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CHARLES STACY FRENCH
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A Biographical Memoir by

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CHARLES STACY FRENCH

December 13, 1907–October 13, 1995

BY GOVINDJEE AND DAVID C. FORK

STACY FRENCH WAS BORN on December 13, 1907, in Lowell, Massachusetts, and died at Stanford, California, on October 13, 1995. The Botanical Society of America Merit Award, in 1973, described C. Stacy French as a "skillful and persistent investigator of the spectral properties and state of chlorophyll in tissues; inventor and *gadgeteer* par excellence; able and genial administrator of a productive center of botanical research—Carnegie Institution of Washington at Stanford."

Stacy was a key figure in the revolution that deciphered the photochemical events of photosynthesis, beginning with his elegant work in 1952, where he demonstrated, with Violet M. K. Young, efficient excitation energy transfer from the red and blue pigments (the phycobilins) to the green pigment chlorophyll (Chl) *a*. Stacy's later work is known mostly for the state-of-the-art analysis of the different spectral forms of Chl *a*-protein complexes and their function in the different photosystems of algae and plants.

The invention of instruments and the pioneers who displayed them have played key roles in the advancement of all the sciences (Jardine, 1999). Stacy French was one of those inventors. His prestigious inventions include the "French pressure cell" for breaking of cells; the first automatic recording fluorescence spectrophotometer; a huge (in size) curve analyzer and general purpose graphical computer; a derivative spectrophotometer; and a patented optical range-finder-based land-surveying instrument. He received many honors, including election to the National Academy of Sciences and American Academy of Arts and Sciences, both in 1963; membership in the Academie der Naturforscher Leopoldina in 1965; the Charles Reid Barnes Life Membership in the American Society of Plant Physiologists (now American Society of Plant Biologists) in 1971; the 1973 Merit Award of the Botanical Society of America; and honorary doctorate from Göteborg University in 1974.

In composing this memoir for Stacy French we shall sometimes use the thoughts of his collaborators, which are recognized only partially in the acknowledgment at the end. One of us (D.C.F.) was Stacy's close colleague for 35 years and the other (G.) was also for almost 35 years a distant colleague, a scientific admirer, and a friend—all of which accounts for the personal tone of this account. Both of us feel that Stacy was a mentor in the true sense of the word. We have freely drawn information from the text that was written by D.C.F. and edited by G. (Fork, 1996) and by Stacy himself (French, 1979).

Stacy's father, Charles Ephraim French, born in 1864 in Berkeley, Massachusetts, was a physician who had a good practice in Lowell, specializing in ear, eye, nose, and throat diseases. Stacy's mother, Helena Stacy, born in 1867 in Colebrook, New Hampshire, was a kindergarten teacher. Stacy did not consider his early education in Lowell to be any good. As a child he suffered quite a bit from respiratory infections, but had the benefit of a well-educated tutor, Flora Ewing, who guided him in kitchen-scale experiments in addition to the basics of Latin, English, and Algebra. From his father he learned to do basic carpentry, wood-working, and house painting, skills that came in handy in his later research life. French (1979, p. 2) recalls: "I entered Loomis in Windsor, Connecticut, in 1921, failed the first year, repeated it the next and later, when the time came, was refused permission to take the Harvard entrance *so as not to spoil the school's record.*" His mother enrolled him for one year in Lowell High School, and from there he was admitted to Harvard via a summer school course in botany.

Helena Halperin, daughter of Stacy, recalls:

> From his father, Stacy also learned his life-long love of wilderness. As a young man, he was an avid skier and a serious and capable climber. He climbed the Matterhorn in the 1920s, before it was common. During part of his childhood in Massachusetts, he went camping every weekend, year round. He helped build the Appalachian Mountain Club (AMC) hut at Franconia Notch, New Hampshire, and spent a great deal of his youth in the Appalachians. As an adult, he took his wife and children camping many weekends, and almost every vacation. Often, he'd roll out of the tent long before others, go fishing, build a fire, and serve fresh trout when his family was ready for breakfast.

In his first year at Harvard, Stacy seemed interested in pursuing engineering (including mathematics and physics), but in his second year he became interested in biological sciences, particularly general physiology, mainly because of the laboratory with good instruments (instruments seem to be one of the threads in Stacy's life). Stacy's undergraduate adviser was a prominent mathematician George D. Birkhoff. He met in 1928 his first mentor, Robert Emerson (1903-1959),[1] from whom he first learned about photosynthesis, and he wrote, "I have stayed with the subject ever since." His undergraduate thesis dealt with the temperature coefficient of catalase action under the guidance of W. J. Crozier and A. E. Navez. Stacy received his B.S. degree from Harvard in 1930. Another example of his open and frank personality is in his statement, "[My undergraduate thesis] very nearly kept me from graduating with my class in 1930 through lack of attention to the math and organic chemistry classes" (French, 1979, p. 3).

For his Ph.D., Stacy attended Harvard, where he was among a group of graduate students who organized the Chlorella Club. The club was organized because students "felt the department seminars were too dominated by the professor" (French, 1979, p. 4). William Arnold, who had in 1932 discovered with Robert Emerson what later became known as the photosynthetic unit concept[2] (i.e., thousands of chlorophyll molecules feeding excitation energy to a few photoenzyme molecules) and Caryl Haskins, who later became president of the Carnegie Institution of Washington, were prominent actors in the Chlorella Club. Stacy states: "[It] gave us more education than any of the biology courses" at Harvard. For his doctoral research Stacy worked on the rates of respiration in the green alga *Chlorella* at different temperatures. He accumulated a lot of experimental data and found it almost impossible to write his thesis, but according to Stacy (French, 1979, p. 4), a postdoctoral fellow from China, Pei-Sung Tang, saved his academic life by helping him separate the scientific wheat from the chaff of his innumerable experiments. Stacy worked with Tang on the effects of oxygen pressure on respiration of *Chlorella*; this led to Stacy's first scientific paper in the *Chinese Journal of Physiology* (1933). Stacy received his Ph.D., in biology, from Harvard in 1934.

Not being able to stay at Harvard, Stacy accepted a position at the California Institute of Technology with Robert Emerson to study photosynthesis of purple bacteria. In preparation for this area Stacy spent the summer of 1934 taking Cornelis B. van Niel's famous course in microbiology at the Hopkins Marine Station in Pacific Grove, California. Van

Niel was known to lecture for four to six hours without losing his students' interest. There were, however, distractions at Caltech for Stacy, such as excellent skiing in the mountains in the area. Emerson, who one of us (G.) knew well, was in Stacy's words "justifiably disgusted with my performance and we were barely on speaking terms for the academic year. . . However, in spite of my poor performance, he arranged for me to spend the next year in Berlin with Otto Warburg, which was what saved my scientific career" (French, 1979, p. 5). It seems that there were a number of times when the assistance and generosity of others allowed Stacy to develop his own skills as a scientist and a successful scientific career.

Since research with the Nobel Laureate Otto Warburg[3] was a crucial turning point in Stacy's career, we quote here Stacy's own statement (French, 1979, p. 6):

> The year with Professor Otto Warburg at the Kaiser Wilhelm Institut, (now the Max Planck Institut) was one of intense concentration on the efficiency and action spectrum of photosynthesis in *Rhodospirillum rubrum.* Living in the laboratory building and eating at Harnack House just around the corner in Dahlem was most convenient. The laboratory was excellently supplied with optical equipment left over from Warburg's determination of the action spectrum for dissociation of the CO complex with the respiratory enzyme, which had brought him the Nobel Prize. I was treated very well by the professor and all his staff. The rigid discipline and long hours without outside distraction were just what was needed to convert an easygoing freewheeling academic type into a professional scientist.

Stacy further wrote: "I am grateful to Warburg for refusing to speak English though he could do it better than I." Stacy's measurements of hydrogen and carbon dioxide photoassimilation in purple bacteria were published in *Science* (1936); his measurements on the quantum yield and action spectrum of bacterial photosynthesis were published soon thereafter (1937).

French held a teaching fellowship from 1936 to 1938 at the Harvard Medical School. Here, with the help of Professor Theodore Lyman (of Harvard's physics department), he measured absorption spectra of photosynthetic bacteria. It was at this time that the seeds of the French press may have developed in his mind. With the help of Professor G. W. Pierce, he was the first to use a supersonic vibrator to break photosynthetic organisms.

Stacy went to work with another Nobel laureate, James Franck,[4] at the University of Chicago in 1938. The appointment with Franck (known for the Franck-Condon principle, among many other things) was that of an instructor (research) in chemistry (on an annual salary of $2,400). This job in 1938 required Stacy to help Franck establish a photosynthesis laboratory. It was during this period that Stacy married Margaret Wendell Coolidge, who was the daughter of the first master of Lowell House at Harvard, Professor Julian L. Coolidge. It was Margaret who helped repair Stacy's friendship with Robert Emerson.

Although Stacy published, with Franck and Ted T. Puck in 1941, one of the earliest quantitative papers on the relationship between chlorophyll *a* fluorescence and photosynthesis, Stacy's philosophy and personality did not match Franck's. Franck's approach was too theoretical for Stacy's tastes. One of us (D.C.F.) wrote in 1996: "In essence, Stacy was Franck's technician and was expected to do the critical measurements to support Franck's current thinking. Stacy, being an inveterate experimentalist, wanted to measure as many of the parameters of photosynthesis as possible and then try to explain their significance but Franck had no appreciation for this approach." Although Stacy was unhappy working with Franck, he did associate with Hans Gaffron[5] (a former Warburg associate), Roderick Clayton,[6] Robert Livingston, and Warren Butler.[7] At Chicago, Stacy measured oxygen evolution in isolated chloroplasts according to Robin Hill's procedure with visiting biochemist Mortimer Anson of the Rockefeller Institute. They proposed the name "Hill reaction"[8] in 1941 for the effect (French, 1979, p. 10). An aside: When Stacy was with Franck, Clayton was an undergraduate student helper whose assignment was to prepare neutral density filters. Stacy recalls (French, 1979, p. 10) that these very filters made from metal screens had been used by him in many experiments.

Stacy was able to "escape" from Franck after accepting a position in 1941 as an assistant professor of botany at the University of Minnesota. The invitation had come from George Burr (who was once Jack Myers's thesis advisor). Here he mentored his one and only doctoral student: A. Stanley Holt. French and Holt together studied the Hill reaction with various dyes, and, using ^{18}O experiments, showed that oxygen came from water in isolated chloroplasts (1948,1,2). (Sam Ruben, Martin Kamen, and coworkers had already shown it to be the case in whole algal cells.) Stacy had a stimulating academic time in Minnesota. With Holt and Glenn Rabideau, Stacy measured absorption spectra of leaves and algal cells, after first setting up a laboratory-made monochromator, and a large Ulbricht integrating sphere. In Minnesota, Stacy met, among others, Allan Brown (who has done some of the pioneering mass spectroscopic measurements, separating respiration from photosynthesis) and Albert Frenkel (who later discovered photophosphorylation in photosynthetic bacterial membranes).

During World War II, Stacy asked his neighbors to give him all their grapefruit rinds so that he could grow penicillin for war use, thus avoiding conscription. In addition to teaching plant physiology he taught elementary physics, and researched chlorophyll-containing paint for camouflage purposes in order to satisfy the draft board.

Stacy was promoted to an associate professor's position at the University of Minnesota, but left for Stanford in 1947 when he was appointed director of the Department of Plant Biology. The offer of the directorship from Vannevar Bush, president of the Carnegie Institution of Washington, and from Carnegie trustee Alfred Loomis was the best thing for Stacy's life and scientific career. He was happy to get away from Minnesota's long and cold winters and the associated respiratory problems they caused him. He accepted this challenging position and succeeded Herman Spoehr on July 1, 1947. Spoehr, Bush, and Caryl P. Haskins were his greatest supporters throughout their common period at Carnegie. Spoehr wrote on May 31, 1946, to V. Bush :

> I have been watching French for some time and have known him ever since he took his degree. . . French has grown very well, he is doing excellent work at Minnesota and impresses me as a very level-headed fellow and a hard worker. We have (*handwritten:* been) pretty close professionally because he is probably the livest [perhaps, he meant 'liveliest'?] worker in the field.

On November 14, 1946, Caryl P. Haskins gave the recommendation to V. Bush that he should hire French :

> It would be my opinion that Dr. French is definitely of the caliber to occupy a post of this sort [director's position]. As you know, he is personally very highly qualified and I would definitely feel that his qualifications on the scientific and technical side are equally high. He did very fine work in the field of photosynthesis at Harvard and his work since that time has been outstanding. . . I think I can say unreservedly that I would have no hesitation in recommending him for this important work. . . I think that both he and the Institution would be very fortunate."

Photosynthesis research reigned supreme during Stacy's long tenure (1947-1973) as director of the Department of Plant Biology; however, Stacy was equally supportive of the experimental taxonomy group with Bill Hiesey, David Keck, Jens Clausen, and Malcolm Nobs. Innovative biochemical investigations were devoted to photosynthesis by James H. C. Smith (formation of chlorophyll in greening systems), Harold Strain (chromatography of photosynthetic pigments), Harold Milner (chemistry), and Herman Spoehr (chemistry). A major project was the large-scale culturing of algae. This research led to a well-known book, *Algal Culture from Laboratory to Pilot Plant* (Burlew, 1953). Stacy was, however, most interested in the machine shop and the wood shop in the basement of the Plant Biology building. He spent more time in these shops, smoking his cigars and tinkering with instruments, than at his desk or in the laboratory. A most dramatic sight was the assemblage of ingenious pieces of biophysical equipment that Stacy and his postdoctoral fellows were building to study photosynthesis. Over the years the shop had the expertise of George Schuster, Louis Kruger, Richard Hart, Frank Nicholson, and Brian Welsh, and was open to all.

Stacy was a master at building ingenious instruments. He loved to conceptualize and develop new pieces of apparatus that would permit him to investigate new and different aspects of photosynthesis. Stacy was a pioneer in integrating methods from physics, engineering, and chemistry. In 1935 he had met the Russian professor N. N. Lubimenko, who believed in the existence of different forms of chlorophyll *a* in vivo. At Harvard he had broken photosynthetic cells by using supersonic vibrations. Vannevar Bush had made some suggestions about breaking cells through a fixed hole. Stacy wrote:

> To have a small hole that could be opened up when plugged and then constricted again led me to the idea of a *needle valve* instead of a fixed hole. I threaded an ammonia needle valve into a steel cylinder with a hole in the center to contain chloroplast suspension. The suspension was forced through the valve by a steel piston with a seal. This extrusion principle worked well and has since found much use for disruption of bacteria and

other cells as well as for chloroplasts. When I told Dr. Bush that the device was being made commercially, he suggested that I ask the manufacturer to attach my name to the device and to give a free one to the laboratory. The first request was acceded to at once, and about 20 years later the American Instrument Company gave the department a "*French Press*." We still drive it with an old hydraulic jack made for automobiles (French, 1979, p. 17).

In other words, Stacy accomplished the task of breaking difficult cells by releasing the pressure suddenly through a needle valve, the cells being forced out and broken by the resulting large shear forces (1948,1; 1951,2). This device is currently in use in many laboratories around the world.

Stacy also constructed the first automatic recording fluorescence spectrophotometer that simultaneously measured and corrected fluorescence emission spectra. This was done with two homemade grating monochromators and a rotating plastic drum on which was inked a correction curve followed by a photocell. We doubt that with the advent of many commercial fluorometers, anyone today would bother to build such an instrument. One of us (D.C.F.) wrote (Fork, 1996):

Stacy had a good relationship with the Carnegie astronomer Ira Bowen of Carnegie's Mt. Wilson/Palomar Observatories and he helped Stacy obtain large diffraction gratings and optical components needed to construct the big monochromators he used. At one time when results were not what were expected, Stacy found that a mouse had taken up residence inside one of these large monochromator enclosures. After this experience Stacy would sometimes claim an unexpected result was probably produced as a result of the mouse "peering out the monochromator slit."

Both of us loved this lighter side of Stacy's personality.

Later, Stacy fabricated an ingenious instrument to measure the first derivative of the absorption spectrum. With this instrument, he successfully detected minor spectral forms of chlorophyll *a*, whose absorption spectrum would have otherwise gone undetected. The method used the vibration of the slit of the monochromator over a few nanometer wavelength intervals. Although it took several years of Stacy's time to complete this spectrophotometer, it towered imposingly above its tables, and was a sight to be seen. Interestingly, several of its optical and electronic parts were from military surplus equipment; this was amusingly obvious from the note on one of its aluminum circuit boxes: "Do not remove from the airplane." Almost all visitors noticed it, and some even laughed. Stacy taught Jeanette Brown to run this "monster" (1959). Although he believed that these derivative spectra showed the existence of several forms of chlorophyll *a* in vivo, some remained unconvinced until the pigment-protein complexes were biochemically isolated and their absorption spectra measured. As a companion to the new spectrophotometer, Stacy built, with the help of his electronics assistant, Gordon Harper, another innovative instrument, a curve analyzer (1954). It consisted of five separate, vertically moving tables on which were drawn Gaussian curves. What a sight: The curves were followed by a photocell that tracked the curve as the table moved; a potentiometer recorded the changes as a voltage. This provided the data for the ordinate as the wavelength was recorded on the abscissa. This giant machine produced a curve that was the sum of the curves on the five tables.

Stacy would often change the parameters on each of the movable tables, using trial and error to estimate the various spectral forms of chlorophyll necessary to produce a resulting combined curve that would match the measured overall absorption spectrum. There was some relief as one could sit on a stool in the middle of the semicircle of tables and contemplate what and how much to change next. (Luxury was in sight when Stacy decided that the operator would sit on a swivel armchair.) Later, Stacy added a curve digitizer. Then, normal Gaussian-shaped component curves could be added together to fit the measured absorption spectrum: For most this was easier to accept and understand. When digital computing became available, Stacy and his coworkers were able to more easily analyze absorption, fluorescence, and action spectra; the existence of several forms of chlorophyll *a* (particularly, Chl *a* 662, Chl *a* 670, Chl *a* 677, and Chl *a* 684) became generally accepted (1972).

A visit by Francis Haxo to the department was a great pleasure to Stacy; he instantly fell in love with the rate-measuring oxygen electrode of Haxo and Lawrence Blinks for the measurement of the action spectra for oxygen evolution. The advantages of this polarographic technique—very small samples and the fast speed of measurement—became obvious to Stacy. Together with Per Halldal, Stacy obtained an action spectrum for oxygen evolution, using the Haxo-Blinks method. There was much excitement in the air: Robert Emerson and his coworkers had discovered an enhancement effect in photosynthesis when far-red light (absorbed in Chl *a*) was combined with light absorbed by Chl *b* and other pigments (Emerson et al., 1957), and Blinks had discovered a two-light effect in oxygen transients (Blinks, 1957). Ideas and experiments around the world were beginning to shape the concept that two light reactions must cooperate to bring about photosynthesis in plants (see Govindjee and Krogmann, 2004). Jack Myers and Stacy measured the action spectra for the Emerson enhancement and the Blinks effects and showed that alternation of lights absorbed preferentially by (what we now call) pigment system 1 and pigment system 2, even at intervals separated by 0.6 s could still produce enhancement: This was clear evidence for separate and lasting effects produced by the two photosystems of photosynthesis (1960). Both of us recall our own involvement in the Emerson enhancement effect that was parallel to this work of Stacy. Fork (cited in Haxo [1960]) had shown that one photosystem (now called "I") was indeed sensitized by Chl *a*, as he had shown both the blue (Soret) and the red bands of Chl *a* in the action spectrum of the Emerson enhancement when the second light beam was absorbed by the red pigment phycoerythrin in the red alga *Porphyra*. Govindjee and Rabinowitch (1960) showed that Chl *a* 670 was in the same system as Chl *b*, and it turned out that Stacy had also independently observed this (see 1961,1).

After Stacy became director of the Department of Plant Biology, President Vannevar Bush (see Trefil and Hazen, 2002) started a fellowship program for postdoctoral fellows and visiting investigators from around the world. Stacy and James Smith would visit laboratories in Europe to recruit outstanding scientists. Among others, they recruited Louis N. M. Duysens and Bessel Kok. These programs were very good for the Carnegie laboratory and for the visitors, creating friendships and cultivating the exchange of scientific ideas. Collaboration between Carnegie scientists and scientists throughout the United States and the world flourished under Stacy's leadership. Stacy felt rather strongly that for most scientists, significant intellectual life from within may not be that easy, and that intellectual growth is often dependent on interactions with others. An efficient way to become successful is to interact, he thought, with the "right people" and to learn from their lives. Stacy French was an outgoing friendly person, always jovial and full of humor. He seemed to buy scientists' time and then give it back to them with trust, and above all, he had great fun doing research with them. Stacy promoted a laboratory style that allowed scientists to think and do research independently. He stressed cooperation over competition among scientists. It was a wonderful place to be, as frequent, informal, and friendly discussions were encouraged. We remember well the scientific discussions that continued over lunch as we sat in the Adirondack chairs under the trees in the native plant garden and shared our food with scrub jays and brown towhees.

One of us (G.) remembers Stacy's generosity over the years: his acceptance of the independent observation of the presence of Chl *a* 670 in the Chl-*b*-containing system; his acceptance that G. was the first to see Chl *a* 670 in Chl-*c*-containing systems; and his insistence that we publish our work on the two-light effect, that we did in his laboratory in 1963, without his name, since as young beginning scientists we needed independent publications (Govindjee and Govindjee, 1965).

Though many of his friends did not appreciate Stacy's cigar smoking, his habit did have some use in the laboratory: There were plenty of cigar boxes to keep lenses, prisms, filters, photocells, and other small items. Some of these boxes may still be hidden in the basement of the current laboratory. Hemming Virgin told us a story that he once observed what he thought was an exciting new fluorescence band, but soon thereafter discovered this new emission band was seen only when Stacy was standing near the equipment, smoking his cigar. One of us (D.C.F.) remembers that Stacy seemed to enjoy leaning over a complicated optical or mechanical setup with a long ash clinging precariously to the end of his cigar with a comment such as, "What are you doing?" Sometimes the ash fell, causing minor embarrassment. His smoking did stop when his doctors told him to stop. (Also see Govindjee, 1989, for a presentation at Stacy's eightieth birthday.)

Everybody who met Stacy knew that he was not one for appearances. He usually drove an old car with an interior well worn from transporting dogs. He wore rumpled coats and baggy pants. Franck Nicholson told one of us (D.C.F.)

an amusing story about Stacy coming into the shop one day and asking for a needle and thread, and when these could not be found, mending his trousers with a stapler.

Stacy married his first wife, Margaret W. Coolidge, in 1938. They had a daughter, Helena Stacy Halperin, and a son, C. Ephraim French, as well as four grandchildren. Stacy and Margaret were wonderful and hospitable people; they hosted many great parties at their homes in Palo Alto and later in Los Altos Hills. Stacy and Margaret had one or two dogs of their own and at these parties even neighbor dogs would be welcomed to mix with the group, wagging their tails and hoping for some of the hors d'oeuvres. Almost everyone we know felt Stacy and Margaret's genuine friendship and loved the informal manner with which they made everyone feel welcome in their relaxed, warm home.

Stacy felt a great loss when Margaret died in 1992. After Margaret's death, Stacy met Lee Penland, a retired lawyer and they fell in love and were married in 1993. Together Lee and Stacy continued the tradition of extending warm hospitality to visitors of their hillside home. Memorial services for Stacy were held at Hidden Villa Ranch in Los Altos Hills on October 28, 1995.

The legacy of Stacy can be felt from the recollections of others. We present some selected quotes below.

Jack E. Myers, a longtime associate of Stacy French wrote (personal communication, e-mail message on May 4, 2005, to G.):

I first met Stacy in 1936. . . James Franck evidently heard of my observation [on oxygen uptake in high light in *Chlorella*] by grapevine and sent Stacy (his post doc) to see me in Minneapolis. Stacy and I sat outside the old botany building overlooking the Mississippi and talked about my experiments. . . Actually, Stacy was not very much interested in my experiments.

He wanted to follow up on Robin Hill's then recent observation of O_2 evolution by chloroplasts but Franck thought that chloroplasts were a waste of time. Stacy later got a faculty position (at Minnesota) and set about following chloroplast activity with dye reduction. Later, when Stacy had become the Director at the Carnegie laboratory, I spent several sessions as a visiting investigator. One session came at a time when Vannevar Bush, Head of the Carnegie Institution of Washington, housed in Washington (DC) headquarters, was goosing the laboratory to follow up on the idea that algae seemed to be more efficient than higher plants. Everyone at the laboratory had a special project on algae. One of them was Ed Davis; Ed was studying growth of a *Chlorella* Culture pumper through a long length of Tygon tubing under Sunlight. You may remember that Tygon tubing used to darken with age. This led Ed to order a new batch from a supply house. When it came we all admired it and Ed was set to install it the next day. Stacy then playfully substituted a batch of the old dark tubing just to see the look on Ed's face the next morning. . .

In 1959, I went to the Carnegie for a semester on a Guggenheim Fellowship to work with Stacy. He had spent some time with Francis Haxo learning the technology of the O_2 electrode. . . I had no family with me and, thus, I spent much of each night doing experiments (watching the recording of O_2 exchange). Stacy did all the "instrumental improvements" in light beams and shutters. It was the most productive year of my life. At heart Stacy was an inventor who loved to make optically based instruments from surplus bomb sights. He carried to extreme an analog analysis of spectra which he called a curve analyzer. He inked in and made opaque one side of a spectral curve; then, he devised a feedback system that made a photocell follow the curve. That allowed adding, subtracting or multiplying spectra. It was the basis of his intense study of absorption and fluorescence spectra in search of putative in vivo chlorophylls. The graph paper on which the curve follower worked was held in place by suction from a vacuum-cleaner motor. I remember that when I last admired the machine several parts were still held in place by C-clamps from its development days. Stacy did not go for fancy store-bought instruments and preferred to build his own. He was a great collaborator whom I remember with affection and admiration.

Yaroslav de Kouchkovsky, a 1963-1964 Carnegie research fellow, wrote (personal communication, e-mail message on May 2, 2005, to G):

Stacy French's policy was very open-minded: once he trusted people, he gave them all opportunities to enrich themselves through multiple contacts and encouraged them to follow their own research line, provided it was original. He let people conduct their investigations as they thought best, but his door was always open for on-the-spot discussions. I started thus my new research work with complete freedom and eager to do something new. The Plant Biology Laboratory of the Carnegie Institution of Washington, at Stanford, California, had an excellent reputation in the field and was a mecca for all "photosynthesizers." Many essential discoveries of that period originated from there and many original set-ups, that could not be found elsewhere, were built by Stacy, for example, the first derivative spectrophotometer. The French laboratory was a place of open and friendly exchanges, with morning coffee breaks that gave the opportunity to discuss the latest research advances or projects presented by a colleague. This maintained a good level of cooperation, and no publication could be submitted to a journal before first being edited by all the other members of the laboratory. Of course, this special atmosphere, that looks "old-fashioned" nowadays, was possible not only because it was then a small community, but at the first place, it was due to the rich personality of Stacy French.

Jan M. Anderson, a 1996 Carnegie research fellow, wrote (personal communication, email message on May 1, 2005, to G.):

I enjoyed my time with Stacy French immensely, the peaceful lab and the marvelously friendly group. However, I missed my beautiful Cary 14 spectrophotometer back in Canberra. Jan Amesz and I persuaded Stacy to request a Cary on loan. Despite good reports from all who tried it out, Stacy was not persuaded to buy the machine, as he said there was no one there who would really understand just how good it was. We greatly admired his string-and-sealing-wax approach for all the ingenious machines he built, but regretted the outcome of the Cary battle, which Stacy won. I greatly admired his writing, and on rereading his papers can hear his unique accent. I was only there for six months, the first-ever Aussie and thought it was heaven.

Tasso Melis, a 1979-1981 Carnegie research fellow, wrote (personal communication, e-mail message on May 2, 2005, to G.):

Stacy French had already retired when I arrived at the Carnegie in June 1979. He was at the Institute daily, albeit for a few hours. He became very interested to learn that I was building a "Duysens-type" split beam absorbance difference spectrophotometer . . . He encouraged me in this effort, in fact he gave me his high-resolution high-throughput Bausch and Lomb monochromator, which I successfully used in conjunction with this spectrophotometer during my entire stay at the Carnegie Institution. Stacy was fascinated (perhaps even awed) by the effect of the "silicon" revolution on scientific equipment. The advent of semiconductors had made a huge difference in the size of electronic scientific devices. Looking over my shoulder, as I was trying to calibrate components of the spectrophotometer, Stacy once marveled at how small the digital components of the lock-in amplifier were, compared to the electronic lamp amplifiers of the yesteryear. He also recognized the novelty of the absorbance difference spectrophotometry approach in the measurement of the photosystems, as he once quipped, "No one really ever measured the stoichiometry of the photosystems." Stacy French exemplified the ultimate research scholar, and I could see how, under his leadership, many of the great colleagues of his generation walked, worked, and contributed to the advancement of the field of photosynthesis research at the Carnegie Institution.

Arthur Grossman, one of the current staff members of the Department of Plant Biology of the Carnegie Institution, Stanford, California, wrote (personal communication, email message on May 2, 2005, to G.):

Stacy would come by every couple of months and find me and talk about what I was doing, although he might have been a little skeptical about my approaches, he always seemed amused, animated and curious about what I might (or might not) accomplish. I still remember the large motor that I think Frank Nicholson scavenged from a huge chart recorder that Stacy had built; the motor was used to power the spit for the traditional hog roast that was held at Carnegie every year.

Winslow Briggs, currently director emeritus, recalls:

Perhaps Stacy's most outstanding trait was his almost fanatic support of his staff members in protecting them from distractions (e. g., teaching, having graduate students) that would interfere with their research programs. A steady stream of both short- and long-term scientists marveled at the wonderful atmosphere for research at the Department. Another trait was his unwillingness to buy commercially produced equipment when he was certain he could build a better model himself for a fraction of the price. During his era, he was almost always right. For example, he was doing derivative spectrophotometry mechanically twenty years before computers made it routinely possible. His instrumentation provided one ground-breaking discovery after another. When I first arrived at Carnegie in 1973, and wanted to purchase a large piece of equipment before I officially became director, I almost had to stand over his shoulder to get him to sign the purchase order! Despite this inauspicious start, he and I remained very close friends after I became director.

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On behalf of the two of us (D.C.F. and G.) and several others shown in a partial list below, we remember you Stacy, for your scientific accomplishments, for your friendship, your free spirit, and for all the fond personal remembrances you have given us.

Amesz, Jan
Anderson, Jan M.
André, M.
Arnold, William A.
Berry, Joseph
Björkman, Olle
Björn, Lars-Olof
Bril, Cornelis
Brown, Jeanette S.
Chen, Shao-lin
Davis, Edwin A.
de Kouchkovsky, Yaroslav
Decker, John P.
Detchev, Giorgi
Duysens, Louis N. M.
Emerson, Robert
Fewson, Charles A.
Gasanov, Ralphreed A.
Gibbs, Martin
Goedheer, Joop C.
Goodwin, Richard H.
Govindjee, Rajni
Habermann, Helen M.
Hagar III, William G.
Halldal, Per
Haxo, Francis T.
Heber, Ulrich W.
Hill, Robert
Hiyama, Tetsuo
Holt, A. Stanley
Jacobi, Günter
Jorgensen, Erik G.
Kok, Bessel
Krauss, Robert W.
Kupke, Donald W.
Landolt, E.
Latimer, Paul H.
Lewis, Charleton M.
Lewis, H.
Loeffler, Josef E.
Loos, Eckhard E.
MacDowall, Fergus D. H.
Madsen, Axel
Mantai, Kenneth E.
McGinnis, William G.
McLeod, Guy C.
Menke, Wilhelm
Michel, Jean-Marie
Michel-Wolwertz, Marie-Rose
Milner, Harold W.
Milner, Max
Müller, Alexander
Murata, Norio
Murata, Teruyo
Myers, Jack E.
Ninnemann, Helga
Pearcy, R. W.
Pickett, James M.
Raymond, Lawrence P.
Ried, August
Sager, Ruth
Schiff, Jerome A.
Schreiber, Ulrich
Schulman, Marvin D.
Šesták, Zdenek
Shibata, Kazuo
Smith, James H. C.
Soeder, Carl J.
Stanier, Roger W.
Strain, Harold H.
Strehler, Bernard
Takamiya, Atusi
Tamiya, Hiroshi
Troughton, J. H.
Urbach, Wolfgang
Van Niel, Cornelis B.
Vidaver, William E.
Virgin, Hemming I.
Vishniac, Wolf
Von Wettstein, Diter
Weaver, Ellen C.
Wiessner, Wolfgang
Wolf, Frederick T.
Wraight, Colin A.
Young, Violet M. (Koski)

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