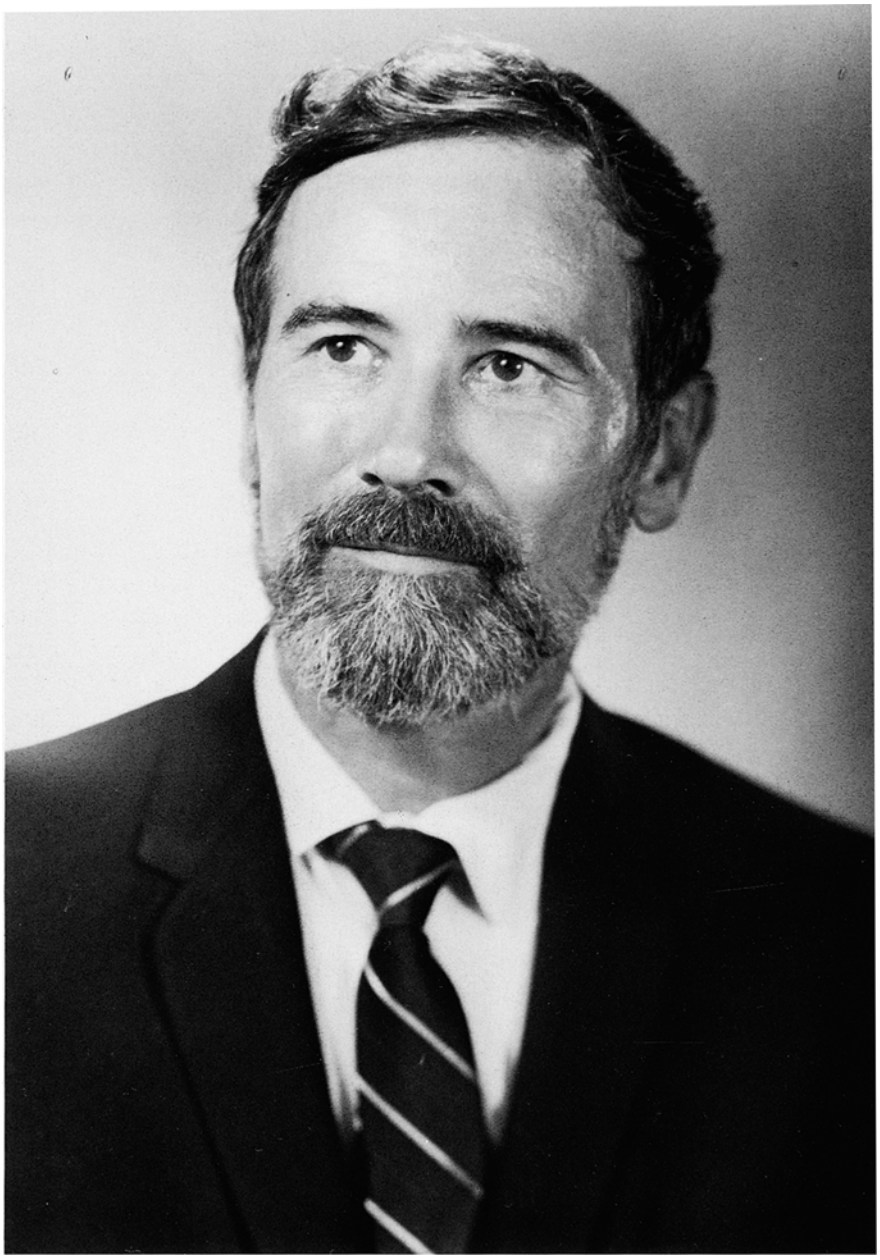

Roderick Clayton (Photograph taken in 1968).

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Personal perspectives

Memories of many lives*

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When I was six I was enrolled in Todd Seminary for Boys, a private boarding school. Todd Cemetery for Boys, we called it. I remember seeing the brochure: 'For *normal* boys aged six to sixteen', and wondering when they would find out about me.

At nine I made a butterfly net and a cyanide killing jar. I also mailed in fifteen cents for a 'chemistry set' consisting of a few packets of chemicals, a little spoon and a booklet of instructions. This grew into a laboratory in the basement; the local high school would sell me good things such as sulfuric acid and sodium metal. I can recall mixing hydrogen and oxygen in a milk bottle (wrapped in an old pair of pants) and tossing in a match, and making thermite from copper oxide and aluminum, and other stunts growing out of the tamer experience of 'turning water into wine'.

I remember myself as a solitary daydreamer. When I wasn't in the neighbor's garden with my net, or exploring new hazards in my laboratory, I lived in the Land of Oz or pretended to be Tarzan. But I knew without question that I would be a chemist.

When I was thirteen we moved from near Chicago to Pasadena, California, and I was delighted. I was newly aware of the girls, and the butterflies were new and excitingly different too. Best of all, winter never came. And I soon learned that the California Institute of Technology was where I should go to college.

In high school I was a dedicated achiever, and I kept this up through my freshman year at Cal Tech. Some slippage began in my second year, and by the third year I was going to the beach or playing cards endlessly, drinking and sleeping too much, and attending classes almost never. To my astonishment I collected a harvest of failing grades and was advised to take a year off in order to grow up. I was shocked – my store of infinite talent had failed me!

* This is the first of a series of invited articles that will appear under the heading "Personal Perspectives" – Govindjee, editor "Historical Corner".

That was in 1942, and before I knew it I was a flying instructor and then a bomber pilot in combat. I can remember sitting in the barracks on Guam at night, doing math problems to prepare for a return to school. I enjoyed a partnership with a skink, a pretty green lizard that perched on my shoulder waiting for insects that were attracted by my lamp. The butterflies were new and different on Guam too, but the combination of military uniform and net did elicit some comment.

I emerged from World War II with a dear wife, Betty Jean (later B.J.), a baby son Rick, and an ardent desire to start classes again. Back to Cal Tech, this time as a Physics major. I believed that this option would be simpler because I had forgotten so many of the details that make up Chemistry. As it dawned on me that I was preparing to be a theoretical physicist, I began to wonder what on earth I was doing. Some of my classmates, such as Don Glaser and Richard Ferris, were evidently far brighter than I; they belonged there. And a new word, Biophysics, was in the air.

The following year, still at Cal Tech as a beginning graduate student in Physics, I made an appointment to see Max Delbrück. His first words as I sat down in his office were, 'I detect a fecal odor. Examine your shoes!' He was right. His next words were 'What do you think Biophysics is?'

Answers to that question came slowly to me over many years. For undergraduate preparation I recommend a solid grounding in physics, chemistry and mathematics, together with enough biology and biochemistry to form a base for broadening and deepening knowledge in these areas during graduate study. I am, of course, prejudiced by my own experiences. I think that the physical sciences should be learned at the problem-solving level; the verbal or 'hand waving' level is not enough. At the same time I have learned to respect the more intuitive arts of dealing with living things. Too often I have seen physicists make incursions into biology with great confidence that a difficult problem can be solved with one or two well chosen experiments, unaware that the subtle complexity of life will almost always give the experiments an ambiguous outcome.

As for a specific choice of career, I suppose we should go where our hearts take us. I don't envy the custodian of a sophisticated instrument who becomes effectively a technician for a paradae of collaborators bearing biological samples. If I were an X-ray crystallographer I would hope to become deeply involved with one or more biological questions.

If I had become strictly a physicist, I think that cosmology would have been the field for me. As it turned out, my sustaining interests became Photosynthesis and Vision. In both of those fields physics, chemistry and biology come together in a convincing and satisfying way.

Returning to my career as a graduate student, I spent four challenging

years with Max Delbrück as my Ph.D. thesis advisor. Such a forbidding personality he had, concealing such a generous nature. In my last year as Max's student he invited me to join him in an article for *Scientific American*. He gave me first authorship and three fourths of the honorarium. Many years later he gave his Nobel Prize money to Amnesty International. Shortly before he died, my wife and I visited him. He emerged from his bedroom, his skin black with hematomas, grinning and saying, 'Behold the hundred-year-old man.' In retrospect I can see that he reserved his brutally direct manner for those he believed able to take it and benefit from it. It was a compliment.

My Ph.D. thesis was on phototaxis and chemotaxis in *Rhodospirillum rubrum*. I graduated in Physics; this was justified in part because I built a monochromator and also because I survived the fearsome final doctoral examination. My memory of that ordeal is centered in the grinning faces of James Bonner and John Kirkwood as they conspired to explore the depth of my ignorance. To make me feel at home at the start of the examination, Max Delbrück told me how he had failed his final oral examination the first time around, at Göttingen, because although he had mastered theoretical physics, he knew almost nothing about experimental physics, and blandly admitted it.

In any event I made it, and at the commencement ceremonies in 1951 I was pleased to have B.J., Rick and our baby daughter Ann (just over a year old) with me. To my surprise I was awarded the degree Magna Cum Laude. I remember thinking, 'Well, I hope you people know what you're doing.'

The guidelines for the Ph.D. degree at Cal Tech said that the thesis research should have great importance *or* originality. I felt that mine had neither, and I wondered by what magic an original idea would enter my head. Eventually, over the years, I learned that if I simply began working I would soon have more avenues to explore than time could allow. Discoveries grew out of the experiments, as long as I tried to be observant. When an experiment went wrong, that was not a signal to throw the material in the sink and do something else. It was the time to wonder what had happened, how it had happened, and whether it could happen again, with and without small variations. As this approach bore fruit I was able to stop worrying about originality.

However, my first six years of work after graduation were not very satisfying. True, I had the benefit of a postdoctoral year with C.B. van Niel at Stanford's Hopkins Marine Station. Kees van Niel's mastery of logic and its correct expression was complete, and he had a passion for cultivating this ability in others. During my year with him a young woman prepared his tea each day, and they played the same grammatic game each time:

"Tea is ready if you are, Dr. van Niel."
"Miss Brokaw! If, in fact, tea is ready, then tea *is* ready, whether I am or not."

Kees' formidable intellect, meticulous logic and imaginative perspectives were an inspiration, and he was always ready to help with the little things. But I was determined to go my own way, and blind to a broader look at the world of science. Biochemistry and biochemists constituted an awe-inspiring mystery that I lacked the will to penetrate; one attempt to learn the subject from *Annual Reviews* convinced me of that.

After my year with van Niel I joined the civilian faculty of the U.S. Naval Postgraduate School, conveniently close at hand in Monterey, California. There I taught Physics to military officers and continued to probe the tactic responses of *Rs. rubrum*. It was quaint research, long before we became aware of the importance of membranes and the existence of permeases. I remained in this setting for five years, driving myself in the laboratory and comfortable at home. I knew that there was a fellowship of scientists 'out there', and that I was losing contact with it just when I should have been seeking it out and becoming a part of it.

I had the nature essential to a scientist; the unquestioning passion to explore. I didn't do science to help humankind, or for any other external reason. I did it because there was no other way. But I expressed this drive in isolation, single-mindedly pursuing a narrow range of experiments, relentlessly repeating one activity to the exclusion of all else.

On the social side, B.J. and I became co-presidents of the Carmel PTA and I went on from there to be a member of the Board of Education. From those experiences I learned three things. First, how to become a high-handed parliamentarian. Second, that some of those harrumphing conservative businessmen had humane natures and were truly dedicated to the welfare of children. Third, that public speaking is only easy if you know what you're talking about. It was my duty one year to welcome the new schoolteachers. Six pretty ladies (ladies, not women; that was in 1956) waiting expectantly for me to say something. My mouth felt full of cotton wool, my tongue was clicking stickily, and I somehow got through the next fifteen minutes. I never had that kind of experience lecturing on science; there I would be carried away by my enthusiasm for the subject.

Such was life on the Monterey Peninsula. Research, new social experiences, research, stunning beauty of nature all around, research. Finally stasis and dissatisfaction. One day B.J. and I looked at each other and knew that we had to get out of this spooky candyland called Carmel-By-The-Sea; this premature retirement.

In the fall of 1956, then, I applied for fellowships as a way to eject myself from the Navy School and lotus blossom land. Never mind finding a 'real' job to go to; just do it!

Someone told me that my chances for a fellowship would be better if I claimed that I wanted to learn something new. I had just tried pouring hydrogen peroxide onto culture plates of photosynthetic bacteria, some grown aerobically and others anaerobically. The colonies of cells grown aerobically caused the peroxide to fizz vigorously, whereas those grown anaerobically gave only a puny fizz. The effect was far more spectacular with *Rhodobacter sphaeroides* than with *Rs. rubrum*. These observations gave me the basis of a fellowship application: I would study the adaptive formation of catalase in photosynthetic bacteria (this was just before the terminology 'induced enzyme synthesis' came into vogue).

Parenthetically, these early experiences about catalase gave me my first inkling that it might help sometimes to change research materials. I have come to know the great value of trying different organisms rather than staying wedded to just one. *Rb. sphaeroides* proved later to be of the utmost value for isolating photosynthetic reaction centres.

The National Science Foundation awarded me a Fellowship to go to England, to work in the laboratory of D.D. Woods at Oxford. The last words we heard as we embarked, spoken by our dear friend Milton Mayer, were, 'Well, kiddies, your salad days are over.'

Our stay in the England midlands was made less than delightful by the Asian Flu, which laid all four of us low, and by the Suez crisis, which engendered in some quarters an aura of hostility toward Americans because of the Eisenhower administration's stance in the matter. Be that as it may, we spent the better part of a year at Oxford and then set sail for Trondheim, to Helge Larsen's laboratory at the Norwegian Institute of Technology. That was a good experience, between the abundant snow (my first taste of skiing, on the big Norwegian touring boards), Helge's warm hospitality, and eventually the midnight daylight.

During that year I outlined some of the basic physiology of induced catalase synthesis in *Rb. sphaeroides*. I also learned that being in Europe did not make it easy for me to find a job in the United States. Luckily I came to the attention of Stan Carson of the Biology Division of Oak Ridge National Laboratory, and in the fall of 1958 we pulled into Knoxville, Tennessee on a train: ourselves, our clothes and an enormous amount of liquor that we had won on the steamboat playing Bingo. Knox County was 'dry', so we arrived in a state of great illegality.

I felt that Biology was the strongest Division at Oak Ridge National Laboratory, owing to the vigorous personality of its Director, Alexander

Hollaender. He was tough and paternalistic. We all respected him, some liked his ways, and some, including myself, feared him (I had a trembling fear of authority figures). Those who feared Alex either avoided him, as I did, or became his sycophants, of which he had quite a coterie.

Alex (I never dared call him that) had created a perfect environment for a young investigator to do research to his heart's content; everything was beautifully organized and facilitated. I stayed with the study of induced catalase synthesis for another year, developing mutants such as a strain of *Rb. sphaeroides* in which as much as one fourth of the total protein could be catalase.

But Bill Arnold was there, and I was soon drawn toward his interest in photosynthesis. Bill's life was science. He loved to sit behind his pipe and speculate on the more paradoxical aspects of nature. An excellent intuitive physicist, he would not let himself be absorbed by the mainstream of thought, preferring to keep his hypotheses on the fringe of accepted believability. His style was the opposite of mine; he liked to think for a long time before launching an experiment. His literary style, too, was opposite to the flowery way of writing that I had cultivated. From Bill I learned to be more economical and to the point in my composition, and to appreciate the value of such terse sentences as 'It does'.

Bill Arnold was intrigued by the notion that the photosynthetic unit behaves like a semiconductor, with electrons and holes migrating in the antenna until they are trapped at special reactive sites. This idea had more heuristic than practical value. It was fun. Most important for me, I was reminded of the publications by Emerson and Arnold thirty years earlier, proposing the existence of photosynthetic units. This prepared me to recognize photosynthetic reaction centers when I first saw evidence of them.

As soon as I was drawn into the study of photosynthesis I began to feel like a biophysicist. Heretofore, all my preparation had let me be a microbial physiologist who could fix his own amplifier and solve his own differential equations. Now I actually found myself bringing physics, chemistry and biology together in my work.

These thoughts take me back to an earlier time, the summer of 1940. I was eighteen, and I fell into an opportunity that must have influenced by career, although I was not aware of it then. I was allowed to be an apprentice assistant in the laboratory of James Franck in Chicago, with Stacy French my immediate supervisor. I measured the time course of fluorescence in leaves and chloroplast preparations, using a device of Stacy's invention. Sometimes I did a 'real' experiment, such as examining chloroplasts with graded concentrations of ascorbate. But in my spare time I brought in as many different leaves as I could find and measured their fluorescence induc-

tion transients. I fancied that a taxonomic key could be based on the shapes of these transient patterns.

Once James Frack caught me doing a handstand on a laboratory bench. I learned indirectly that he was pleased. And I remember breaking Theodore Puck's Warburg vessel. He was not pleased.

Most of all I remember the kindness of Stacy and Margaret French, who smiled through my gaucheries and took me camping with them to Devil's Lake, Wisconsin. They have remained good friends throughout my life even though I see them rarely.

As for James Franck, I came to know and admire him through occasional contacts over the years. I once gave a talk at Duke University with Franck in the audience. He bore in relentlessly throughout my lecture, with questions and challenges that I fielded as best I could. At the end my host was terribly embarrassed and I was exhilarated. I saw Franck a few months later, in a hotel room in Florida, about a year before he died. He expounded to me his theory of $n\pi$ states in photosynthesis. He concluded by saying, 'I know that I do not have much longer to live, and that in a year or two this will all be nonsense, but just at this moment I have the exquisite thrill of knowing *exactly* how it all works!'

And now back to Oak Ridge and Bill Arnold, around 1961. Bill was studying the delayed fluorescence of chlorophyll in leaves and algae, a phenomenon that Bernard Strehler and he had discovered recently. In some tests he would paint suspensions of algae, or of bacterial membrane fragments, onto the disc of a phosphoroscope and allow them to dry before measuring the emitted light. In some aspects these dried films were quite lifelike.

Bill had just read a paper from Chance's group showing light-induced cytochrome oxidation in preparations from photosynthetic bacteria at liquid nitrogen temperature. In his imaginative way Bill wondered if drying would give the same effect, of slowing diffusion-controlled reactions, as lowering the temperature. If so it would be a far more convenient approach. We tested this with dried films of membrane fragments from photosynthetic bacteria, allowing the suspensions to dry onto glass slides. Dried films from *Rs. rubrum*, *Rb. sphaeroides* and *Chromatium* did indeed show light-induced absorbance changes corresponding to the oxidations of bacteriochlorophyll (bchl) and cytochrome. The oxidation of bchl was reversible even at 1 °K. We took this as direct evidence that the 'first step' in photosynthesis is of an electronic nature.

Dried films of photosynthetic tissues are ideal for the biophysicist who wants to avoid the messy wet activities of biochemistry. I soon had a large collection of such films, and they remained active after lying in my desk drawer for years. But having been introduced to the wonderland of light-

induced absorbance changes, I was looking at everything from films to living cells.

L.N.M. Duysens and his colleagues had already described these absorbance changes in liquid suspensions of cells and membrane fragments, and had proposed the oxidation of bchl as an early step in bacterial photosynthesis. I was drawn right away to Emerson and Arnold's appealing model of an antenna of pigments serving a special reactive site. I wondered if the oxidation of bchl was part of a 'primary' event in such a reaction center. The magnitude of the change suggested that one out of 50–200 bchl molecules could be oxidized reversibly by light. But at the time, there was no way to know whether the absorbance change reflected a small alteration of all the bchl in the tissue, or a gross change in a small fraction of 'special' molecules (the ones that are part of a photochemical reaction center). Luckily I got an answer to this question because I took a two week vacation.

I was growing cultures of a carotenoidless mutant, Strain R-26, of *Rb. sphaeroides*, derived from a high catalase strain. I had developed it in order to ask whether a very high level of intracellular catalase could protect the creature against photo-oxidative injury (it could not). The mature cultures were of a lovely slate blue color. On returning from vacation I found that the cultures, left in the light cabinet, had undergone some sort of senescent transvestism. Their color had changed from blue to pink. Nearly all of the blue bchl had changed to the lavender-pink bacteriopheophytin (bph). The long wave absorbance maximum of bph is at 760 nm, whereas that of bchl in vivo is at 870–890 nm. The absorption spectrum of the pink cultures of *Rb. sphaeroides* showed a large peak at 760 nm and a small residual one at 870 nm; not quite all of the bchl had changed to bph. I was excited to find that nearly all of the remaining 870 nm band could be bleached reversibly by light. The entire spectrum of this change, with many details from 280 to 1250 nm, was exactly like that in material from young cultures having a normal complement of bchl and very little bph. This, together with the magnitude of the change, showed that the light-induced oxidation of bchl was indeed the property of a small fraction of the total pigment, a special component that was also immune to any senescent conversion to bph. Right away I took the simple view that this 'special' bchl belonged to a photochemical reaction center (RC), and that its oxidation was part of the primary photochemistry of photosynthesis.

I have always trusted and valued simplicity, in science as well as in art. Of course there are complications behind any simple picture, some of them known and some not. Some are irrelevant and some are pathways to new insights. It is possible to stay aware of the complications without becoming lost in them or discouraged by them. Too often I have seen scholars so

involved in a complex picture, and so conservatively careful to be scrupulous in their scholarship, that they cannot embrace and pursue a simple idea that is right under their noses. Yes, the simple idea might be wrong, but if it is fertile it will lead to more experimentation and more thought and more discovery.

There are also scholars, young and old, who read the literature so avidly that they can believe that any experiment they may think of has been done (properly) or is not worth doing. I'm lazy at the library and would rather be in perpetual danger of reinventing the wheel. Recall Shakespeare's Hamlet: 'Thus the native hue of resolution is sicklied o'er with the pale cast of thought.'

So I went ahead with the idea that the oxidation of P870 (a phenomenological term for the photoactive bchl in the RC) is part of the first chemical step in bacterial photosynthesis. Not a side reaction; not a safety valve for the disposal of excess excitation energy. More important, I was ready for the more central and fruitful idea that RCs exist and can be isolated.

At this point in Oak Ridge I felt that I had gotten a good start in photosynthesis research, and I was grateful to Bill Arnold for his part in this, but I was ready to move on.

The years from 1962 to 1966 took me, and B.J. and Rick and Ann, from Oak Ridge to Dartmouth Medical School and thence to the C.F. Kettering Research Laboratory in Yellow Springs, Ohio.

Clint Fuller had invited me to Dartmouth, and I found that the Medical School there was freshly alive with basic science, thanks to the recent arrivals of physiologists and biochemists such as Andrew Szent-Györgyi, Sinya Inoue and Mel Simpson, and of course Clint. These scientists became my entrée into the exciting world of the Marine Biological Laboratory at Woods Hole, Massachusetts; more of that later.

Dartmouth also provided my introduction to the thrill of downhill skiing, a love that has stayed with me to this day, making winter a season to welcome with excitement. One of the best teachers I have met in any field of endeavor is Simon Mayer, who heads the skiing school at the Dartmouth Skiway.

In the laboratory at Dartmouth, and subsequently at the Kettering Laboratory, I continued to refine the spectroscopic definition of the RCs in *Rb. sphaeroides*. There was always a little 'antenna' bchl that did not become converted to bph in aged cultures, and I found that a detergent such as Triton X-100, added to a suspension of membrane fragments, promoted the photooxidative destruction of that residual pigment. Then all of the remaining 870 nm absorbance showed reversible light-inducing bleaching. In fact, by adding detergent I had separated the RCs from the photosynthetic

membrane, but I was unaware of that. If I had gone right ahead with some simple techniques for purifying proteins I might have isolated the RCs in 1962 instead of 1967. In that instance I paid a real penalty for not reading enough and for not being willing to explore techniques that were not very familiar to me.

At Dartmouth the basic scientists had fallen into an imbroglio with the clinicians, with several key persons about to resign, so I left in favor of the Kettering Laboratory, with Leo Vernon as Director and Tony San Pietro as a senior biochemist.

Yellow Springs, Ohio was our fourth place of residence in six years, not counting the European adventure. A positive consequence of having changed jobs so often was that my salary had risen faster than it would have if I had stayed at one place. In the negotiations, before a final commitment, I could ask for the moon and hopefully get more than just a slice of it. But when new friends asked where we were from, we would say simply, 'the road'.

In Yellow Springs the pattern of daily experimentation continued, punctuated by exciting moments such as finding the fluorescence of P870 and discovering the light-induced shift of the absorption bands of antenna bchl, analogous to the carotenoid band shift. Probably the best new finding in that period, from 1963 to 1966, was made by Berger Mayne in my laboratory. He discovered the chemically induced luminescence of chlorophyll in chloroplasts; as a chemical perturbation he used the acid-base shift that had recently been described by André Jagendorf. It turned out that this was a chemical stimulation of delayed fluorescence. In any case it led to many experiments in other laboratories, using various chemical stimuli. We had one more observable phenomenon; another window into the pathways of electron flow.

It was in Yellow Springs that B.J. agreed to let me train her to work in the laboratory. From then until her death in 1981 she was my daily working companion, a good playmate and a great help to me. She soon showed a fine aptitude for the subtler, more intuitive aspects of microbial and biochemical techniques. She knew when something was just a little bit wrong with a culture; she was the only person other than myself whom I would trust to maintain my collection of bacteria.

B.J. was naturally social and comfortable with people. She could be assertive in confronting an occasional miscreant in my laboratory, where I usually could not. And she could do this without generating any resentment in spite of her unique position of being both a technician in the laboratory and my wife. Also she soon undertood how hard it can be to leave the laboratory in the afternoon and get home at a decent hour.

Work was my life, and I know now that it was the loving presence of my wife that sustained me: that let me be a compulsive daily work addict without going entirely crazy.

I can remember many dinner parties at which I, the loner, watched the hands of the clock go around while B.J. charmed my colleagues and their families. They were my friends too, but I wanted only small doses of other people. When Rick was four years old he said, "Daddy, what's your favorite word?" I didn't say 'love'; I said 'work'.

At the Kettering Laboratory the work began to bear fruit in that I was becoming 'known' in my field. I felt the exhilaration of being asked to be an invited speaker or chairman of a session at a conference, or to write a review article or even a book. Of course these activities kept me out of the laboratory, and I could foresee the pitfall of changing from a scientist into a writer, lecturer, organizer, and (horrors!) administrator. But it was heady stuff at the time.

An especially titillating part of this new gratification came when I was asked to participate in the Physiology course at Woods Hole; I saw this as the blue ribbon course of the Marine Biological Laboratory. I came as a guest lecturer for a couple of years and then, from 1967 to 1971, I was an instructor in the Physiology course. What excitement – I had arrived! Every summer, at the cost of considerable disruption at home, I would take half of the electro-optical equipment from my laboratory to Woods Hole and set it up for the use of the students and myself.

In retrospect I have mixed feelings about those summers. It was indeed a hotbed of intellectual excitement, and with the beach right underfoot I didn't spend *too* much time making experiments. But there was also a curious edge of anxiety there. For too many of us it was a certificate of success to be there, and there was a certain tension underlying the annual recertification. Too much importance was given to an invitation (or lack of one) to a major book publisher's cocktail party. I'm glad and grateful for the experience and I'm happy to have it behind me.

So – the Yellow Springs years were marked by a new level of comradeship with my wife and a burgeoning sense of recognition in the scientific community. By 1965, persons at several major universities were showing an interest in me. After the customary dances of mutual appraisal Cornell emerged as my first choice, and I agreed to come there as a Professor.

At Cornell I could foresee, for the first time, that I could be happy to remain where I was for the rest of my professional life. Ithaca was attractive, Cornell was a great university, especially in the sciences, and skiing was nearby.

I came to Cornell with a dual appointment in Biology and Applied

Physics. It was hoped that I could generate new interactions between physicists and biologists, perhaps with some sort of Center for Biophysics. But I had no taste at all for building administrative structures, and I didn't feel the potential value of such a Center. I believed, and still do, that physicists, chemists and biologists will find each other if they have a point of mutual interest, and will enter into a collaboration if they can help each other in a research program. I think that administrative efforts to promote greater interaction will yield little more than that.

For myself, I enjoyed teaching but my first love was to work in the laboratory. I turned my back on any sense of obligation to serve in administration. When asked to be on a committee I didn't say 'No'; they would only keep on asking. I said, 'Yes, sure,' but I went to no more than one meeting out of two, avoided looking at the chairperson, and somehow failed to do much of the homework assigned to me. Soon – very few committees. I don't recommend this to anyone else; it's just the way I was.

I did form one committee, on a request to look into the Calculus offering for non-science majors and to suggest improvements. To learn at first hand, I volunteered myself as a teaching assistant and met classes for recitation. It was 1968, a time of minimal academic motivation among the students. Extreme motivational overkill by the teacher would elicit a barely perceptible response. Even so, it was fun, and we generated new courses that made more sense to these students.

My regular teaching was confined to two recreational courses, Photosynthesis and Vision. That, plus some contributions to laboratory courses on Plant Physiology. As a teacher I tended to be a didactic steamroller; I would get so carried away with the subject matter that the students would hesitate to interrupt me with questions, even though I had urged them to do so. On the first day of class I couldn't resist pointing out that the word 'photosynthesis' could be rearranged to spell 'snot hypothesis.' So much for levity in the classroom.

In the laboratory I was finally ready to approach the problem of isolating the RCs from *Rb. sphaeroides*. It seemed natural to start with suspensions of membrane fragments from material that was the most promising spectroscopically, the carotenoidless strain R-26, treated with the detergent Triton X-100. I suggested to my first Postdoctoral Associate at Cornell, Dan Reed, that he apply some simple techniques for fractionating proteins to this material. The simplest methods with which I had had any experience were centrifugation through sucrose density gradients and fractional precipitation with ammonium sulfate. These proved sufficient, and we soon had our first rather crude samples of 'purified' RCs.

The subsequent years at Cornell, from 1968 to 1980, are in my memory

a potpourri of personalities in the laboratory, local events, conferences and travel, superimposed on the fabric of daily research.

The refinement and definition of reaction centers progressed little by little, along with the extension of this work to creatures other than *Rb. sphaeroides*. We accumulated morsels of data such as the quantum efficiencies, kinetics and redox properties of early and more peripheral events in bacterial photosynthesis. With time, after the exciting early exploitation of the isolated RCs, I began to feel that I was merely tidying up odds and ends, dusting a corner here and polishing a doorknob there.

Given the current state of knowledge, I began to feel that if I were to discover something significant I would have to invest two or three years in hard study: chemistry appropriate to the problem of oxygen evolution, for example (it would never have occurred to me that an amino acid residue in a protein might be an electron carrier in Photosystem II). The idea of such an investment was too forbidding. But the infusion of fresh approaches and ideas, by new people in the laboratory, kept the atmosphere fresh and vital around me and in me. Thank you all, my extended family in science! Also thanks to AEC, ERDA, DOE and NSF!

I won't try to comment on every person who worked in my laboratory at Cornell, or with whom I have collaborated. I'll make a list, and then reminisce about a few with whom the personal relationship became special.

Graduate Students: Yoram Barouch, Bob Downs, Nate Levine, Don Middendorf, Tom Owens, Mike Sheetz, Sue Straley and Hume Vance. *Postdoctoral Associates and Visiting Scientists*: Dan Brune, Don Bryant, Richard Cogdell, Tom Ebrey, Peter Heathcote, Chuck Rafferty, Dan Reed, Lou Sherman, Bill Sistrom, Terry Trosper, André Vermeglio, Richard Wang, Colin Wraight, Tomoko Yamamoto, Hon Yau, and Ken Zankel. *Peripheral Collaborators*: Jim Bolton, Jacques Breton, Don DeVault, Les Dutton, Darrell Fleischman, Bob Haselkorn, Stan Holt, Jack Leigh, Dave Mauzerall, Garth Nicholson, Bob Niederman, Itzakh Ohad, John Olson, Bill Parson, Roger Prince, and Ken Sauer.

Don Middendorf was the graduate student with whom I formed the closest personal ties. The word 'sweet' sounds trite, vapid and cloying. But Don *is* sweet, in the best and strongest sense of the word. He is sympathetic, understanding and loving while retaining a firm sense of his own self. He now teaches at Evergreen State College and has a base for more technically sophisticated research in Bill Parson's laboratory at the University of Washington. I envy Don's students; he has such infectious curiosity and awareness of the excitement to be found in science and in life generally. He and Bill Parson – what a pair they are! In my mind Bill is like an older brother to Don. He has the same enthusiasm; the same generous and engaging

manner. I think that they differ in that Bill channels a greater fraction of his time, his self, to the laboratory, although (taking myself as an extreme point of reference) he is not unbalanced in that direction. It's an enriching experience to know Bill, and exciting to collaborate with him.

Colin Wraight came to my laboratory in 1971, a fascinating apparition wearing an Edwardian see-through shirt, a wispy beard and a philosophical smile. His excellence as a scientist and his awareness of things artistic and human were soon evident. Colin went on to establish himself solidly at the University of Illinois, and our friendship has grown over the years. When I began to slide into my self-inflicted bad years, which I have yet to describe, Colin foresaw the deadly outcome the most clearly and showed the most concern. He tried to warn me; he cared, but at the start of that period I didn't want to hear. We shall always be caring and loving friends.

André Vermeglio arrived in 1975 from Saclay, near Paris, from a laboratory with excellent experimentalists and theoreticians and from a background of studies mainly on green plant Photosystem II. With me he got his first large taste of working with photosynthetic bacteria, and I can still hear his constant refrain drifting across the hall: 'Just like system two!'

Chuck Rafferty had heightened my interest in measurements with polarized light, showing me how RCs could be oriented in stretched gelatin films so that they showed strong linear dichroism. André had had experience with orientation by means of electric and magnetic fields, and with the technique of photoselection. So we launched extensive studies of orientations of the transition moments of chromophores in RCs and in membrane fragments; these were exciting times for me because I was learning again. I was especially pleased to find a use, for the first time since high school, for spherical trigonometry. I remember worrying that some day the RC's would be crystallized and their structure elucidated in detail, and that our measurements with polarized light would then be retroactively superfluous. Be we gained many insights into the chromophores and their interactions, beyond the mere knowledge of orientations. Now, through the splendid efforts of Michel and Deisenhofer, the structure of the RC has been defined crystallographically, and it is gratifying that our conclusions on orientations are compatible with the structure.

André is a truly excellent scientist. He can take a balky instrument and a messy experiment situation and swiftly generate function, order, and conclusions that are valuable and reliable. Coupling these talents with fine qualities of kindness, patience and humor, he is a most remarkable man and a valued friend.

Of course, there are others on my list of associates who are superior scientists and/or splendid human beings; good friends. I have singled out

Don, Bill, Colin and André because we have stayed in touch and kept our friendship alive over the years.

Beyond the laboratory work and the personalities, many things came to me in the 1970s that were rewarding, exciting or just plain fun.

There was the travel. Rick and Ann had asserted their independence and flown the nest, and B.J. and I were left to ourselves and each other. Travel can be challenging to couples. At home they have developed ways of pursuing their lives in parallel, being together or not, hour by hour, as they choose. Those well established and often subtle patterns don't work when traveling. But I remember that we always had a good time, seeing new things, and old and new friends, wherever we went.

Then there were the books. As you can tell from this narrative, I kept my interests and most of my knowledge focused quite narrowly on bacterial photosynthesis in general and on reaction centers in particular. Writing a book was a good exercise in learning. In my last book, *Photosynthesis: Physical Mechanisms and Chemical Patterns* (Cambridge University Press, 1980), I tried to give appropriate weight to green plant photosynthesis and to areas such as ATP formation with which I was not very familiar. It was invigorating to learn these things and then to get something out of the 'green plant' sessions at meetings.

Finally there were the honors, which I of course wanted and which felt good when they came. I was elected to the American Academy of Arts and Sciences in 1975, and to the National Academy of Sciences in 1977. I remember the glow, and I remember Jack Myers' remark: 'I'd a lot rather people were asking why I *wasn't* in than why I *was* in.' And there was the first annual Prize in Biological Physics of the American Physical Society, given to me and George Feher in 1982.

B.J., my life companion, died in September of 1981 and my life, as I had lived it until then, died too. Her passing was not as prolonged or cruel as I had feared, and we were at a place we both loved – our country house in Thetford, Vermont. She died in the night, and the next day I spent with the telephone, satisfying a need to make contact with as many people as I could. The following day it came to me that I was free to obey any whim at any time. I could smoke a little marijuana with a friend. I could have the adolescence that I fancied I had missed. Within a few months I was exploring every kind of drug I could find (thankfully, never narcotics and never the needle). I was a pretend hippie, a pretend redneck (I learned to say 'Say what?' instead of 'What did you say?'), and a brother to black people. I made myself a public outrage. At first I was reveling in a sensory kaleidoscope, but soon it was not enough to get 'high' every day; I had to get as far removed from reality as I could. I didn't realize it then, but I was running

away from the shock, the dislocation in my life, that B.J.'s death had brought.

The early pleasures wore off and I tried to recapture them by using even more. By the spring of 1985 I had reached the end of the trail. I had already had to retire; in this process the administrators at Cornell were exceptionally gracious, supportive and generous. I had turned my back on my friends and colleagues, I had burned up most of my money, and I was ruining my body. My life was saved by an intervention that brought me into a rehabilitation program for six weeks and from there into a system of support that helped me to learn how to live.

Now, for the first time in my life, I'm becoming aware of a real self, of the realness of the people around me, and of the full pleasure of sharing friendship and love with others. I'll probably always have my compulsive nature, but I'm not such a loner any more. For that, and to that extent, I'm grateful for the turn that my life took. As I write this, in July of 1988, I have just returned from a Gordon Conference of Physico-Chemical aspects of Photosynthesis, to which Colin Wraight (this year's Chairman) invited me. It was my first scientific meeting, other than the National Academy of Sciences Meeting, in many years, and it proved to be one of the great experiences of my life, for several reasons. I was pleased to see that the science still made sense; that its growth had not passed beyond my comprehension of it. Reaction centers had been crystallized and characterized by X-ray diffraction; it was marvelous to have a clear mental picture of their structure. Being the first membrane protein to have been crystallized, RCs are in the thoughts of many biochemists outside of photosynthesis. It was gratifying to see that most of my earlier findings, made on a trail littered with luck and faith, have remained essentially correct in the light of this new knowledge.

Best of all was the sense of coming home. I was overwhelmed by the warmth of old friends welcoming me back. I had turned away from and shut out my entire past, and had been trying to build a new life based on human awareness and artistic endeavors. To live a new and different life, at my age, brought a strong feeling of disorientation and aloneness. Now I feel that my continuity with the past has been restored. I look forward to more conferences and to adventures of research in friends' laboratories. The sense of strangeness and aloneness is gone, at least today, and I look forward to the best of lives – the old blended with the new.

After writing this, I learned that the 1988 Nobel Prize in Chemistry was awarded to Michel, Deisenhofer and Huber for the crystallization and structural analysis of RCs from *Rp. viridis*. It is satisfying to know that my work, 20 years ago, had paved the way for this event, just as the work of

Emerson and Arnold, 30 years earlier, had set the stage for my contributions.

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