ADVENTURES IN SERENDIPITY

Arthur Galston

I was born in a place called Brooklyn, New York, in the shadow of the Brooklyn Bridge. My parents were both immigrants, having come from the part of the old czarist Russian Empire that lay between Poland and the Ukraine. They got out for the favorite reason in those days—violent anti-Semitism and pogroms. My father came over in 1893 as a boy of twelve. He had six years of elementary school education and then earned enough credits at night to matriculate in the Cooper Union. But he was never able to do so because of the necessity to earn a living. My mother came over as a young woman of fifteen, having left her folks back in the old country, hoping to see them again soon. The practice was to send one family member over to serve as a sort of anchor for others. Then the young children were sent over, followed by the older people, until the whole family emigrated. Unfortunately, my mother never saw her parents again, which made her very sad.

Although my mother was a very bright woman, she never had any formal education in America. She spoke a sort of Polish-Russian mixture, Yiddish, and a little English, with a marked accent. When I was in the third grade, there was a move to get immigrant parents "up to snuff" as far as the English language was concerned. So I said to my mother, "You have to take this training." She said, "Oh, what do I want it for?" And I cajoled her by saying that when I went away to college, as I firmly intended to do, she and I could correspond. She accepted that reasoning and became a crackerjack English student. All during my college years, she would write letters on penny postcards and I would correct the grammatical errors, which she then never made again. She was a wonderful student; I just wish she could have gotten a proper education.

The late **Arthur W. Galston** was the Eaton Professor Emeritus of Botany in Yale's Department of Molecular, Cellular, and Developmental Biology and professor emeritus in the School of Forestry and Environmental Studies. He spent much of his career studying the processes of higher plant development, particularly the role of light. A gifted saxophone player, he worked his way through Cornell University's State College of Agriculture in the early 1940s by performing in swing bands in upstate New York. After earning his M.S. and Ph.D. at the University of Illinois in 1943, he taught at the California Institute of Technology and later served in the U.S. Navy. He joined the Yale faculty in 1955 and continued teaching long after his mandatory retirement in 1990. In addition to developing a popular introductory bioethics course for undergraduates, he remained active in Yale's Institute for Social and Policy Studies, where he helped to lead the Interdisciplinary Bioethics Project. Ironically, his work on plants provided the scientific basis for a controversial defoliant used by the U.S. military in the Vietnam War. He campaigned against the use of Agent Orange and visited Vietnam repeatedly to assess its impact. In 1971 he was the first American scientist invited to visit China since the Communist revolution. He published hundreds of articles, several widely used textbooks on plant physiology, and two anthologies on bioethics, as well as *Daily Life in People's China*, based on his travels. He died on June 15, 2008.

Both my parents worked at the so-called needle trades, making garments for men and women. My father was a member of several unions, beginning with the International Ladies Garment Workers Union, headed by David Dubinsky. Later he joined the Amalgamated Clothing Workers of America under Sidney Hillman, a famous political figure in those days. That accounts for my lifelong pro-union orientation. I realized at first hand what union membership meant for a working man, especially during the Depression, when jobs were scarce, wages were poor, and there was no social safety net. Our family was strung out pretty close to the economic bottom. Had it not been for union membership, we probably would have moved to the poor house.

I had two older sisters who were very bright, but for various reasons they never got to go to university. My oldest sister was the recipient of a scholarship to the University of Pennsylvania and was preparing to go when my father suddenly lost his job. She decided not to accept the scholarship and instead went to work to help support the family. My other sister, six years older than I, did the same. My mother hadn't planned to have more than two children and was going to do something about terminating me before I saw daylight. However, the family doctor, a Dr. Lerner, dissuaded her when he convinced her that it might, after all, be a boy. That is what saved me.

What does a young boy growing up in such an environment do? In families of the sort I came from, there were only two possibilities. First of all, you had to be a professional. Your parents didn't want you to go through the same kind of life that they went through, so you had to be a doctor or a lawyer. As I had read several books by Paul De Kruif that turned me onto the idea of becoming a savior of mankind through medicine, that became my stated ambition. Perhaps wrongly, however, I became convinced that this goal was impossible for me to attain since there was no money to facilitate my passage through the tortuous years of college and medical school.

At that point, a friend informed me that there was a veterinary school up at Cornell that taught medicine of a different sort. Since tuition was free to residents of New York State, the price was right. So I applied to Cornell, where I had to enroll in the School of Agriculture for one preveterinary year. During that year I took physics, chemistry, English, and biology, as well as a fifth course intended to show my devotion to veterinary medicine in order to compete for entry into this elite school. The first semester I took a course called "Types and Market Classes of Livestock;" the second semester I studied "Feeds and Feeding." Imagine this young boy from Brooklyn studying types and market classes of livestock! The laboratories were held in the judging ring. They would bring out, for example, Duroc Jersey hogs of various qualities and sizes. We students would have to analyze and rank them, saying something like: "This one has good hocks and this one is well-fleshed here. This one shows great vigor and this one is not so good." In this way you'd rate the animals, and then in turn you got graded on your rating. I turned out to be pretty good at it. At the end of the semester, I was invited to join the Cornell Judging Team, an honor that I turned down. So I knew a lot about cattle, hogs, chickens, and ducks, and could also devise a good menu for farm animals.

Cornell is divided into two kinds of schools. There are the endowed institutions, like the School of Liberal Arts and Sciences, where you pay tuition and take whatever courses you want. And then there are the state schools, like agriculture, veterinary medicine, and home economics, where you pay no tuition but can only take prescribed courses, with perhaps room for a few others. As a result, I could not enjoy most humanities courses; for example, I never had a history or literature course at Cornell. I managed to take one philosophy course and one English course, and that was it. I have always felt deprived of a proper education for that reason.

After spending a year in agriculture, I was offered admission to Cornell's veterinary school. When I visited it, however, I became immediately disenchanted. Although it is now a formidable and worthwhile scientific institution, at that time it was caught between the old horse doctors and modern science. I could not see myself spending my life in that atmosphere. What to do? I had become entranced with the professor of botany, a gentleman named Loren Petry. He was eloquent in his lectures, was interested in real science, and had a wonderfully global outlook. Every Saturday night in the student union, he used to host what we called "Petry's bull sessions." You could raise any question for general discussion, over which he would preside while puffing on his pipe, saying very little, but patiently admonishing us when we strayed. I decided to emulate him and switched to a botany major, specializing in plant physiology and biochemistry.

After earning my bachelor's degree from Cornell in 1940, I received an offer from the University of Illinois to continue my graduate work as a teaching assistant. I had never been as far west as Champaign-Urbana. I will never forget riding on a Greyhound bus through the hills of Indiana, where I first saw tall corn, and then arriving in Illinois, flat as a pancake, where the corn was even taller. "What a world I'm going to be inhabiting," I thought to myself. I entered the University of Illinois that September and had completed three semesters of graduate work by the time Pearl Harbor was attacked in December 1941. The university came out with an edict that said, in effect, "If you've finished three semesters, this being wartime, we will consider that you're half-way through your graduate work, and if you work hard you will get your Ph.D. in three years." So that's what I did. At the end of that time I thought I was going directly into military service, but, as it turned out, a friendly faculty member changed my fate. I'll tell you about that later.

My faculty adviser was a wonderful man named Harry Fuller. Although not a great experimentalist, he was very cultured, very knowledgeable, and a good scientist. He turned me onto a phenomenon I had never heard of. It's called photoperiodism, which refers to the role of day length in plant development. As plants go through their lives making roots, stems, and leaves, they suddenly, at some magical moment, make flowers. What causes the transition from vegetation to reproduction? It's an important developmental change, and for most plants it is determined by precise measurement of the length of day. The so-called long-day plants, such as spinach, need at least a certain critical number of hours of light in a twenty-four-hour day to trigger the flowering process. Conversely, so-called short-day plants flower only if they receive less than a certain number of hours of light per day. An example of the latter is Maryland Mammoth tobacco, with which photoperiodism was discovered. Finally, there is a third group of plants that seems not to care about day length.

I was intrigued by the phenomenon of a plant measuring time. It turns out that these measurements are precise down to a few minutes. For example, with some short-day plants eight hours and twenty-eight minutes of light per day will permit flowering, but if you go to eight hours and thirty minutes, the short-day plant will not flower. What precision! It had to be the result of a biochemical mechanism of exquisite sensitivity, probably involving hormones. The thesis subject suggested by my professor, Harry Fuller, was to investigate the physiology and biochemistry behind photoperiodism (which, by the way, turns out to be very important economically). As I was about to start on my experiments, Fuller handed me a package of seeds and said, "I think you should work on this plant." They were seeds of a plant of which not a single commercial acre existed in Illinois in 1940: it was called soybean, and it is now our major agricultural export. Soybeans had been brought over from China, but all of the available varieties were ill suited for the United States. Our growing season in Illinois was not long enough for them. They would start to flower and set seed late in the summer, but then frost would come before the pods matured. My job was to try to accelerate the transition from vegetation to reproduction so that the seeds could be filled out and harvested out before the frost came.

My graduate student colleagues called my project "the sex life of the soybean." I did a biochemical analysis of the problem and decided that I had to somehow thwart the growth-promoting effects of auxin, then the only known plant hormone. Searching for a molecule that would antagonize the effect of auxin, I came across a very simple compound called 2,3,5-triiodobenzoic acid, or TIBA. I then did the following simple experiment: I poised soybean plants photoperiodically on the cusp of vegetation versus reproduction to make them optimally sensitive. Then I sprayed a control sample with water and another sample with varying concentrations of TIBA. I found that at certain concentrations, TIBA would induce vigorous flowering in otherwise vegetative plants. In theory, this advance could permit a successful harvest to be obtained in Illinois. So I succeeded in my project; I got my Ph.D. and was happy with the results.

My discovery had various unforeseen consequences, of which I'll mention only one. As you go up the concentration range with TIBA, you get to a point at which deleterious effects start to exceed the beneficial effects. Most compounds show a so-called dose response curve; that is, there is an increasing beneficial effect with increasing concentration of the compound, after which the beneficial effect reaches a peak and then declines. The deleterious effect I had noted was the shedding of leaves. Inadvertently, I had discovered a wonderfully effective defoliant. I noted this small fact in the appendix to my thesis, giving it no significance at all, but it came back to haunt me, as I will detail later. Another consequence of Harry Fuller's turning me onto this project is that I determined to study the wonderfully fascinating interaction of light and plants for the rest of my experimental life.

In the course of trying to find out things about photoperiodism, which is red-light sensitive (a pigment called phytochrome mediates the perception of photoperiod), I stumbled onto a molecular explanation of a blue-light-sensitive process called phototropism. Everyone knows that if you put a plant in a dark room and place an ordinary light bulb in one corner, the plant will bend toward the light. That's not a red-light reaction. If you turn on a red light in a photographic dark room, the plant won't bend toward it. It will, however, bend toward a blue light. Later on, when I was at Caltech, I made a discovery leading me to propose that a class of compounds called flavoproteins, containing vitamin B2 (riboflavin) attached to a protein, acted as photoreceptive pigments—that is, pigments that received the light. Light cannot be effective unless it is captured by a pigment molecule, which then in turn becomes activated. This activation energy makes possible a stimulation of a subsequent chemical reaction.

My suggestion about flavoprotein, which I advanced in 1949 and 1950, ran exactly counter to the theories of many "pundits" of the day, who were impressed with what they called the logical power of comparative biochemistry. It's well known that we humans see thanks to a group of pigments that belong to the carotenoid class of compounds (related to the orange pigment of carrots). Since the carotenoids absorb light in humans in vision and the so-called action spectrum for phototropism in plants that relates action to wave length of light was fairly similar to light absorption by the carotenoids, the established orthodoxy was that carotenoids had to function visually in plant phototropism as they did in our bodies. I didn't believe it. So much experimental evidence stood against that conclusion, and I was just "ornery" enough to trumpet it. I was invited to write a few reviews in influential publications like *Science,* but my view was roundly condemned by several important scientists. George Wald was a recipient of a Nobel Prize for his investigations of carotenoid visual pigments. He and another Harvard professor, Kenneth Thimann, pushed the view that they functioned similarly in phototropism in plants. But it turns out that I was right and they were wrong. A half-century later, Winslow Briggs, an old friend of mine from Harvard and Stanford, unambiguously showed through molecular genetic analysis that the visual pigment for phototropism is indeed flavoprotein. We now know of a whole class of photo-activatable flavoproteins. I consider the photo-biochemistry of flavoproteins to be my major scientific contribution. I didn't have the satisfaction of doing the final molecular genetic work that Briggs and his colleagues did at Stanford, because I had never been trained in that technique and probably couldn't have done it anyhow. Nonetheless, it was very satisfying to have made that contribution.

As I mentioned earlier, I was expecting to go into military service after receiving my Ph.D. in 1943. I had tried to enlist once, to get a commission, but was told I had a calcified lesion in my lower left lung and was thus unfit for military service. A few months later, when they were scraping the bottom of the personnel barrel, they found that although I wasn't fit to be an officer, I was fit to be an enlisted man. So I entered the Navy, but under special conditions. In those days there was an examination called the Eddy test, named after Admiral Eddy. I learned that if you knew how to solve a quadratic equation, and you knew Ohm's Law, you could pass the Eddy test. So I took it, passed, and was inducted as a seaman first class, two ranks above apprentice seaman. I was sent to Great Lakes Naval Training Center in Illinois and studied for a while to be a radar technician. After several weeks, a personnel officer read my qualifications jacket, noticed that I had a Ph.D., and asked, "What are you doing as an enlisted man?" They pulled me out and sent me to Columbia University as a midshipman training to become an ensign. After my promotion, I was trained to become a radar officer on an aircraft carrier at a wonderful place called St. Simon's Island in Georgia, where I lived in the King and Prince Hotel, one of the premier vacation spots in those days.

After finishing the course, I was assigned to a light aircraft carrier in Oakland, California, and was on my way to a train station to go there when I was intercepted by a courier who announced that my orders had been changed. This was typical of the Navy: they would train you to do something and as soon as you felt qualified to do it, they changed your duty and trained you for something else. Somebody had noticed that I had an aptitude for languages. Having learned French, German, and Russian, I was considered a good candidate for Japanese language school. Unfortunately, I didn't get to go to Monterey or any of the other garden spots in California to which many of my colleagues were assigned. Instead, I was sent to Oklahoma State University in Stillwater. Our teachers were all Japanese-Americans who had been "relocated" from concentration camps in California, where the United States government had interned them following Pearl Harbor. They were wonderfully kind people, despite the indignities that had been heaped on them. One of them, named Naganuma, had written our textbook. When we graduated from Japanese language school, the commanding officer said, "Gentlemen, I congratulate you. You are now well qualified to speak and read Japanese with anyone who has used the Naganuma reader." Midway through the course (which should have continued for another six months or so), the atom bomb was dropped and the war ended. We were told that if we wanted to continue with the Japanese language, we'd have to sign up for three additional years. I was having none of that, so I shipped out and was reassigned to a military government team on Okinawa, where my language skills were somewhat useful. Nevertheless, I still had to avail myself of a Nisei interpreter from Hawaii, who translated for me as I went from village to village trying to do reconstruction in agriculture and fishing.

At the end of the war, vast opportunities suddenly arose for people with my training. The United Nations Relief and Rehabilitation Administration was opening up vast enterprises in Asia and offered me and others some very cushy deals. I was tempted to foreswear academic life and become an UNRRA official when, to my surprise, word came from the Guggenheim Foundation that I had been awarded one of the special wartime Guggenheims to help service people readjust to academic life. I decided that was too good to pass up, so I turned UNRRA down and planned to leave service. But where to start? Let's go back to Illinois to find out.

At the University of Illinois, I had taken a biochemistry seminar with a wonderful young professor named Herbert Carter. It consisted of reading the literature and delivering reports to the seminar, trying to make the papers interesting and meaningful to the other graduate students. Everybody else was an animal biochemist; they talked about the biochemistry of blood, urine, and liver, and such things. Then I would come in and talk about photosynthesis, nitrogen fixation, and other equally wonderful processes from a totally different plant world. For example, I reported on some very exciting experiments at the University of California at Berkeley. Samuel Ruben, Martin Kamen, and Zev Hassid had just used carbon 14 for the first time to trace the chemical course of CO_2 fixation. Although Herb Carter was unfamiliar with that world, he was delighted to learn about it.

When I went to say goodbye to Carter, I thought I was leaving for my induction into the military. Instead, as mentioned earlier, I was declared unfit for service. In any case, Carter had other plans for me. He said that as I now had a Ph.D. in science, I should not have to serve as a fighting man. I asked what else I could do. He replied that I should be a scientist working for the government. I had no idea where to go or whom to contact, but Carter knew some of the people whose papers I had reported on in his seminar. He made a few calls, one of which resulted in an offer from the California Institute of Technology, where a man named James Bonner had a grant for work on an emergency rubber project. The Japanese had conquered the Malaysian peninsula, which contained 90 percent of the world's rubber supply. In response, the United States activated several research projects. For example, Professor Fuller was asked to go down to South America to look for rubber trees. But the government also decided to look for alternative sources for rubber. They started two projects, one on natural rubber, the other on synthetic. We biologists fastened on a little dandelion-type plant called guayule, from the Sonoran desert, which produced harvestable rubber after growing for about a year and a half in the desert. We then harvested the whole plant, ground it up, and threw it into a tub of boiling water. The so-called bagasse became wet and sank to the bottom, while the rubber granules floated up and were skimmed off. They had to be purified because they were somewhat "tacky" at that stage, but we developed fermentation techniques to remove the tackiness. Within a comparatively short time, we had excellent truck tires to present to Jesse Jones, the head of the Reconstruction Finance Corporation, which had financed the research. At about the same time, however, the chemists had learned the secrets of rubber synthesis from butadiene. As a result, the government went with the synthetic program and ended the guayule program.

It was at that point that I was inducted into the Navy. When I was discharged in early 1946, after about two and a half years of service, I headed for New York, where my wife was staying with her family and our infant son, conceived while I was in training at Columbia. I was all set up to return to Caltech, which wanted me back. But since all four grandparents lived in New York, I was under tremendous pressure to find a job in the East. When I visited Yale, Edmund Sinnott, then dean of the Graduate School, offered me a job as instructor in the Botany Department. I didn't know what a lowly job that was, so I accepted it. After a year, having been overworked and underpaid, I quit and returned to Caltech. As an instructor I did not like Yale at all. Nine years later, after I had prospered at Caltech, Yale wanted me back. This time I was hired as a full professor, at a respectable salary and under better working conditions.

To return to the defoliation story, I had inadvertently discovered how to cause leaves to fall off of the soybean plant. Sometime after I returned to Caltech in the fall of 1947, I was visited by two gentlemen from a place called Fort Detrick in Frederick, Maryland, where the U.S. Army chemical warfare service had started up a research program on defoliants. During World War II, we had employed landing craft to hit the beaches, where they disgorged soldiers. Frequently, our men rushing onto the beaches would be mowed down by withering machine gun fire from enemy hiding in the dense shrubbery. Somehow we had to get rid of that shrubbery. The military leaders tried bombs, but they didn't work. You can knock a tree over with a bomb, but you can't get the leaves off that way. Finally, the military discovered that there were people known as plant physiologists who had some tricks to accomplish defoliation. It was then that they came across my information and decided to start a project with TIBA, which became one model compound for defoliation experiments. After many trials and synthesis of new compounds, with much improvement over my first model, two compounds emerged as most effective. One is called 2,4-D (2,4-dichlorophenoxyacetic acid), the other 2,4,5-T (2,4,5-trichlorophenoxyacetic acid). Both consist of an aromatic nucleus to which is attached a side chain. The synthesis is a simple matter of bringing the side chain together with the nucleus.

In 1965, I read in the *New York Times* that American forces were using 2,4-D and 2,4,5-T in a mixture known as Agent Orange to defoliate the upland jungles of Vietnam. In so doing, they hoped to thwart the transport of men and materiel from the Hanoi region down to the Saigon region over a path called the Ho Chi Minh Trail. It was remarkable how relatively small men could push altered bicycles loaded with a ton and a half of materiel through the jungles for more than a thousand miles. It was this flow that we wished to interdict. American planes flew wing tip to wing tip over the trail, their tanks loaded with Agent Orange. A shower of defoliant descended over a wide area, and about forty-eight hours later leaves fell off most of the trees. Some trees recover and put out a second flush of leaves. A second spraying is again effective in defoliation. About three times is all a tree can stand before it dies. Many of the killed trees were important economically. Teak, for example, is much favored for furniture, and the mangrove that lines the estuaries near Saigon protects the coast from erosion and facilitates the growth of sea food. Entire communities of these trees were killed during the Vietnam War and have never recovered. These losses have had serious economic and ecological effects that have yet to be corrected. Nor has the United States ever contributed so much as a dime toward helping to alleviate the damage.

When I read the report in the *Times*, I became alarmed because I realized that these compounds were hitting not only plants but also people, and I knew that they had not been toxicologically tested. So a group of us from several colleges formed an informal committee to press for proper toxicological tests. We made such pests of ourselves that the Defense Department commissioned tests from a commercial laboratory called Bionetics, a branch of Litton Industries. There were three types of tests: for mutagenicity (does it cause mutations?), for carcinogenicity (does it cause cancer?), and for teratogenicity (does it interfere with normal development of embryos in utero?). The results came out quickly for the first two tests, showing no adverse effect, but the teratogenicity results were inexplicably delayed. We pressed repeatedly for their release, without success. Finally, my colleague Matthew Meselson at Harvard contacted one of his insider friends who worked as a "Nader's Raider." He gained access to a classified file and, through the magic of Xerox, his one removed copy became many copies, and soon the news was out.

The test showed that 2,4,5-T, one of the two compounds in Agent Orange, was a highly teratogenic substance that caused malformations of fetal development in mice and rats, even at very low concentrations. The toxicity was actually due to an impurity called dioxin. We calculated that with the quantities that had been dropped on Vietnam, if a woman drank water from a shallow well, she could get a teratogenic dose, assuming that the toxic dose for humans was proportionately the same as for rodents (which, of course, we didn't know). This was big news. Now we had a smoking gun; what to do next? "You can't fight city hall," the old saying goes. But we could and did—and won. Here's how.

In 1970, President Richard Nixon had as a science advisor a distinguished physicist named Lee DuBridge, previously president of Caltech. I had known him well during my stay on the faculty there. So did Matt Meselson, who had been a graduate student of Linus Pauling's. We decided to call DuBridge and pass the information to him. Lee was a wonderful man. He was willing to listen to us and promptly convened a meeting with military defoliation officials in the Old Executive Office Building next to the White House. After we presented our data, DuBridge decided to notify the president. Nixon, to his credit, immediately called Secretary of Defense Melvin Laird and ordered him to stop spraying Agent Orange. Although the war did not end until 1975, we spared the people of Vietnam five years of continued spraying. Nothing that I will ever do in my life is apt to have such good consequences as that.

As a result of this action, I received an invitation to visit Vietnam, where I met the prime minister of North Vietnam, Pham Van Dong, in 1971. That was the year the Chinese invited the American ping-pong team to visit. Reasoning that this might be an indication of a Chinese opening to the West, my colleague Ethan Signer of MIT and I decided to file applications for Chinese visas before we left North Vietnam. Originally, ours was supposed to be a ten-person delegation sponsored by the Scientists' Institute for Public Information. However, because of the difficulties involved in traveling to that part of the world, together with the polarized American attitude about the Vietnam War, our party of ten quickly shrank to two: Ethan Signer of MIT and myself. As we prepared for the trip, we became aware that getting a valid U.S. passport for travel to Vietnam was virtually impossible. All U.S. passports were clearly marked as invalid for travel to Cuba and North Vietnam, and people who had traveled to those countries had had their passports confiscated upon their return to the United States. A State Department official named Ruth Shipley, in charge of issuing passports, simply denied all further travel to such people. But thanks to a Supreme Court verdict in the case of a Yale assistant professor named Staughton Lynd, who traveled abroad after his passport had been confiscated, we were provided with an opening. The court ruled that the right to travel could not be denied a U.S. citizen, even without a passport. So we traveled to Vietnam in the midst of the war without the protection of valid U.S. passports. We were on our own, for better or worse, and if we got into any kind of trouble, we could not call on our country's diplomats to aid us.

To our surprise, our visa applications for travel to China were accepted and we became the first American scientists invited to the People's Republic of China. On that historic trip, we were treated as visiting dignitaries and even got to meet Premier Chou En-lai. I have a picture of myself in the Great Hall of the People in Beijing, shaking hands with him. After an interview lasting two and a half hours, I told the premier in parting that I wished I could come back with my wife and family, because my wife was really the "China expert" in our family. He agreed and in 1972 we did return to China as a family. I had also said that we wanted our visit to be an in-depth experience, not just a tourist trip. When the premier asked what we wanted to do, I replied that I'd like to be a member of an agricultural commune, since agriculture is the backbone of China and 80 percent of China's billions then lived on farms. He arranged for us to be part of a work brigade at the Lu Gou Qiao Renming Gungsha, or Marco Polo Bridge People's Commune. We had a most remarkable experience in the summer of 1972, which I wrote up in a book published the following year. It was an exciting tale and was a bestseller for a while, until it was eclipsed by a later book written by Shirley MacLaine.

This is obviously a tale written by serendipity. One does an experiment to elucidate "the sex life of the soybean" and winds up with a chemical that knocks leaves off trees. That turns into a fight with the United States government. (Ethan Signer and I testified often before Congress and lectured widely.) We went to Vietnam, then to China, upsetting our normal life. For a scientist, this virtually full-time diversion was serious. For about three years, I really couldn't do my scientific work well. Fortunately, the outside activity eventually tapered down.

The last part of my story I'll make brief. As result of these experiences with the social and ethical consequences of scientific research, I began in my own teaching to try to emphasize those aspects. In 1977, I set up the first bioethics course ever taught at Yale in the former Department of Biology. Shelley Geballe, who was then a law student, was my teaching assistant and helped me frame some of the important questions in the course, called "Problems in Bioethics." We took serious problems and analyzed them first from a scientific point of view. My thesis was that you had to get the science right before you started to do an ethical analysis. Of course, we weren't experts in ethical theory or analysis, but we indulged in some group study and got a little better at it. Finally, after my mandatory retirement at age seventy, I started to teach college seminars in bioethics and have done so continuously for sixteen years. Timothy Dwight College was always one of the sponsors, together with several of the other colleges. I loved college seminars, which are limited to eighteen students, but there was also a frustration attached to them, because each year I would have between sixty and seventy-five applicants. How do you choose your class? You had to save six places for the sponsoring college and six for the cosponsor, leaving six places open for the rest of Yale. I thus suggested to the Institution for Social and Policy Studies (ISPS) that we float a course open to all students in Yale College under the sponsorship of their Bioethics Project, of which I was a member. We did so in the year 2002, just to see if anyone would come. To our amazement, 350 students showed up. The university had to give us the Law School auditorium for a classroom. The next year, 464 people showed up. This was one of the most interesting educational experiences I had ever had at Yale. I gave only four or five of the twenty-six lectures. For the rest I acted as a sort of impresario, inviting my friends from Medicine, Forestry, Political Science, and Divinity to come and speak to their specialty. Although some of my colleagues said that this type of course could not work, we showed that, with proper coordination, it could. Besides, the course was vastly popular.

I was also in charge of a monthly graduate seminar lecture series at the ISPS. One volume of these collected lectures came out in 2001, and a second in 2004, published by Springer Verlag in Germany. The recent volume has three sections: "Science and Society," "Medical Ethics," and "Environmental Ethics." Partly through our efforts, bioethics at Yale has become more than medical ethics, and environmental ethics is now an important aspect of instruction in bioethics.