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JAMES FREDERICK BONNER

1910—1996

A Biographical Memoir by
FRANK B. SALISBURY

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Biographical Memoir

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James Bonner

JAMES FREDERICK BONNER

September 1, 1910—September 13, 1996

BY FRANK B. SALISBURY

BOTH THE SCIENTIFIC and personal lives of James Bonner were highly active, extending over wide ranges of diversity, and so productive that a significant legacy is left for us to contemplate and build upon. His range is indicated by over 500 publications, including 10 books, devoted to roughly three-dozen fields of scientific and philosophical inquiry, not to mention over 300 graduate students, postdoctoral fellows, visiting professors, and others who worked in his laboratory and gained from his penetrating insights and always active mind. Most of these friends and acquaintances would agree that James Bonner's brilliant mind went well beyond the norm for human society.

Here is a preview of his diverse interests. Early on, he studied plant hormones including auxin, B vitamins, and wound hormones. He coauthored a seminal paper in 1938 on the physiology of flowering, studied rubber production over a period of at least forty years, and spent most of his final forty years attempting to understand how chromosomes with their genes and proteins function in the growth and development, not only of plants, but of animals as well. As if this were not enough to keep him occupied, he was an active member of the National Ski Patrol, traveled over much of the world, climbed mountains in the Himalayas,

Nepal, and many other places, wrote on the philosophy and future of science, and made hundreds if not thousands of close friends in many parts of the globe.

FAMILY MATTERS

James Bonner was born September 1, 1910, in Ansley, Nebraska. When he was six weeks old, his father, Walter Daniel Bonner, moved the family to Kingston, Ontario, Canada, where he became professor of chemistry at Queen's University. Five years later the family moved to Salt Lake City, Utah, where James's father became head of the chemistry department at the University of Utah. His mother, Grace Gaylord, was also a chemist, as was his paternal grandfather. The Bonner siblings in addition to James were Lyman (b. 1912), Priscilla (b. 1914), David (b. 1916), Robert (b. 1917), Walter (b. 1919), and Francis (b. 1921). All received doctoral degrees; four of them became biochemists, two became physical chemists, and one (Robert) became an applied mathematician and computer specialist.

In Salt Lake City the family lived in a semi-rural environment including, for much of James's early years, a "minifarm/orchard" or "farmlet," as James called it, on the outskirts of Salt Lake City. These surroundings were chosen by the parents so the children could have ample opportunity for physical work in an agricultural setting.

James married Harriet Rees on January 1, 1939. Their daughter Joey was born June 10, 1948, and their son James Jose on May 1, 1950. The marriage was dissolved in 1963, and James married Ingelore Silberbach in 1964. They remained very happily married until Ingelore's death on September 3, 1995. James's death followed a year later on September 13, 1996.

All of James's friends and even his children called him James. When someone referred to "Jim Bonner" it was

obvious to all those friends that someone did not know James very well—unless it was one of his mountaineering friends, who often called him “Jim.”

STUDENT AND POSTDOCTORAL YEARS

James graduated from high school in 1927, entered the University of Utah, and spent two years as a chemistry major with a minor in mathematics. He found chemistry and math to be both easy and enjoyable—“Fun,” he said. After his sophomore year, James’s father took a sabbatical year at the California Institute of Technology (Caltech). James and his brother Lyman had tuition scholarships, and James signed up for physical chemistry. He found the first term at Caltech to be most exciting. Everything was fun because they learned by solving problems and never by rote. He thrived on physical chemistry, physics, and quantitative analysis. For the second term, he studied biology under Thomas Hunt Morgan and also became acquainted with such other luminaries as Alfred H. Sturtevant, Calvin B. Bridges, and Henry Borsook. He enjoyed the biology class, but it was “too easy because there were no problems.”

Theodosius Dobzhansky was a new assistant professor who came with Morgan from Columbia University. Dobzhansky had arrived from Leningrad in Morgan’s laboratory only to find that the laboratory was moving to Caltech within the year. Morgan offhandedly made him assistant professor when his one-year Rockefeller fellowship ran out. As such, he was teaching the biology class, including the laboratory. James was amazed to discover that biologists took field trips, specifically to Corona del Mar with Dobzhansky to trap *Drosophila* (fruit flies) in half-pint milk bottles with yeast suspension on paper towels. Because of the Depression, the tuition scholarship could not be funded,

but Dobzhansky made James his research assistant, and, when the family returned to Salt Lake City, James remained to work with the fruit flies. There were more field trips, not only to the beach but also camping in the nearby mountains. For James, the idea that biologists got to take field trips to the great out-of-doors was very seductive! Dobzhansky had arrived at Columbia without speaking any English, yet he was submitting and publishing approximately one paper per month, a practice that he continued for the fifty-five years that he was active in genetics. James had quite a bit of writing experience, particularly under Professor Crabtree at the University of Utah, so he corrected Dobzhansky's language in his early research papers. At the end of the summer, James hitchhiked home to Salt Lake City to finish his bachelor's degree in chemistry and mathematics at the University of Utah in 1931.

The Depression was deepening, but James returned to Caltech on a \$750-per-year teaching assistantship in the Division of Biology. He had also been accepted in the Division of Chemistry. Upon arriving, he went to the Kerckhoff Laboratories, where the biology division was located, and found them deserted. He rode a borrowed bicycle out to the Caltech farm, and there was E. G. Anderson hoeing weeds out of his corn—the maize that made Anderson a famous geneticist. He met George Wells Beadle, who later received the Nobel Prize (with Edward Tatum) for the concept of “one gene, one enzyme.” Bonner and Beadle became lifelong friends.

Back at Caltech, they were building the Dolk Greenhouse, and there James met Herman E. Dolk, a plant physiologist, and Kenneth V. Thimann, who was instructor of biochemistry, later to become a famous plant physiologist. James learned from these young scientists that Fritz W. Went in the Netherlands had discovered a plant growth

hormone, which he had named auxin. After a stint in Java, a common interval for young Dutch botanists, Went came to Caltech in 1933.

Thanks to Morgan and Dobzhansky, James had set out to become a geneticist, but Dolk and Thimann put him to work on production of auxin by a fungus (*Rhizopus*). He was very successful, wrote a paper, saw his name in print, and was “hooked” as he said. He determined that he would try to write as many publications as Dobzhansky did!

During that summer and for three years until he finished his Ph.D., he played the flute in the Pasadena City Symphony. He practiced for an hour each day with the windows of his graduate-student office in the Kerckhoff Laboratories open, and the music wafted out across the campus.

James bought a used Pasadena police motorcycle (Henderson four cylinder). He found a friend with one just like it, and for a while they rode together. One day he learned that his friend had been hit while standing still. He visited the friend, who never fully recovered, and immediately traded his motorcycle for a 1924 Chevrolet Superior roadster.

He graduated with a Ph.D. in biology in 1934. Sturtevant was his thesis chairman because Morgan was off collecting his Nobel Prize. James’s father came to graduation. Although Sturtevant was the thesis chairman, James actually worked with Dolk and Thimann. Dolk was killed in an automobile accident in 1933 and was replaced by Went, with whom James also studied for his doctoral research.

In the midst of the Depression, James received a fellowship to support a postdoctoral year in Europe. Morgan had arranged for him to work in Utrecht, but when he arrived in the summer, the laboratories were almost deserted. He toured Europe by train and by bicycle, finding

that his German worked well, and visiting various famous professors with whom he had corresponded. He visited Berlin, Jena, Leipzig, Dresden, Prague, Munich, Heidelberg, Innsbruck, Zürich, Bern, Basel, Cologne, and finally back to Utrecht. There he worked with Professor Kruyt, the most famous colloid chemist of the time. Biologists thought that colloid chemistry would provide insight into the functioning of protoplasm. Although protoplasm is indeed colloidal in nature, colloid chemistry provided little insight into protoplasm function. Nevertheless, James learned to speak Dutch and also worked in the laboratory where Went had discovered auxin, working under Went's father F. A. F. C. Went. Fritz Kögl, with his assistant Hanni Erxleben, had recently arrived to be head of organic chemistry with A. J. Haagen-Smit as chief assistant in botany. While James was there, that team isolated indoleacetic acid (IAA) from urine and showed that it had auxin activity. They called it heteroauxin, and, although it was one of the most important discoveries in plant physiology, James heard about it only after he had returned to Caltech.

Kögl, Haagen-Smit, and Erxleben were studying two other "auxins" that they had isolated from urine and corn oil. They called these auxin a and auxin b. They eventually published molecular structures for these compounds—structures that were like nothing known then or since. Although textbooks included the structures until the early 1950s, no one was ever able to isolate the compounds again or even to confirm any part of the proposed structures. James Bonner and Samuel Wildman became convinced in the late 1940s that the compounds as described had never existed. Indeed, they concluded that Erxleben, who was carrying out the actual experiments and reporting the data to Kögl, had somewhere gone astray and continued to report faulty or even fraudulent data to Kögl, who, using the data, de-

veloped the unlikely structures. To add to the romance of the story, Erxleben proved to be a German spy located in the Netherlands during those years before World War II. In 1966, J. A. Vliegenthart, who by then occupied Kögel's chair, found samples of auxins a and b in a locked cabinet and analyzed them by mass spectrometry. Auxin a turned out to be cholic acid, for example, confirming that the compounds as originally claimed had never existed.

While in Utrecht James also worked in the Department of Biochemistry of Leiden Medical School under Professor Bungenberg de Jong, who was an expert in coacervates (one colloid suspended in another). This work was not very fruitful although James studied pectins (the "glue" between cell walls of adjacent plant cells), which continued until as late as 1960.

In early 1935 James moved to Zürich, where he worked in A. Frey-Wyssling's laboratory writing an important paper as noted below. Frey-Wyssling was for many years the world authority on cellulose and cell walls. In the autumn of 1935 James attended a botanical congress in Amsterdam, where he made a lifelong friend of Hiroshi Tamiya, who at that time, in common with most of his Japanese colleagues, wrote papers in German. He and James conversed in German, but Tamiya later became highly adept at writing English and wrote most of his important papers in that language. James came home on the *Bremen* in five days near the end of 1935.

PROFESSIONAL CAREER

Morgan offered James a position as research fellow at Caltech, which he began in late 1935, being advanced to instructor (1936—37), then assistant professor (1937—43), associate professor (1943—46), professor (1946—81), and finally professor emeritus (1981—96). After retirement, he

set up a company called Phytogen (a California corporation), of which he was chairman, chief executive officer, and chief scientist. The company was eventually subjected to a takeover.

He was a member of at least twelve national and international societies: the National Academy of Sciences (1950), German Academy of Sciences (1970), American Association for the Advancement of Science (fellow), American Society of Plant Physiologists (president, 1948—49), and the Botanical Society of America (chairman, Physiological Section, 1949—50). In 1949 he was elected to the American Alpine Club, and as noted above he was for many years a member of the National Ski Patrol System. He was also active in the Sierra Club.

James's incredibly varied and productive research career produced 108 graduate students (1939—88) and approximately 200 postdoctoral fellows, visiting professors, and others. His 500-plus publications (including 10 books) outline his research interests. Beginning with his undergraduate study at Caltech and continuing until at least 1961, he studied various topics, including plant growth substances and related matters (47 publications), pectins and cell-wall characteristics (17 publications), wound hormones (6 publications), various B vitamins as root growth substances and effects of the vitamins when added to whole plants (31 publications), embryo culture and tissue culture (4 publications), rooting of cuttings (8 publications), and the physiology of flowering (18 publications). During World War II and continuing until 1983, James studied the environmental effects and biochemistry of rubber production (16 publications).

Other topics to which James contributed include allelochemicals (3 publications), plant nutrition, especially of camellia (16 publications), miscellaneous biochemical

studies (12 publications), plant respiration (9 publications), the proteins of green leaves (7 publications), crassulacean acid metabolism (CAM) in succulents (2 publications), various plant responses to environment, which James called phytonics (5 publications), sterol metabolism (10 publications), and uptake of solutes and water (8 publications).

Beginning about 1956 (at the urging of a former graduate student and by then postdoctoral fellow, Paul O. P. Ts'o), but with a significant acceleration in 1961, James became interested in protein synthesis, microsomal/chromosomal proteins, histones and chromatin (including non-histone chromosomal proteins) and molecular biology in general (including 3 papers on the molecular biology of memory!), nucleic acids, and the genome. This work encompassed about 190 publications with about 112 coauthors. James made numerous contributions in this area, far too many to discuss in limited space.

In addition to the topics listed above that seem to bear at least some relationship to each other, there were studies in physiological ecology, the biology of plant growth and cell chemistry, many exotic travel logs, examination of a lunar sample (2 papers), the geochemistry of biomolecules, and carcinogens. James was highly interested in the functioning and infrastructure of science (14 papers) and speculations about the future of biology (30 papers). There were at least five editorials and autobiographical and biographical papers.

Obviously, space won't allow discussion of all those fields. I have somewhat arbitrarily chosen the following for some discussion: growth substances including studies of the cell wall, vitamins as root-growth hormones, photoperiod and the physiology of flowering, rubber, the "new plant biochemistry" (including plant respiration, mitochondria, protein of green leaves, etc.), and chromosomes and related topics (histones, non-histone chromosomal proteins, etc.).

GROWTH SUBSTANCES INCLUDING STUDIES OF THE CELL WALL

While the geneticists were on vacation when James returned to Caltech as a graduate student, Dolk and Thimann convinced him that he should study *Rhizopus suinus*. They were using the fungus as a source of auxin to apply to plants to study auxin effects. James discovered that the addition of bactopectone made the fungus produce 100 to 200 times as much auxin than without the bactopectone, especially when the fungal culture was aerated. This had the potential to become a breakthrough discovery. We now know that the auxin was indoleacetic acid (IAA) and that it is produced in the plant from tryptophan, a component of the bactopectone. James and the others could have discovered the nature of auxin many years before it was actually established. But a connection between auxin and tryptophan was not evident to them; instead, the work was hailed only as a great discovery of how to produce more auxin to apply to plants!

As a graduate student, James also developed the section-growth test for auxin in which sections of oat (*Avena*) coleoptiles are floated on auxin solutions; in response to auxin they can double in length within 24 hours. This test has been widely used. Although it is somewhat less sensitive than some other tests, it is much simpler and quite suitable for many studies. Using this system, James measured growth of the sections in solutions with different pH values. He discovered that, even in the absence of auxin, the sections grew in acid solutions much more than in neutral solutions. This discovery of "acid growth" was not pursued at the time, but it generated much interest in the early 1970s. A leader in the study of acid growth was Robert Cleland, one of James's former graduate students (and my office mate at Caltech).

While in Zürich working with Frey-Wyssling, James used the polarizing microscope to study cell-wall properties. His manuscript, proudly written in German, showed that auxin made the cell-wall microfibrils slide past each other more easily. In his autobiography, he referred to this as “a considerable contribution.” Indeed it was. James was a coauthor of ten subsequent papers related to cell-wall stretching and cell growth, and numerous other investigators have pursued this initial observation of “wall loosening.” When James returned to Caltech, he studied effects of auxin on root growth with Johannes van Overbeek and J. B. Koepfli. Much was learned about auxin during the late 1930s.

In response to advice from Frits Went, James studied the wound hormones that had been proposed many years before by Haberland in Austria. String beans can be cut lengthwise, the seeds removed, and a drop of juice from ground-up pods added to the exposed inner part of the pod, causing cell division. With his first postdoctoral fellow, James English, Jr., he isolated the active substance, which he and English called traumatic acid. This proved to be 1-decene-1,10-dicarboxylic acid, a previously unknown substance that was active in plants. This substance also worked on potato slices. Forty years after the work by English and Bonner, Zimmerman and Coudron showed that it was apparently the product of a non-enzymatic oxidation of 12-oxo-trans-10-dodecenoic acid, the first compound in the jasmonic acid pathway, a pathway that is now well known, widely studied, and involved in plant growth regulation.

In the early 1950s Carlos Miller and Folke Skoog, as well as F. C. Steward, discovered compounds that cause cell division. These are now known as cytokinins, and they are well accepted as plant hormones. The wound hormone has the same effects and might be considered in the same

class, but this is seldom done. James's work with the wound hormone has been almost forgotten.

VITAMINS AS ROOT-GROWTH HORMONES

Phillip White had grown tomato roots through repeated transfers by adding yeast extract to a medium that contained the essential mineral nutrients and sucrose as an energy source. James set out to find what it was in the yeast extract that allowed the growth of the excised tomato roots. He obtained some vitamin B₁ (thiamine), which had just been synthesized, and it made the pea roots grow nicely, although growth slowed after six to eight transfers. James was ecstatic about his discovery and wrote to Phillip White to "tell him the joyous news." White never answered, but he published similar experiments quickly in the *Proceedings of the National Academy of Sciences*. James's paper was written first, but it appeared only later in *Science*, with a longer paper in the *American Journal of Botany*. James's conclusion: "Be careful how you spread the joyous news."

There was much more work on roots from various species as influenced by known vitamins. Fred Addicot was James's first graduate student, and the two of them discovered that many roots required thiamine and also niacin. Tomato roots require thiamine and pyridoxine for unlimited growth. They further showed that the B vitamins were synthesized in the leaves and transported to the roots where they apparently make root growth possible. The papers James published, and there were at least thirty-one of them, pointed out that these vitamins ideally met the definition of plant hormones—organic substances that are synthesized in one location, transported to another in the plant, where at millimolar concentrations they cause some noticeable growth or metabolic response. Although this was expounded in the 1952 textbook *Principles of Plant Physiology*, which

James coauthored with Arthur Galston, the idea never caught on among plant physiologists. Apparently it is difficult for many of us to think of vitamins, often known to act as coenzymes, as also being hormones. The study of B vitamins as plant hormones took most of James's time up to the beginning of World War II.

PHOTOPERIODISM AND THE PHYSIOLOGY OF FLOWERING

James received a letter from E. J. Kraus, chairman of the botany department at the University of Chicago, inviting him to spend the summer of 1938 working with Karl Hamner on photoperiodism. Kraus had worked on the physiology of flowering, specifically on the theory that the carbohydrate/nitrogen ratio controlled flowering, and Hamner with graduate student Edith Neidle was studying the effects of nitrogen on flowering of *Xanthium pensylvanicum* (now *X. strumarium*, the cocklebur), a short-day plant that flowered when days were shorter than about sixteen hours. Plants were maintained in a vegetative condition by keeping them under artificial lights so the day length exceeded sixteen hours. One day Neidle discovered that all the plants in the greenhouse were flowering. Although the greenhouse managers were at first reluctant to admit it, they finally reported that the power had been off for one night so the plants had received a short day. Hamner and Neidle then found that *Xanthium* would indeed flower in response to a single short day.¹ (Nitrogen, however, slightly promoted flowering rather than inhibiting it as the carbohydrate/nitrogen theory predicted .)

Actually, Kraus wanted James to spend the summer in Chicago and then probably join the University of Chicago botany department. James asked R. A. Millikan, who was de facto president of Caltech and who had come from Chicago, if this sounded like a good idea. Millikan said to

go but not to accept any money because James wouldn't like it and would be back! That was how it worked out.

James and Karl Hamner did much during that brief summer and made at least one discovery that changed the direction of research in photoperiodism from that time on. They asked the question that seems obvious now, but it had not been asked since the discovery of photoperiodism in 1920 by Garner and Allard (and before that by Julien Tournois in France, who published his findings in an obscure paper): In the day-length response, which is more important, the day or the night? Hamner and Bonner exposed plants to various day lengths with a constant night or to various nights with a constant day. The length of the day did not seem to be very important; to produce flowering, the night had to exceed about 8.5 hours almost independent of the day length. They suggested that we should speak of long-night plants rather than short-day plants.

In the most important series of experiments, Hamner and Bonner interrupted the day with brief intervals of darkness (which had no obvious effect) and the night with brief intervals of light, which completely suppressed the flowering response. The inductive dark period had to be long enough and without light interruption if the cocklebur plants were to flower. This was the discovery of the night-break phenomenon, which was extensively studied for many decades, up to and including the present. (Thanks to James, I studied the phenomenon for almost three decades.) One of the most important discoveries in plant physiology came a few years later at the U.S. Department of Agriculture laboratories in Beltsville, Maryland, when the pigment phytochrome was discovered. Harry Borthwick and Sterling Hendricks used lettuce seed germination and the night-break phenomenon in *Xanthium* to make this discovery. Phytochrome accounts for literally dozens of plant responses to light.

There were numerous other studies during that summer of 1938. For example, with grafting experiments, they studied the nature of the flowering hormone that had been postulated by Mikhail Chailakhyan in Russia, and many attempts were made to extract and isolate the hormone—a goal that has yet to be achieved in a satisfactory fashion. There were experiments to show which part of the plant was sensitive to day length (the leaf). The facilities at the University of Chicago left nothing to be desired, and there was a platoon of student assistants to help with the experiments. The paper reporting the results was published in December 1938, in the *Botanical Gazette*, a University of Chicago publication edited by Kraus. James wrote in his *Annual Review* autobiography, “I have no hesitation in describing this paper as a minor classic.”

James’s interest in photoperiodism continued until the early 1960s. Graduate student John Thurlow worked with James to discover an inhibitory effect of auxin in flowering of *Xanthium*. James Liverman followed up on this work and with James wrote an important review of flowering in the early 1950s. With James, Jan Zeevart and I independently studied nucleic acid synthesis as a part of the flowering process in *Xanthium* in the late 1950s and early 1960s, and Erich Heftmann working with James and Zeevart implicated sterol metabolism in *Xanthium* flowering.

As a graduate student, I worked on flowering of cocklebur with James for two years (1952—54). My story provides some insight into James’s scientific personality and philosophy. Liverman and Bonner, based on the very recent discovery of phytochrome, had developed a theory about the interaction of auxin and phytochrome in *Xanthium* flowering. They called this the photocycle and were excited about its possible implications. I set out to test the hypothesis with *Xanthium*. Liverman was a postdoctoral fellow by

then, testing the hypothesis with the *Avena* section-growth test. With Liverman's help, I developed a quantitative method to measure the flowering of *Xanthium* based on stages of floral development. Virtually every experiment that I did, however, seemed to refute the photocycle, but it was almost impossible to talk to James about my findings, because he was preoccupied with other studies during the first six months of my sojourn at Caltech. When he finally became available for consultation, I spent many hours on numerous occasions presenting my data on the little blackboard in his office. I would finish the explanation of the most recent series of experiments by saying, "See, that certainly disproves the photocycle." James would look at the data for a few moments, contemplate them, and reply, "Yes, but . . ." He would invariably have an alternative explanation for my data. It was in sessions such as these that I realized I was dealing with a truly brilliant mind! The scientific tension between him and me continued for the next eighteen months of my graduate study. Finally, I had set up a typewriter and even a bed in the headhouse of the Dolk Greenhouse, where I was staying while I wrote my dissertation. I wrote the literature review, description of methods, and lengthy presentation of the approximately 125 experiments that I had been able to complete. Finally, it came time to write the discussion chapter. How could I come to conclusions diametrically opposed to the theory that my major professor had published and described in symposium talks during the previous two years? James came to the Dolk Greenhouse and spent a full day and a half reviewing my results, experiment by experiment. At the close of this intense study, James said, "You're right!" He never again defended the photocycle, and indeed within a few weeks he was at another symposium describing my results with no mention of the photocycle. Some scientists develop a hypothesis and defend it to their deaths without

flinching and in spite of any contrary evidence that might appear. James was not cast in that mold. For him, only the truth mattered.

RUBBER

All natural rubber comes from Southeast Asia. Guayule (*Parthenium argentatum*) is the one plant in the western world that has been a serious rubber producer. It is native to northern Mexico and part of Texas, and it was producing small amounts of commercial rubber at the beginning of World War II. James and Frits Went decided to become specialists “by making ourselves master of all guayule knowledge, learning about how it was grown and what one could do to improve it.” Frits arranged a meeting with officials of the Intercontinental Rubber Company (IRC), which was the only large company involved in production of rubber from guayule. The meeting was in Salinas, California, where IRC had large guayule plantations. Unfortunately, the shrubs had been planted on a ten-year rotation, not exactly a quick way to get quantities of rubber for the needs of World War II. Nevertheless, the plants produced 8—10 % of their dry mass as rubber suspended in a milky latex. James and Frits studied the nutrient requirements of guayule plants and how to kill the various pests that reduced the yields. James was appointed a special agent of the U.S. Forest Service assigned to the Emergency Rubber Program. He showed that the production of rubber is controlled by night temperature; below 10°C (50°F) rubber is produced. Much better production occurs below 7°C. This finding was ignored in spite of several publications, although later work by others specified the enzyme that was induced by the low temperatures.

James eventually entered into a long-term association with the Rubber Research Institute of Malaysia to which he was

an advisor from 1965 to 1975. He was made a member of the Malaysian Rubber Research and Development Board, and in 1975 he became chairman of the Agricultural Science and Biology Subcommittee of this board, a position that he occupied until shortly before his death. In this capacity, James developed a technique for adding ethylene (ethaphon) to the bark of the rubber trees, which increased latex production and essentially doubled world rubber production!

THE "NEW PLANT BIOCHEMISTRY"

After the end of the Second World War, Samuel G. Wildman arrived as a postdoctoral fellow. With Sam, James made a new start with what would today be called cell biology, the isolation of "chloroplasts, mitochondria, cytoplasm, and lots of enzymes!" They ground spinach leaves in a colloid mill, centrifuged the product at 20,000 g_n and found that the supernatant contained the soluble leaf proteins. Furthermore, over half of the soluble leaf proteins consisted of a single component of molecular mass ca. 500,000, which they called fraction I. Sam found this fraction in many other leaves besides spinach. It was later shown by John Littleton of Palmerston North, New Zealand, a former postdoctoral fellow of James, that fraction I was the main protein in the stroma or fluid part of chloroplasts. He, Paul Ts'o, and others went on to show that fraction I is ribulose-1,5-bisphosphate carboxylase/oxidase, which is now often referred to as rubisco. Rubisco is the enzyme that fixes CO_2 in photosynthesis. It is the most abundant protein in the world, and Sam Wildman continued to study it until he retired.

James with Adele Millerd, a postdoctoral fellow from Australia, and others showed that plant mitochondria were much like those of animals. They could carry out the Krebs

cycle as well as oxidative phosphorylation, including ATP production. Their 1951 paper brought the level of understanding of this aspect of plants up to that of animals.

CHROMOSOMES AND HOW THEY WORK

Paul O.P. Ts'o, who took his Ph.D. with James and by 1956 was a postdoctoral fellow, convinced James that they should study the most fundamental problem of biology—how chromosomes control cellular metabolism. Much was already known following the discovery of the DNA double helix in 1953; James thought that it might be too late to begin such work. Nevertheless, studies were devoted to protein synthesis and other topics related to what was to become molecular biology. In early 1960, Ru-Chih C. Huang came from the laboratory of Joseph Varner to work with James as a postdoctoral fellow. The goal was to isolate chromatin (the genetic DNA combined with protein and constituting chromosomes) from the nuclei of pea epicotyls (young stems above the cotyledons or “seed leaves” in young pea seedlings). Ru-Chih Huang quickly found that crude nuclear extract would indeed incorporate the ^{14}C -labeled nucleoside triphosphates into something that was insoluble in TCA (trichloroacetic acid). She purified the enzyme activity and found that it caused incorporation of all four riboside triphosphates into something that RNAase would degrade, which could only be RNA. Furthermore, this synthesis of RNA depended on the presence of DNA in the reaction mixture. This discovery with Huang was extremely important, and three other groups published similar results near the end of 1960.

James and Huang found that RNA transcription worked much better if the DNA was stripped of its histone proteins. Suddenly, it became important to learn all that could be known about histones. Paul Ts'o encouraged James to

arrange a conference on histone biology and chemistry. They obtained money from the Office of Naval Research, the National Science Foundation, and private donors and invited "everyone in the world (56 people) who knew anything about histones." The result of this conference (published as a book) was the realization that knowledge about histones was in a state of complete confusion. Various workers had estimated that the number of histones varied from a dozen to thousands; no pure histones had been prepared up to that time, and nothing was known about the relation of different histones in different species.

Douglas Fambrough, a new graduate student, was sent to Stanford University for a month to learn from Kenneth Murray how to isolate histones using amberlite CG-50 chromatography and polyacrylamide gel electrophoresis to monitor the purity of individual histone fractions. With these techniques they could purify individual histones. They found that there were only five different species of histones (each with its subspecies), and they compared the histones of pea plants and calf thymus. They found that the histones III from these two organisms had similar amino-acid compositions except for one cysteine amino acid in pea compared with two cysteines in the calf histone. These cysteines reacted in such a manner that it was possible to form "a great variety of multimers upon oxidation of solutions just by sitting in the refrigerator." This had been a source of confusion in study of the histones. About this time a postdoctoral fellow, Keije Marushige, published a paper showing basically that what can be done with pea histones can also be done with rat liver histones.

James discussed these matters with Emil Smith who was the chairman of the Department of Biological Chemistry at the University of California, Los Angeles, Medical School. Clearly, it would be interesting to compare histone amino-

acid sequences from peas and calf thymus. They decided to begin with histone IV, which is the smallest of the histone molecules and therefore the easiest to separate from the others. Smith said that he needed two grams of pure histone IV for the analysis. They obtained the calf histone rather easily by collecting thymuses from slaughter houses, but it took 24 tons of dried pea seeds, germinated in barrels, with manual separation of shoots from roots and cotyledons, to obtain the 2 grams of pure pea histone. The effort took a full year. Nevertheless, the sequencing showed that the histones IV of peas and cows were essentially identical. There were only two conservative amino acid replacements between the two species. This was an amazing finding, suggesting that the sequence of amino acids in histone IV is so essential that it has been conserved from the time when animals diverged from plants, possibly close to a billion years ago.

Bonner's lab continued to sequence histones for a while, but because many others started doing it, James concluded that it had become "a growth industry . . . one of those things that's best to turn over to others." As part of this work, the stoichiometry of the five species of histone molecules emerged. There is one molecule of histone I for two each of the other four histones, making a total of nine molecules in the group. Interestingly enough, in spite of this provocative work with the histones in the early 1960s, it is only in a current spate² of papers that molecular mechanisms controlling gene regulation via histone acetyl-transferase have been identified. Bonner's lab carried out much additional exciting work in this field, too detailed for discussion here. Characteristically of much "progress" in science, the foundation work of the 1960s is now lost in the dimness of the distant past.

JAMES'S PHILOSOPHY: FAR-FLUNG PASTURES

In his autobiography for the *Annual Reviews of Plant Physiology and Plant Molecular Biology*, James notes that his most important contribution is probably the graduate students, postdoctoral fellows, visiting professors, and others who have worked in his laboratory. He further says, "Some will no doubt complain that it is more profitable for the serious scientist to stick to his problem and flog it to death. To them I say, for myself, browsing in far-flung pastures is more fun. Dark CO₂ fixation by succulents, chemical plant ecology, the path of carbon from CO₂ to rubber, plant taxonomy [!], and treatment of plant-chemical interaction by enzyme kinetics are all matters that I have also touched, and they have all been fun."

James's final word in that biographical review is typical Bonner, including the double explanation points. "Finally, I spoke earlier about the world that awaits exploration. I have studied it pretty thoroughly. It's all wonderful. From Katmandu to Timbuktu to Kota Kinabulu and beyond. Do not miss it!!"

IN ADDITION TO MY personal acquaintance with James Bonner, I learned many details of his life from five autobiographical sources,³ his amazing list of publications, and memories supplied by acquaintances, including his son Jose Bonner, Paul O. P. Ts'o, Robert (Bob) Cleland, and Ru-Chih C. Huang. I am most grateful for this help. In addition to these sources, I taped an interview (29 single spaced pages) with James on November 22, 1991. Ray Owen and Stephanie Canada, longtime associates at Caltech, worked with me during the preparation of the manuscript.

NOTES

1. Either Hamner or Bonner told me that Hamner had given a lecture telling how he had recently discovered that the cocklebur was an unusually sensitive short-day plant. James heard this story

and was excited about the possibilities. He asked Hamner to see if they could spend the summer together, and Hamner arranged this with Kraus. In my 1991 interview, James had forgotten about hearing the talk but agreed that it probably had happened that way.

2. See, for example *Nature* 387:43, 49 (May 1, 1997) and the five papers from *Cell* that are cited in the nature paper.

3. Chapters from my life. In *The Annual Reviews of Plant Physiology and Plant Molecular Biology* 45:1-23, 1994. My life as a chromosomologist. In *The Molecular Biology of the Mammalian Genetic Apparatus*, ed. P. O. P. Ts'o, 2(1977):317-26, and The life and times of James Bonner, pp. ix-xii, in the same volume. The beginnings of an autobiography sent to the National Academy of Sciences and the Caltech archives. A manuscript dictated to longtime Caltech secretary Stephanie Canada.

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