their home and friends and found it ideal for their children. He had developed the department of medicine at Washington University into one of excellence as judged by both clinical and research authorities. All was going very well indeed. What then led Barry to accept the offer from Hopkins? His own comments on this question indicate that he was probably lured by the new challenge, as well as by the old ties to The Hopkins:

Decisions of that kind . . . are not made up here in the head by logic, they're made down here in the middle of your solar plexus. I had a feeling at that time that I might be able to contribute something if I went back to Hopkins. . . . I'd been in St. Louis for thirteen years. I knew that Carl Moore was there and would take over. I didn't have any worries about leaving the ship when it wasn't in good shape. Dr. Lowell Reid, who had been the vice president and was then acting president of Hopkins, persuaded me very convincingly that I could make a contribution.*

Wood commented on his experiences on returning to Hopkins as follows:

I had reservations about whether I could be effective in administration, whether I would be happy in it, so I left myself an escape hatch. The trustees very kindly permitted me to keep a laboratory in the microbiology department, where I could continue to do research. I tried to do my administrative work in the morning and to keep the afternoons free for the lab. But when you are doing research, it's not only what you think about when in the laboratory that is important, it's also what you think about after

**Ibid.*

dinner, or in the morning while shaving. You've got to have your mind on these research problems. What I found happened to me was that in all these hours outside the working day I was thinking about the administrative problems, not the research. I found this very difficult, and after four years of it, when circumstances made it possible for me to return full time to teaching and research [as chairman of the department of microbiology at Hopkins], I jumped at the opportunity.*

It must be said that some of Wood's attempts to bring about administrative changes met with opposition. He said in describing his efforts to modernize the curriculum that "trying to change the curriculum is like trying to move a graveyard!"

Barry's stint as an administrator did not leave him with a jaundiced attitude toward this activity, since he went on to say: "Administrative problems have to do with the welfare of other people. That's what an administrator is, he's trying to do things so that other people can operate properly. It seems to me that it is one of the most unselfish of occupations, to be a good administrator." Furthermore, after Wood returned to teaching and the research laboratory, he continued to give generously of his time and wisdom in many outside advisory and administrative activities such as The Board of Overseers of Harvard College (1944–1955); Armed Forces Epidemiologic Board (1950–1962); Board of Trustees, Rockefeller Foundation (1954–1971); National Academy of Sciences (1956–1971, elected 1959; Council Member 1959–1965); and The President's Science Advisory Committee (1960–1962); to list only a few of the most prestigious. Wood was also a dedicated member of The American Society for Clinical Investigation and the Association of American Physicians, serving as President of both.

One outside activity deserves special mention, for it was an unusual one and one that Barry prized perhaps above all others. This was the so-called Interplanetary Society, also

**Ibid.*

known more widely as the Pus Club. Shortly after Barry returned to Hopkins, he and two of his closest friends, Paul Beeson and Walsh McDermott, decided it would be pleasant and rewarding to gather once or twice a year, bringing along their research groups to spend a day in informal scientific discussions and good fellowship. This was convenient, since Beeson was in New Haven and McDermott in New York. A very special relationship existed among these three men: each was endowed with unusual qualities for leadership and each had wide-ranging interests and abilities, from clinical medicine or public health through microbiology to modern science at the molecular level. And furthermore they constituted an unusual three-way mutual admiration society, apparently completely free of any friction or ill will that might have resulted from their common interests and competition. These meetings were remarkably successful and were slowly enlarged to include selected outside groups, such as René Dubos and his colleagues in New York. With passage of time junior people from the three departments moved into positions of independence. These alumni continued to participate in the Pus Club meetings, and the meetings grew larger and larger, but the special quality persisted. At no time was there any formal organization or charter; throughout, the only official members were Barry, Paul, and Walsh!

The last decade of Barry Wood's life was spent with his "first loves," research and teaching. He built an outstanding microbiology department. He took a personal interest in the departmental courses, attending all lectures and student labs and accepting responsibility for many of them himself. His lectures, like his research papers or review articles, were models of clarity and precision. His style of lecturing was calm and refined, not flamboyant, but the overall effect was nonetheless inspiring to, and appreciated by, the students.

His research in these last years at Hopkins consisted in the

main of a series of studies on leukocytic pyrogen. He and his colleagues demonstrated convincingly that neutrophil leukocytes and mononuclear phagocytes are cellular sources of pyrogen. They investigated the stimuli and conditions required for pyrogen production and release, and they established the site of action and many of the pharmacological properties of endogenous (white cell) pyrogen, as distinguished from various exogenous substances that produce fever. Finally they launched a successful attack on the isolation of endogenous pyrogen and the study of its chemical nature. Although the major effort was on pyrogen, in these Hopkins years an occasional paper appeared on other topics, such as surface influences on phagocytosis of streptococci, mechanism of action of penicillin, and nature of heat labile serum opsonins. All in all Barry Wood was author or co-author of over 100 papers reporting original scientific observations; nearly all of these were published in *The Journal of Experimental Medicine*.

His reputation as a scientist and as an excellent writer and speaker led to many invitations for review articles or name lectureships. He authored two articles in *Scientific American*, "White Cells and Bacteria," in 1951 and "Fever" in 1957; he gave a Harvey Lecture entitled "Studies on the Cellular Immunity of Acute Bacterial Infections" in 1951; he contributed to *Physiological Reviews* in 1960 an article entitled "Phagocytosis, with Particular Reference to Encapsulated Bacteria." His published name lectures and monographs included the Shattuck Lectures on "Studies on the Causes of Fever" in 1958; the Dunham Lectures on "Miasmas to Molecules," 1961; and Expo '67 Noranda Lectures on "Report to Metchnikoff," 1968. He also contributed to several textbooks, the most significant being his part in the Harper and Row *Microbiology*, co-authored with Davis, Dulbecco, Eisen, and Ginsberg.

Wood's later years in Baltimore were happy and successful ones, marred only by family illnesses. He remained active as an athlete throughout his life. In his fifties he was lean and fit, the picture of good health and certainly an unlikely candidate indeed to suffer a heart attack. But he was an example of how deceiving appearances can be. His father had died at an early age, probably of coronary artery disease, and Barry inherited the tendency, which made its appearance despite his excellent general physical condition and his physiognomy. He suffered a severe, debilitating coronary artery occlusion in 1969. He was incapacitated for several months but recovered nearly completely and was back to a reasonably normal life. His wife's illness, leading to her death in the summer of 1970, added to the tragic burden. Then came the final, fatal heart attack on March 9, 1971, while Barry was in Boston to attend a dinner honoring President Pusey. His life thus ended in Boston where it had started some sixty years earlier, a life cut short, but a life nevertheless full and rewarding in the extreme.

What qualities made Barry Wood the special person, the natural leader that he so obviously was? Let me quote from some of his close friends and colleagues who attempted to answer this question when they paid memorial tribute to Wood shortly after his death.

Dr. A. McGehee Harvey said:

That he was a rare human being was clear to all who knew him. What were the ingredients of his greatness? Barry aimed constantly for excellence in everything he undertook—in research, in teaching, in administration and in sports. He had ability in abundance and the self-discipline to succeed. He was a great teacher, for whom teaching was a sacred trust. Every student was given special care. He was no mere theoretician of medical education; he spent long hours preparing for lectures and conferences, clarifying his writing and sifting his courses so that only the finest remained. Giving everything his best, he inspired others to do their best; and so he led by example, softly. Another ingredient of his greatness was that rare quality

of speaking with one voice and listening to all—presidents and first year medical students alike. This quality above all brought respect and trust from countless students and colleagues who sought his help and advice. Barry's unique combination of talent in teaching, research and clinical medicine, together with his sense of humility and natural benevolence of spirit, gave him that degree of wisdom which is the basis of effective leadership.

Polly Bunting wrote:

I have thought often . . . about what made Barry so special. One of the elements certainly was his capacity for growth, growth professionally and in his personal relations. Success never tended to arrest his development or dull his interest in the new adventures ahead. Growth is the right word, for new skills, new findings, new relations with friends and family, implied no rejection of the past but were built on it and into it, contributing to the whole. Barry's powers of concentration were prodigious, as was his ability to organize his work and his life. These have never been endearing qualities . . . yet somehow in his case they did not offend. Rather they made possible time for those memorable discussions of problems and plans, the companionable golf games, the leisurely Sunday afternoons at Owings Mills. Somehow, very early in life Barry had ordered his priorities to give highest place not to being organized, nor to any specific set of achievements nor any ideology, nor even to people as such, but to those particular individuals who were close to him, those whom he taught, those with whom he taught, those with whom he worked, his friends and his family. . . . Barry was a far more complicated person than was generally realized. . . . He was a very private person, choosing to work through personal problems alone or with those who were directly involved. It must have taken hours of careful thinking as well as a touch of genius to move so surely among us. I suspect it was not as easy as it looked. Love never is easy, only wonderful. Slowly, over the years, one came to realize that what made Barry special was his love.

Walsh McDermott commented on Barry's greatness as follows:

Two qualities stand out as the key essentials for greatness and Barry had them both. The first is that the holder is widely acknowledged to be set off

from contemporaries by some set of distinctions. The second is that the holder has an unusual capacity to bestow some of this distinction on many others. I shall content myself with only one example of the first, the general acceptance that he was different. Barry was a frequent subject of conversation among his colleagues. Yet whenever he attained a particular success, and there were many, I never heard it said even in the most indirect way that "there but for . . . a few minor breaks go I." He stood alone in this immunity to critical comment based on jealousy or envy. But it is on Barry's transmission of his greatness to so many others, that I wish to dwell. . . . Barry had a truly extraordinary ability to bring himself into the lives of others and make them the better for it. . . . To do this for another person must have required reflection, sometimes considerable reflection, about that person. Yet he made it seem as if it were a wholly spur of the moment affair. . . . The striking phenomenon of transmission of himself, came as an act of enthusiasm. It seemed no more and no less than that he couldn't bear to see you not share the fun . . . of high quality. . . . He made you think about yourself, and what you were doing, in such a way that in effect you lifted yourself by your own bootstraps to his generous applause. Clearly Barry saw himself as he was and others as they could be. How does this transmission of himself differ from the fine influence on young scientists and professionals exerted by so many of our top-rank leaders through the years? In many ways it really doesn't differ—and well that is, or we should be in far more trouble than we are. But it does differ in two important ways. First, in the extraordinary personal nature of the relationship without its ever being a possessive intimate relationship. For Barry was deeply convinced that it was the personal relationship that counted. Indeed he believed the transmission of respect for quality could only be made in this way. And second, it differs in that those whose lives were touched by Barry came to know not necessarily the world's greatest scientist or professional in this or that area of learning or creativity, but a truly great man.

The last point made by Walsh McDermott, that Barry was convinced that it was the personal relationship that counted, is borne out by Barry's own remarks when he reviewed his career shortly before his death. In a response to a question from Dr. Robert Glaser, Barry said, "Again it comes back to people, Bob. You look back on your life and you see that the

things that really influence you are your experiences with specific people." At every turn of his career, Barry gave credit to someone who had exerted personal influence, intentionally or otherwise, to lead him along the path he took. The list of people mentioned in this regard by Barry is an impressive one indeed: James Conant, L. J. Henderson, Mansfield Clark, Polly Bunting, Soma Weiss, Chester Keefer, William Castle, Warfield Longcope, Oswald Avery, Hans Zinsser, and John Enders. In turn many of us, myself included, are indebted to Barry for having exerted by his personal touch a telling influence in determining the course of our careers.

It is difficult or impossible to expand in a meaningful way on the incisive analyses of Barry Wood's unique qualities as seen by Mac Harvey, Polly Bunting, and Walsh McDermott. My own comments can only reiterate theirs in a more simple summary. The features that stand out in my mind are: his unusual endowment of physical and mental talent and the drive to make the best use of these talents, a genuine interest in other people and extraordinary ability to influence them for their own good, and versatility—an All-American triple threat in his career accomplishments as well as in his college football days.

The many direct quotations from Barry Wood contained in this memoir are all derived from a motion picture made only two months before his death. This film,* an informal discussion between Barry and Dr. Robert J. Glaser, provides an overview of Wood's career and is a marvelous record of his special personal qualities, which come across well on the screen. Barry's closing words in this interview were especially prophetic and touching in view of his imminent death: "Well,

* *Ibid.*

the only thing I'd like to say at the end is that it has all been wonderful fun. I wouldn't change one thing; it has been a tremendous privilege. And I hope that in the next generation, where things are going to be more complicated, that it will still be possible for people to have as much fun and reward as I had."

BIOGRAPHICAL MEMOIRS
HONORS AND DISTINCTIONS

PROFESSIONAL AND HONORARY SOCIETIES

- American Society for Clinical Investigation, 1941–1971
Board of Overseers of Harvard College, 1944–1955
Council on Pharmacy and Chemistry, American Medical Association, 1944–1949
American College of Physicians, Fellow, 1945–1971
Association of American Physicians, 1947–1971; President, 1962–1963
Experimental Therapeutics Study Section, USPHS, 1949–1954
Central Board, Armed Forces Epidemiological Board, 1950–1962
Scientific Advisory Committee, Common Cold Foundation, New York, N.Y. 1951–1955
Citizens Committee on Human Rights, St. Louis, Missouri, 1953–1955
Walter Reed Army Medical Center, Consultant, 1953–1960
Board of Trustees, Rockefeller Foundation, 1954–1971
Armed Forces Epidemiological Board, Commission on Epidemiological Survey, 1954–1964, 1966–1969
Medical Fellowship Board, National Research Council, 1955–1957
Interurban Clinical Club, Honorary member, 1956–1971
National Academy of Sciences, elected 1959; Council member, 1959–1965
National Advisory Allergy and Infectious Diseases Council, 1957–1961
Dartmouth Medical School Policy Committee, 1957–1962
The President's Science Advisory Committee, 1960–1962
Visiting Committee of the Medical Department, Brookhaven National Laboratory, 1962–1964
Scientific Advisory Committee, Massachusetts General Hospital, 1961–1963
Advisory Committee, Mount Sinai Hospital, 1961–1964
World Health Organization, 1963–1964
Armed Forces Epidemiological Board, Commission on Radiation and Infection, 1963–1964
Advisory Committee, Milton S. Hershey Medical Center, 1963–1964

Board of Advisory Editors of *The Journal of Experimental Medicine*, 1963–1971

Society for Experimental Biology and Medicine

American Society for Microbiology

Population Crisis Committee, 1967

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