I knew I would attend college at Wesleyan University, in Middletown, Connecticut, years before I applied. My esteemed Uncle Henry, the minister, had gone there. More important, my oldest brother Charles had attended from 1929 to 1933, when I was beginning high school. Curiously, I never considered other colleges. Although I was aware of places like Yale, Harvard and Columbia, they were dimly off on the edge of my teenage horizon. My vague impression of Yale, where later I was to spend over 50 years of my life, was as a rich man’s school obviously beyond my reach. I say curiously because my Uncle George had somehow managed to go to Yale College and then, after, to the Yale Music School, so I am unclear why I thought Yale was beyond me. At any rate, I applied to Wesleyan, and nowhere else, and was accepted. Although Wesleyan called itself a university it was not a real university. It was simply an excellent small college, with but 700 male students, one of a number of such fine institutions scattered around the U.S.A. When in later years it admitted graduate students and bestowed the Ph.D. degree, it became a real university, but certainly not of first rank. It remains an excellent undergraduate college but much bigger. Its very small size when I was there was a significantly favorable feature of the place.

I had pored over photographs of the Wesleyan campus, paying special attention to the Shanklin Laboratory of Biology and the Olin Memorial Library. They whetted my desire and gave me something tangible to look forward to. But pleased though I was in anticipation, the real thing, on my arrival in September, 1936, hit me as an agreeable surprise. Like so many American colleges set in small towns, Wesleyan has a very charming campus. Indeed, I was told that High Street, along which many of the college buildings are distributed, was considered by Charles Dickens to be “the most beautiful street in America.” Years later I was told the same thing about Hillhouse Avenue on the Yale Campus. Lord knows how many other streets on how many other campuses were reputed to be so admired by Dickens. Never mind. I was 18 years old and happy to believe what I was told. I fell in love with the street and the campus immediately. I liked everything about the place. What a change from South Side High School and Rockville Centre!
Freshman Year

Things started nicely. The Introductory Biology Course was a natural for me. Also, the staff knew about me and paid me some special attention. In my application, I revealed my ambition to become a biologist. Somebody like me was certainly refreshing for the biology faculty, beset as they were with premedical students. At the first meeting of the required course in English History, as the professors checked our names, one of them ran across the name Trinkaus. He asked me if I was related to Charles Trinkaus, and then immediately he called the others over to meet me. My brother had majored in history and had been a star student. I was delighted with the unforeseen attention but a little embarrassed. I made a mental note to do well in that course. Fortunately, I did. It was an excellent course. Things did not start so nicely in some of my other courses, however. I failed my first exam in math and received a D on my first English theme. This was not a heroic entry into the academic world. Clearly, I had been poorly prepared and I was scared. In consequence, I had to study hard to make up for the deficiencies of my typically poor American primary and secondary school education. That is the main story of my freshman year (1936-1937). Study, study, study. Every day, including Saturdays and Sundays. I remember particularly those grim weekends in the fall when I would only take time out to watch football games Saturday afternoon. The winter was worse; there wasn’t even football. Well, I got through it all and was better for it, my shaken self-confidence restored. I learned something important, that if I worked hard I could do well in a number of subjects, even in a tough college. As I look back on it, the main deficiency of my high school was that they didn’t work me hard enough, not enough homework.

I was lodged in a stolid brownstone dormitory called North College, where I shared two rooms with another freshman. He was an agreeable young man and we got along well enough; but, gradually, as the year moved on, I found that this and that little peculiarities of his ways began to annoy me. I didn’t like the way he brushed his teeth. He probably felt the same about me. Nothing came to a break or bust, but I grew convinced that henceforth I should room by myself. And that is the way it was for the rest of my college career. A single room, always the same, in good old North College. Again, as at home in Rockville Centre, I was in my own private castle. I ate my meals in a small boarding house, just a few doors from the College, run by a hospitable Middletown lady and also in a dining hall called Downey House, where I also waited on tables to have some extra spending money.

In addition to my room, I had another home, the Shanklin Laboratory. I quickly found a job there, making charts for classes in biology. I worked principally with Professor H.B. Goodrich, who was soon to become my mentor, but I worked with other faculty as well. I was paid by the National Youth
Administration (NYA) of the Federal Government, one of the many superb projects of the New Deal under Franklin Roosevelt during the Great Depression. I was able to design and execute the charts because of a certain minimal artistic talent developed in an art class I took in high school. But I lasted only one semester in that course. I was the only boy in a class composed otherwise completely of girls and I couldn’t take it any longer. Talk of a gender gap! Recently (1999), students in the Embryology Course in Woods Hole showed me one of the charts I made in 1936. They were using it as a design for the class T-shirt!

Sophomore and Junior Years

After my freshman year, science was so much in the forefront of my perspective that I completed my major in biology by the end of my junior year, taking inorganic and organic chemistry, comparative vertebrate anatomy, embryology, histology, genetics, physiology, bacteriology and invertebrate zoology. These were standard courses for any biology major in those days. I liked them all and worked hard at them, although it wasn’t that hard for me, more like a labor of love. The laboratory in bacteriology was especially instructive. Because of their rapid reproduction, experiments with bacteria gave results quickly and gave good lessons on the importance of controls. Organic chemistry, in spite of its inherent elegance was a bit of a burden. Professor Albert Hill was an excellent lecturer but somewhat of a martinet, complete with his impeccable dress and large, carefully trimmed waxed moustache. He insisted on lecturing at 8 in the morning, Tuesday, Thursday and Saturday. We were convinced that he reveled in this. Getting up for an 8 o’clock lecture in the New England January and February tested the fiber in one’s young soul. Some of us would arrive in our pajamas (under an overcoat). He didn’t care. We were there. The laboratory part of the course, like all organic chemistry labs, however, was a gas with lots of flames and explosions, chemical and verbal.

In later years, I learned that my physiology course with a laboratory was not standard in American colleges. At Wesleyan, it was a full year course in mammalian physiology (including human physiology), taught by the eminent Professor Edward C. Schneider, an expert in aviation and high altitude physiology and the author of The Physiology of Muscular Activity (1933). The text for the course was Howell’s Textbook of Physiology (13th edition), used frequently in medical schools at that time. It was a very tough course. Dr. Schneider was a very mild, sympathetic and fatherly gentleman, often referred to as “Dear Dr. Schneider,” but as a teacher he was very demanding. Difficult though it was, the course was absolutely fascinating, probably largely because we were really forced to dig deeply to study our own physiology. To this day, almost all the understanding I have of my very own physiological functioning
comes from this course. The mammalian physiology I was introduced to was already at that time in many ways a marvelously advanced discipline. I wonder why such a course is not a basic part of the biology curriculum in every college? What better purpose could a course in biology serve than to give students some understanding of the functioning of their own bodies, the organism they inhabit? I suppose one of the reasons is the tendency of the faculty of undergraduate departments of biology to cater to the biggest customer—the premedical student—on the premise that it would be redundant for them; those who enter medical school will be fully exposed to physiology there. But what about students like me and, say, chemistry and physics majors and even non-science majors, who have sufficient background in science?

Since most students majoring in biology do so because of their desire to go to medical school and become physicians, I found myself surrounded daily by premedical students. Inevitably, some would ask me why I wasn’t interested in going to medical school. The simple truth was that I had never even given it a thought. All of my dreams of a profession were as a future biologist. Such queries, however, forced me to consider the question. I quickly realized that no way could I be a physician. I hate visiting hospitals and usually find being around sick people depressing. Anyway, I loved biology. There was a moment when I hesitated momentarily after my two excellent courses in history and thought that I might enjoy being an historian. But that was just a passing thought. Deep down I most aspired to becoming a scholar, with a penchant for biology.

Recently, in my old age, I discovered another reason why I would have been unhappy as a physician, at least in a number of medical specialties. I contracted a severe case of the shingles, herpes zoster, in my trigeminal system several years ago. When I checked into the Pain Center at the Yale-New Haven Hospital, I found, after several unhappy visits, that nothing tried on me (including morphine) relieved the excruciating pain. I asked the physician-in-charge why and also consulted literature on the subject (in a journal with the no-nonsense title Pain). The answer is that pain due to herpes zoster and lots of other maladies is not at all or only poorly understood at a basic level, as is also true of a number of other areas of modern medicine, notably cancer. I said to myself, “My God. This makes my poorly understood discipline of embryology seem like physics!” There is still no remedy for shingles. I’ve learned to live with it. Practicing medicine in many areas must be very frustrating intellectually to the physicians; they are impelled to try something, even though they do not understand at a basic level what is going on. I was often told, “Let’s try this, sometimes it works, for a while.” I like doing experiments on fish embryos, but would not feel comfortable with this “hit or miss” approach to treating people, another reason for me to stay away from medicine. Fortunately, however, for me and for everybody who can afford it or has good insurance, some outstanding
individuals (including many of my best students) are better able to deal with a
degree of uncertainty and are attracted to medicine.

There are fundamental differences between the education and daily
activities of typical Ph.D.s and M.D.s. Ph.D.s are trained to know a lot about a
little. M.D.s are trained to know something about a lot of things. Ph.D.s often
have the luxury of time in making decisions and, if an experiment fails, they can
try the experiment again. M.D.s are sometimes under enormous pressure to make
quick decisions that can either save or end human life. For many years in
academia, there has been a certain tension between Ph.D.s and M.D.s. There are
disagreements about who is the "real doctor." Faculty often view students
destined for a Ph.D. as more intellectually complete because they have natural
curiosity and an innate desire to learn. In contrast, premeds are often seen as just
interested in good grades rather than the subject in and of itself. Or, perhaps
professors just like budding academicians better than fledgling physicians
because they are flattered that someone wants to be like them. When I was
actively teaching at Yale, very occasionally one of my undergraduate premedical
students would find a way to indicate that he or she thought that the reason I and
other faculty in the Biology Department were biologists rather than "real
doctors" was because we weren't smart enough to get into medical school. I was
a good student who could have easily gotten into medical school, so there was
no question in my mind that I wanted a scholarly life of basic science research.
In point of fact, in my experience, the best biology students often choose
graduate school over medical school. Whatever, I just wasn't interested in
becoming a medical doctor.

Research

My obvious passion for biology and my ability to work hard and
systematically, as judged by my success in my courses, was quickly channeled
into research. At the onset of my sophomore year, Professor Goodrich appointed
me a part-time research assistant. Hubert Baker Goodrich was a graduate of
Amherst College and did his graduate studies with the great cell biologist and
embryologist E.B. Wilson in the renowned Department of Zoology of Columbia
University at the time when T.H. Morgan, G.N. Calkins, and Bashford Dean
were also there. Professor Goodrich’s background was in just the areas that were
becoming central to my interests—genetics, cytology and embryology. He did
some of the early studies of tissue cells’ relationships to their substrata in
artificial cultures. He aptly called these cells in culture “canoe cells” because
they were tapered at both ends and fat in the middle, like a canoe. He was also
well known for his major work on the development and genetics of pigment
patterns in fish. All in all, he had a distinguished scientific career and he was a
thoughtful and caring mentor to me. I have always remembered his attentiveness to my personal and scientific development and have tried to model myself partially after him. I was truly fortunate to have such careful guidance at the beginning of my research career.

He was a ruggedly built man with a shy, dignified demeanor and a modest but highly competent approach to science (Figure 2.1). In spite of his shyness and reserve, he was a man of much personal warmth. Step by little step,

Figure 2.1. My Wesleyan mentor, Professor H. B. Goodrich, and his wife Clara. The photograph was taken at a testimonial dinner for Dr. Goodrich's retirement in 1956. Courtesy of the Department of Biology, Wesleyan University.

he became like a father for me. I had great confidence in his judgement and apparently he in mine. He suggested the research problem and then left me more or less alone—an approach that I appreciated and emulated during my own professional career many years later.

I was instructed to study the effect of ultraviolet (UV) irradiation on the skin of a hybrid of the common goldfish, *Carassius auratus*. This hybrid, called "calico shebunkin," was interesting because of its highly variegated pigment pattern. This fish gradually forms clusters of black pigment cells, *melanophores*, here and there in the skin during the entire life cycle. The idea was to see if an unpigmented part of the surface could be stimulated to form clusters of
melanophores precociously. The UV treatment did just that, suggesting that latent unpigmented cells called melanoblasts, exist throughout in the skin but only form pigment when stimulated in one way or another. The result was unambiguous. Clusters of melanophores did indeed appear in the irradiated area far more frequently than in untreated areas of the skin. But the cellular basis for the result remained obscure. More work was necessary to see if the hypothesis was valid. In spite of this unsatisfying conclusion, the project was, nevertheless, good training. I had to handle the living material carefully during each treatment, check the fish at daily intervals for effect of the treatment, examine control areas for the appearance of melanophore clusters, make photographic records, read the literature bearing on the subject, and so on. This preliminary investigation was published in a short paper, *The differential effect of radiations on mendelian phenotypes of the goldfish, Carassius auratus* by Dr. Goodrich and me in the *Biological Bulletin*, 1939, 63 years ago. It is not an earth-shaking paper, but it was my first published title. I knew when the journal was going to be published and impatiently checked the new journal stacks in the library for about two weeks before the issue with our paper finally arrived. Needless to say, I was thrilled to see my name in print for the first time in a well-respected scientific journal. I felt as if I was a real biologist. It was the first of many publications that has now included the eight decades from the 1930s to the present (see Appendix I). Even after all these years of publishing, I still get a surge of pride and pleasure from seeing a new publication for the first time. Publications are what we scientists use to mark our professional lives and gauge our intellectual accomplishments.

To carry on this research I was assigned a gloomy windowless corner in the attic of the laboratory. I gradually established myself there and also used it for some of my studying for courses. I shared this space with another student on another research project. This chap, Vincent Lopez, was a quiet, unobtrusive young man whom I hardly got to know in spite of our frequent physical proximity. I remember him mainly because he was very neat and excessively orderly. Everything was carefully stacked or lined up in place. One day he came in with a handful of corks; back in those early twentieth century days we used stoppers made of cork as well as rubber and glass. When he left, I noticed that he had carefully arranged the corks in an exact row by size, from the smallest to the largest. I could not resist exchanging a middle-sized one with a smaller one. The next day I checked to see if he had noticed. He certainly had! The corks were properly realigned. That is the end of the story. I never knew whether he thought I did it or whether it was a small lapse on his part. For a change, I was smart and didn’t repeat the trick. I had my fun and let it go at that. If he suspected that I was thus playing with him it might have sullied an otherwise amicable relationship.

This calls to mind another cute, self-satisfying trick that I cannot resist recounting. It occurred in Woods Hole, many years later. One day, a volume of a
journal in hand, I plopped myself at one of the many desks near the windows in the stacks of the library of the Marine Biological Laboratory. At the desk, I found an interesting doodle penciled on the surface of the desk. Someone, no doubt from Harvard, had neatly drawn a copy of the Harvard seal, complete with the Harvard motto—VERITAS. Happily, it was in pencil. So I very carefully erased the ER and replaced it with AN and just left it there. I have no idea who did the doodle or even whether the culprit saw my tweak. Ah, the little satisfactions of our little lives!

My second experience in research at Wesleyan was much more interesting. It was a so-called honors project and thus counted as a full course. Scientifically, it was a problem in what was then termed physiological genetics and it involved a little old friend of mine, the common tropical aquarium fish, the guppy, Poecilia reticulata (formerly called Lebistes reticulatus). The guppies and other tropical fish were housed in a lovely small greenhouse at the end of the hall of the ground floor of the Shanklin Laboratory. To be near my material, I was assigned a laboratory room a few steps from the greenhouse. Imagine me, just a junior in college, only 20 years old, with my own private lab! This was a privilege that accrued from my decision to select an excellent small college. Again, my mentor was Dr. Goodrich and my problem was again, not unexpectedly, in the area of his research interest—the genetics and developmental physiology of fish color patterns.

My project was to describe the cellular basis and the genetics of a new variety of guppy that bred true. It was called “blond” because of its light yellow color that was expressed in both males and females. In contrast, the familiar wild type guppies are darker and strikingly sexually dimorphic; the females are solid silver and the males possess a brilliant variegated black, yellow and red color pattern.

First I had to determine whether the blond variant depended on a single gene and, if so, whether it was recessive to the wild type, and whether autosomal or sex-linked. This determination normally involves straightforward breeding experiments. The guppy, however, required a special twist. It is viviparous. The female retains the fertilized eggs within her body, with a gestation period of about 30 days, until the developing embryos are ready to hatch, at which point these quite advanced embryos or larvae are spewed into the surrounding medium, the water of the aquarium, or wherever. Thus, to get an accurate count of the phenotype of the young, you had better be there at the time of birth or soon thereafter, else the mother might have devoured some of her babies and thus skewed the result. Also, since the sperm from a single copulation remain in the folds of the uterus up to eight months, as established by a Danish biologist, Øjvind Winge, it is necessary to segregate young females from male fish as soon as they can be distinguished. Then they must be bred with a single male of determined genetic constitution.
Knowing these facts of the life cycle of *Poecilia*, I went to work. Of course it was great fun for me—an old tropical fish fancier. I crossed a virgin wild type female with a blond male and vice versa and found that in both crosses the offspring all had the wild type color pattern. Then I crossed a female with a male, and vice versa of this F1 generation and found that the resulting F2 generation segregated into a ratio of wild type phenotype to blond phenotype of approximately 3:1. Back crosses of F1 fish to blond fish segregated approximately 1:1. I repeated all of these crosses several times, about 16 times for each type of cross. The results lead to a simple conclusion. The blond phenotype depends on a single autosomal recessive gene. It is not carried on a sex chromosome, i.e., is not sex-linked.

Making these crosses may seem like old stuff, typical of a first exercise in any genetics course, but it wasn’t. There is a big emotional difference between routine genetic crosses in a course laboratory and making the crosses with your own organism of unknown genetics. First, I had to provide myself with a set of ripe virgin blond females and sequester them (imagine the crap I took from fellow students for this). Next, I made the crosses, then waited for the offspring to appear, making frequent trips to the greenhouse to check. Then I discovered the living evidence that the hypothesis was valid, and finally separated the sexes as soon as sexual dimorphism appeared. Well, these results were immensely satisfying and all terribly exciting, providing an early example of the vibrant enjoyment of research in biology as well as the elegant logic of genetics.

Study of the development of the blond phenotype revealed something quite fascinating. The cells for the red and yellow pigments and for the reflecting tissue of the blond guppies were normal in number and appearance, just like the wild type. But the cells for black pigment, the melanophores, were quite different. Although present in the same number, they were distinctly smaller than their wild counterparts and mostly failed to disperse their melanin granules into dendritic extensions of the cell surface. Indeed, almost all of them were simply little punctate dots. This is why blond guppies are colored light yellow. The yellow pigment cells (*xanthophores*) dominate. I tried to induce blond guppies to distribute their pigment granules out into their cytoplasm by various treatments, including drugs known in those days to have this effect, by denervation, and by varying the darkness of the background of the aquarium. Unfortunately, I never succeeded in influencing the behavior of melanophores. They remained punctate. Wouldn’t it be interesting to recover this blond mutant now and check the microtubules in the cytoplasm of the melanophores? Microtubules are currently the subject of much intensive research and are known to be the intracellular structure responsible for distributing pigment granules in the cytoplasm of melanophores. Nowadays it would be reasonable to hypothesize that the action of the blond gene is cell-specific, producing its phenotype by interfering with the propensity of tubulin subunits to polymerize to form microtubules (or by altering
kinesin-tubulin interaction) but this was 1938-40. Microtubules were then unknown, and were not discovered until 25 years later by Byers and Porter. My work on the blond guppy was published in the journal *Genetics* in 1944, along with the results of some others on another mutant.

**Summer in Michigan**

Beginning in 1938, I continued my education in biology in the summer time. I learned in my sophomore year that I would take the famous Embryology Course at the Marine Biological Laboratory at Woods Hole in the summer of 1939. So I decided that it would be good experience to go first to a fresh-water laboratory and take a course not available at Wesleyan. Besides, I was making so much money at a terrific job with a Middletown tailoring agency, picking up dirty clothes from students and delivering them clean, that I no longer needed the small summer income I made as a naturalist/counselor at Camp Wawepex. I had a small surplus! So I asked around, wrote for catalogs, and finally decided on the University of Michigan Biological Station on Douglas Lake near the northern tip of the Lower Peninsula. I knew, of course, that the University of Michigan was a great university; moreover, the course program of its summer station and the eminence of some of its faculty appealed to me. I applied, paid tuition, and got a job waiting on tables to pay my room and board and off I went. I hitchhiked to Ann Arbor and then was driven to Douglas Lake by a nice couple from the Zoology Department at Michigan.

Incidentally, I hitchhiked all over the place, a common practice among the young in those days. We were low on cash and not in much of a rush. Mainly, hitching worked quite well. However, once on my trip to Michigan, I had a worry that I might not get to Ann Arbor in time for my ride. Some nice driver let me off in Sandusky, Ohio and there I was stuck for I don’t know how many hours. Just as I was losing hope, somebody finally picked me up. Sandusky, my entry to the Midwest, remains to this day a bad memory. As everyone knows, hitchhiking has long since virtually disappeared in America as a cheap means of travel—too dangerous—too many horror stories about either the driver or the passenger. What a shame! I suppose young people nowadays can afford public transportation, or possess a car, or don’t travel.

My summer in Michigan was superb—a most welcome change from the naturalist job at Camp Wawepex, which had become boring. I took one absolutely first-rate course and one lousy one. The first was in helminthology jointly taught by two distinguished parasitologists; W.W. Cort from the Johns Hopkins School of Public Health and Hygiene and Lyell Thomas from the Department of Zoology of the University of Illinois. What fun, digging out parasitic worms from the guts and wherever of various fish, amphibians, reptiles, birds and mammals, identifying each, preserving it as a mounted specimen and
BRIGHT COLLEGE YEARS

studying their often complicated life cycles. Thomas was in charge of the lab and was a good teacher. In addition, he was an extremely nice man, although a terrible lecturer. Cort gave most of the lectures, and they were outstanding. He was an impressively dignified man with great style and presence. He lectured on all sorts of interesting subjects such as hookworm and *Ascaris* in the South, and infection with blood flukes (schistosomiasis) in the Nile delta via intermediate snail vectors. None of my biology lecturers at Wesleyan had Cort’s authoritative style and clarity. Cort was a model for me in my later lecturing days. The other course I took hardly merits attention. I took it as a gut. It was entitled Mammalogy and Herpetology taught by Charles Creaser from Wayne State University in Detroit. It wasn’t a rigorous course but it was summer and I liked the animals and the field trips in the woods and swamps and out on the lake.

That summer at “Camp,” as the *habitués* called it, was my first vacation with people my own age of 20 or older and I basked in it (actually, I was about the youngest student there). I discovered what a fun-loving character I can be, given a chance. I also learned that I had a knack for making those around me have a good time too. It was a true vacation. I enjoyed it all—the summer, the courses, the lake, the swimming, hacking around with other young men in our male compound of primitive little shacks, waiting on tables (everyone ate in the mess hall), playing the clown at a Station party, where I impersonated a woman (Figure 2.2).

Figure 2.2. The author in drag at a party similar to the one at Douglas Lake. This party was held some years later at the MBL Club. Two of my friends are on the right. The bartender is George Davillafranca, a professor at Smith College. Roger Milkman, then a graduate student at Harvard, is next to Davillafranca. Roger was a scientific collaborator with me and now lives in Woods Hole.
I managed to get into the group photo twice, by sitting at one end and then, after the scanning camera moved past me, running around back of the crowd to seat myself at the other end before the camera reached it. I also dated a young woman briefly toward the end of my stay—Dorothy Cort, the comely daughter of the imposing Professor Cort. The last time we were together I rowed her out on the lake, but we were late getting back. There was a strong offshore wind. Later that year Dorothy invited me to a dance-weekend at Goucher College in Baltimore. I stayed with her parents, whom I suspected approved of me as a companion for their attractive daughter, but nothing came of it.

**Back to College**

In my third year at Wesleyan (1938-39), it had become clear to me that the fields of biology that interested me the most were genetics, cytology and embryology. The good biology courses I took as a sophomore and my own research were important in pointing me in these directions. My leanings were solidified and became essentially irreversible by reading five books that by sheer good luck were published around that time by some of the giants in these fields. *Genetics and the Origin of Species* by Theodosius Dobzhansky (1937), *Recent Advances in Cytology* (1937) by C.D. Darlington, *Embryonic Development and Induction* by Hans Spemann (1938), *An Introduction to Genetics* by A.H. Sturtevant and G.W. Beadle (1939) and *Principles of Development* by Paul Weiss (1939). And, then to top it off, I took the Embryology Course at Woods Hole in the summer of 1939 and attended Richard Goldschmidt’s controversial Silliman Lectures at Yale University entitled *The Material Basis of Evolution* in December 1939. What an intellectual feast this all was and how fortunate was I to have all of them come to pass in those very years when my specialized interests were coalescing.

In looking over my old copy of Sturtevant and Beadle from my college days it occurs to me that current students of genetics (and some of their teachers) would do well to study it and learn how remarkably intellectually coherent and sophisticated a science genetics was before its molecular phase. Some seem not to realize that the foundations of genetics were established well before the advent of the Watson-Crick model of DNA, a landmark discovery that is commonly taken as the beginning of molecular genetics. I particularly suggest that current day embryologists could learn a lesson from how carefully these early geneticists were able to analyze the logical structure of genes. They did this analysis long before the molecular basis of heredity was understood. The best of them were careful not to prejudge what this molecular basis might turn out to be.

Although my early interest in biology caused me to concentrate on courses in science and to accelerate completion of my major in biology there was still time enough to study much outside of science. Two full-year courses that
stand out in my memory are American Cultural and Intellectual History by Hugh Brockunier and Introduction to the History of Philosophy by Cornelius Kruse. These were huge eye-openers. With the guidance of Brockunier, an excellent lecturer and the author of an authoritative work, Roger Williams and Early Rhode Island Democracy, and the full required readings, I gained an appreciation of many vital aspects of American history to which I had never before been exposed. In the philosophy course I learned and pondered the workings of Western philosophers that I had barely heard of before: Plato, Descartes, Spinoza, Hume, Kant, Hegel, Schopenhauer, William James, and Bergson. Indulging in philosophy didn’t influence my scientific thinking much but it was a marvelous intellectual game and helped me develop my own view of how I should lead my life.

In particular, the philosophy course brought to a head my growing doubts about conventional religion. Although I was outwardly a good Methodist up to my departure for college, I had already begun to wonder about some of the “eternal” truths that had been an integral, but unthinking, core of my younger years. The origin of some of these doubts was to be found in the Marxist literature that I had been reading as a senior in high school. By studying philosophy I found that there are a lot of ways of looking at the concept of a supreme being and a lot of ways of building an ethical, moral life and that the latter does not necessarily depend on one’s belief in the former. Not surprisingly, this was a frequent topic of discussion in bull sessions with fellow students. The upshot of all this was a realization that belief in the existence of God is based on a hypothesis that can neither be proved nor disproved. P.B. Medawar, the brilliant English immunologist and essayist, put it best in his 1984 book The Limits of Science; “A reasonable case can be made for saying, not that we believe in God because He exists but rather that He exists because we believe in Him.” A belief in God requires faith (faith being the acceptance of things without evidence). Thus, there were two questions for me. Do I need to believe in a God in order to behave ethically? Do I need faith for a happy, fulfilling life? I decided that for me the answer to both questions was no. With this decision, I became an atheist.

This meant that I could not depend on a religious institution to prescribe my ethical life for me. I had to do it myself. In essence, I consciously codified what I more or less already believed ethically, as a result of my background, with honesty and compassion being at the center. This decision has turned out for me to be of life-long importance. I am still that way, with the same principles, but, of course, with a certain degree of fine-tuning that has come inevitably with experience and reflection.

As you might imagine, given my proselytizing nature, reaching this conviction got me into a lot of arguments with various religious types on campus, mainly Catholics (the chief enemies of my Protestant parents). By my senior year, however, I came to understand that these frequently impassioned
discussions were getting nowhere. Nobody was converted. What became most important for me was how people behaved, not what they believed. “Actions speak louder than words.” “By their fruits, ye shall know them.” In the process, I became more tolerant of many of the beliefs of others, religious or otherwise, judging individuals primarily by their behavior.

Now, after my long life in science, I can report that almost all scientists I have known don’t think enough about religion to call themselves atheists or anything. Although most are highly ethical individuals, religion is just not an important part of their lives. The very few who are religious do not let it interfere with their science, otherwise they would not be very good scientists. For this reason, there isn’t much constructive dialogue between science and religion, in spite of continued efforts to promote it.

In reminiscing recently with a Yale colleague, Allen Garen, a keen-witted molecular geneticist, about how I turned atheist during my early years in college he asked, “What took you so long?” Good question. I think I know why. Right up to the end of my high school years I was having too good a time in our Methodist Church belting out those rousing Protestant hymns and going to church parties to grapple with whether God really existed or not.

In retrospect, I am most grateful for the fine exposure to the history of philosophical thinking through the ages that I had in my philosophy courses during those student years. It helped me through the final stages of the formulation of a personal ethical and moral philosophy that has served me well. A course in the history of various philosophies, not only Western, should be required in the education of all college level students.

In addition to the big history and philosophy courses, I took some little courses for my general education, such as psychology, French, political science, astronomy and music appreciation. For me the last was certainly the most important. I had learned very little about classical Western music at home and at school. The music course introduced me to Palestrina, Purcell, Verdi, Bach, Haydn, Mozart, Beethoven, Brahms, Stravinsky, Debussy, Tchaikovsky and all the rest. It was an enchanting, enriching experience. In addition, it was very demanding, requiring hours and hours of listening to records and recognizing what we heard when the instructor played parts of one or another composition in an examination. This course laid the basis for my subsequent lasting rudimentary enjoyment of classical music. As I recall, I only received a B or maybe a C in the course, a sure sign that grades don’t tell everything.

The astronomy course was taught in the charming little Van Vleck Observatory up on the hill above the athletic fields and overlooking an ancient New England country cemetery. The classroom was tiny, with only about a dozen desks. The teacher, Professor Slocum, was lean, dry, bespectacled and humorless, a Gothic novel’s caricature of a college professor. With him, however, we could peer through the observatory’s excellent 20-inch refracting telescope
on clear nights. That was also an eye-opener. The course did not require much concentrated study and I consequently did not learn much; however, it was certainly agreeable. The cemetery by the observatory was one of my favorite haunts on nice weekends. I would sit there peacefully by myself and read and dream, leaning against a large gravestone.

Since Wesleyan was a private college, it was rather expensive, as are virtually all private colleges in the U.S. I made my way, like many others in similar economic circumstances, by a combination of small help from my parents (who saved money by not having to feed me) and savings from my jobs in Rockville Centre and at Camp Wawepex in the summer. I also received scholarships and loans from the college and jobs at Wesleyan, such as waiting on tables, working for a tailoring agency, and serving as a laboratory teaching assistant in the Introductory Biology Course, as a junior, and in the Comparative Anatomy and Embryology Course, as a senior. I was flattered for having been chosen for this and discovered again by practice how much I enjoy teaching.

**Woods Hole**

I am not certain now when I first learned about the Marine Biological Laboratory (MBL) in Woods Hole. I possibly knew of it by the end of high school but certainly during my first year in college. Already, during my sophomore year, Dr. Goodrich, who was the Director of the Embryology Course at the MBL, assured me that I would have a scholarship from Wesleyan to pay my tuition for that course during the summer after my junior year. Furthermore, he told me that I could cover my room and board by waiting on tables in the mess hall. As the time approached, I was naturally filled with expectations. The reticent Dr. Goodrich didn’t tell me much, but I had seen a photograph of a brick laboratory building in Woods Hole. I knew that, along with the *Stazione Zoologica* in Naples, Woods Hole was the most famous marine laboratory in the world, a place where many important and well-known biologists worked during the summer months. This was enough. The place already had an almost magical aura for me.

So, around the middle of June 1939, the semester over, I took the train for Woods Hole. It was the legendary “Cape Codder,” New York—Woods Hole direct. The trip was uneventful, until we passed over the Cape Cod Canal. Now we were on the Cape; we were close. We moved through forests of scrubby pine trees, slowed down, had a brief glimpse of the sea and stopped. It was the end of the line, the little village of Woods Hole. When I got off, nearby I saw the docks for the steamboats or ferries for Martha’s Vineyard and Nantucket. Wide-eyed, I walked along the harbor toward the MBL just down the street. I soon found the Brick Laboratory I had seen in the photograph, but much expanded. Across a little street from it was a small, nondescript, U-shaped wooden building, its
shingles weathered gray. This was not like my arrival at Wesleyan, no charming campus. Actually, except for the harbor, it was rather bleak. Then I noticed a little sign on the wooden building saying simply *EMBRYOLOGY.* This was the place. I had arrived (Figure 2.3).

![Figure 2.3. Old Main at the Marine Biological Laboratory, similar to how it looked when I first saw it. Photograph used by permission of the Marine Biological Laboratory Archives.](image)

I soon learned that this building was the original main research and teaching laboratory of the MBL, and was referred to as "Old Main" for that reason. When I arrived, and for some years after, it was devoted almost entirely to teaching—Embryology and Physiology in the first part of the summer and Invertebrate Zoology in the last part. Almost all research by "investigators" was done in the big "Brick Lab" across the street, which also housed the library.

After a brief introductory lecture by Professor Goodrich, the so-called "Poison Ivy and Sunburn Lecture," the course started right off. We were given outlines. This was a course on many aspects of embryogenesis: fertilization, cleavage (holoblastic, mosaic and spiral, indeterminate and determinate) gastrulation, organogenesis, formation of larvae, metamorphosis to the adult, regeneration, polarity and gradients, reconstitution, parthenogenesis, and polyspermy. We were to study these in the *living* embryos of organisms that thrive in the rich ocean waters around Woods Hole: vertebrates (teleosts) and various invertebrates [coelenterates, especially hydroids, sponges, annelids, molluscs, squid, echinoderms (starfish and sea urchins), crustaceans, and tunicates (sea squirts)].

We were a pretty large class, about 35 of us from all over, and there were five instructors and two assistants. The faculty was outstanding: H.B. Goodrich, the Director, Viktor Hamburger from Washington University, Donald Costello from the University of North Carolina, Oscar Schotté from Amherst
BRIGHT COLLEGE YEARS

College, and William Ballard from Dartmouth College. In his quiet way, Dr. Goodrich was clearly a man of excellent judgement. The assistants, Eugene Copeland from Harvard and John Milford from New York University were helpful and congenial, as assistants should be. With Gene Copeland, this began a life-long friendship, which so far has lasted 63 years. We still see each other from time to time, for he retired to Woods Hole, as have so many biologists. He still calls me Jasper, which had become my Woods Hole nickname.

Each day started with a lecture by one of the instructors or sometimes by a guest. Since the lectures took place in the laboratory, we had to be sure to shut off the noise of the running sea water system beforehand (and not to forget to turn it back on again after). Then we went to work on the beautiful marine embryological material. For me everything was new. I had read about some of it but had never seen any of it before in the flesh.

As I settled down, I soon discovered, gradually, that there is much more to Woods Hole than the Harbor, Old Main, the Brick Lab, the Mess Hall and the bed I slept in. The MBL library was magnificent, with a rich collection of journals, and, can you believe it, they were arranged in the stacks in alphabetical order, instead of by some screwy Dewey Decimal System. Also, there was an outstanding collection of reprints. It was the most convenient library I had ever used (and still is). Just behind the Brick Lab was the Eel Pond, full of boats. Why it was called a “pond” I could not fathom since it was really a small, safe, almost land-locked extension of the ocean. To the west, just down the street, was the “Fisheries,” another old wooden building, which, I was told, was the oldest marine laboratory of the U.S. Bureau of Fisheries. To the east, also just down the street, was the renowned Woods Hole Oceanographic Institute, with its sea-going, sailing laboratory, the ketch Atlantis. Beyond the laboratories, but not far, were the beaches—Stony Beach and Gansett Beach on Buzzards Bay and Nobska Beach with its lighthouse on Vineyard Sound. There were two lovely peninsulas—Penzance Point, where most of the rich people lived, and Juniper Point, where other rich people lived, and where the U.S. Coast Guard had a major station. Since Woods Hole is itself a peninsula, the sea dominates all—Buzzards Bay to the north and Vineyard Sound to the south, separated from each other by Woods Hole and the Elizabeth Islands, which begin at Woods Hole and string down southwest. The swift tidal current running between Woods Hole and the first of these islands, Nonamesset, the Woods Hole Passage, was also called a hole—“The Hole.” (Figure 2.4).

Although all of this scenery and the extracurricular enjoyment of it was great stuff, it was the Embryology Course that really excited me the most. There was the beauty and wonder of working with living embryos, many of which were so lucid, almost transparent, that we could observe many features of development directly, even at the cellular level. I was especially impressed with the striking beauty of developing embryos and the bewildering array of
developmental motifs to learn. I was awed by the meroblastic cleavage (a kind of cleavage seen in eggs with a large amount of yolk, such as birds and fish, where the cleavage furrows don’t divide the entire egg, but instead form a collection of cells resting on a yolk mass) and dramatic gastrulation of the beautiful embryo of the killifish Fundulus heteroclitus; and the holoblastic cleavage (here the whole egg, yolk and all, is cleaved), gastrulation and formation of the famous trochophore larva of the sand worm Nereis limbata (also those of another worm, Chaetopterus) and its phylogenetic significance; holoblastic cleavage and gastrulation of the sea urchin, Arbacia punctulata (Figure 2.5) and the formation of its jewel-like pluteus larva; and by the formation of the tadpole larva of the sea squirt Ciona.

Ciona was of interest because these ascidians were chordates (subphylum Urochordata), part of the same phylum as all vertebrates, including humans. Admittedly, they are primitive chordates, and by looking at the adult forms one would never guess that they are more closely related to humans, than
say are sea urchins. To the untrained eye, a sea cucumber (of the phylum Echinodermata) and a sea squirt (of the phylum Chordata, subphylum Urochordata) seem more alike than say a sea squirt and a cat. Ascidian larvae, once fully formed, however, have the basic chordate body plan with a dorsal tubular nervous system, a skeletal notochord just ventral to the nervous system, and a complex anterior portion of the anterior digestive tract called the pharynx, associated with paired branchial (pharyngeal) arches that form skeletal elements that support gills (in fish) or make up the face (in humans). I had learned all of this comparative anatomy in the abstract in my undergraduate courses, but here was the opportunity to actually see these embryos alive and developing before my eyes. I was excited by this prospect and enthralled by the beauty of these minute creatures.

It was equally fascinating to observe in detail the remarkably similar regular (mosaic) cleavage of the mollusc Crepidula and the annelid (segmented worm) Nereis and ponder its evolutionary significance. And, we did experiments, repeating classical work on the same or similar material at Woods Hole and Naples, such as parthenogenesis, polarity and gradients, reconstitution of hydroids and sponges, and we experimented on our own. For me, this laboratory work on this gorgeous material was the meat of the course and, I believe, intended was to be so by the faculty.

Each day commenced with a lecture by a member of the faculty. Almost all of the lecturers were good and some were superb. They were based mainly on the embryology of the very species we were studying in the laboratory and of related species and on the developmental questions posed by them. In addition, there were 1 or 2 special lectures each week by members of the staff or guests. Some guests! E.G. Conklin on ascidian development, F.R. Lillie on development
of the feather, Mary E. Rawles on development of feather pigment patterns, A.H. Sturtevant on genes and cytoplasm, Caswell Grave on ascidian metamorphosis, William Duryée on the lampbrush chromosomes of the amphibian germinal vesicle, and Charles Packard, the Director of the MBL, on historical aspects of embryology. Two lectures by members of the faculty stood out: Viktor Hamburger on Spemann and His Historical Background and Oscar Schotte on Amphibian Limb Regeneration. Hamburger was a student of Hans Spemann at the University of Freiburg in the heyday of German experimental embryology. For this, there was a certain aura about him. Two of his fellow students in Freiburg were Johannes Holtfreter, a brilliant experimental embryologist who studied morphogenetic cell movements in amphibian embryos, and Hilde Mangold, who discovered the amphibian “organizer” in her doctoral research. Hamburger was an excellent lecturer with a strong but easily understandable German accent. Lean in appearance and rather formal in manner, he was, nevertheless, kind and helpful and had a dry sense of humor. I came to respect him very much. Hamburger’s students at Washington University (in Saint Louis) loved to tell this story about him. His English was excellent and correct but limited in American slang. Each year in his embryology course at Wash. U., he would have the students perform parthenogenesis (artificial fertilization) by pricking unfertilized frog eggs. Half the class would simply prick the egg and half the class would do the same with a needle that had been immersed in blood. At the end of the experiment the results would be tabulated on the blackboard: the percentage of parthenogenesis with “clean pricks” and with “pricks with blood.” This went on for some years before Hamburger finally understood the double entendre. Professor Hamburger lived a long, fruitful life and died only recently at the age of 100. He was active until almost the end. I count it as one of the outstanding privileges of my life to have known and worked with this great man.

Schotte was also a fine lecturer, with a charming but God-knows what accent. He came from several countries in Europe and also from Spemann’s laboratory, where he performed the famous Schötté-Spemann xenoplastic (cross-species) transplantation of salamander ectoderm from the flank to the prospective mouth region of a frog embryo. The result was the induction of mouth parts, but of the donor, the salamander, showing that the genetic constitution of the responding cells was the determining factor. This experiment, in the 1930s, laid the foundation for much contemporary analysis of the genic basis of development. In contrast to Hamburger, Schotté was a little rotund man, with a little moustache, and a big ebullient personality. He could be difficult but also very charming and had a constant, quick eye for the ladies.

I remember Hamburger’s lecture as one of the conceptual high-points of the course. It was, of course, a thrill for me to get lessons straight from the horse’s mouth, so to speak. Both Hamburger and Schotté had come relatively
recently from Freiburg, which, like Yale under Ross Granville Harrison, was a Mecca for embryologists.

Other members of the faculty lacked the presence of Hamburger and Schotté, but they were expert in their subjects and attentive teachers. My memory of Dr. Costello is of a pale, very thin, rather fragile, very careful man, for whom life seemed a little difficult. I do not recall that he ever smiled. However, he was an excellent embryologist and a devoted teacher. He suffered from congestive heart failure and was sickly much of his adult life. W.W. Ballard, the last and one of the best students of Harrison, was vigorously young and handsome. In addition to knowing his embryology, he was an excellent old-fashioned zoologist, a naturalist, like Hamburger. Bill Ballard and I became close friends, and in later years he became a colleague in research as well, as our interests in early fish embryology converged. Our friendship continued until the end of his life just recently (1998) at the ripe old age of 92.

Among the guest lecturers, Edwin Grant Conklin, of Princeton University, was a student of W.K. Brooks at Johns Hopkins University, as were E.B. Wilson, R.G. Harrison and T.H. Morgan. He gave an outstanding lecture, almost like a preacher, which he once was, his legendary vigor undiminished by advanced age. Frank R. Lillie, of the University of Chicago, was a student of C.O. Whitman at Chicago, who was the first Director of the MBL. His lecture was neat, clear and uninspiring. I gathered that he was like that personally. I was also told that, in addition to his fine science and excellent students, such as B.H. Willier, he was an outstanding administrator at Chicago and a great long-time former Director of the MBL. Alfred H. Sturtevant of California Institute of Technology, was a student of T.H. Morgan at Columbia University, along with C.B. Bridges and H.J. Muller, and, as such, was one of the founders of modern genetics [("modern" for us at that time (1939)]. His lecture was exceedingly low-key and not very illuminating, but to see and hear him was nevertheless an inspiration for me, ever the romantic. He was, after all, the man who had constructed the first chromosome map when he was a graduate student and had since then been famous for his short, compact, precise papers on important discoveries in Drosophila genetics and for his outstanding Introduction to Genetics with Beadle. Lectures by famous biologists, such as these, who were in residence at the MBL as investigators, were certainly part of the glamour of the institution.

Not surprisingly, I enjoyed my fellow students a great deal. All were more or less interested in embryology and some, like me, very much so. Our mutual interests led to many vigorous and profitable discussions of embryological issues, big and small. Also, I was lucky. With few exceptions, we were a congenial lot. We worked together, we ate together, and we played together. I made a number of friends, some of whom have remained so for years: Martha Barnes, who was the belle of the course and was called “Barney,” Elmer
Brueker, Fred Ferguson, Charles Metz, John Shaver, Marcus Singer, Ray Watterson, Dorothy Wellington and Chuck Wilde, who quickly attached himself to Barney. Dorothy Wellington, a very sweet, attractive young woman, invited me to a dance weekend at her college, Wheaton College, during the very next year. The situation in the course was apparently just right, for in this short period of several weeks I made more lasting friendships than in all my four years at Wesleyan.

I was also lucky in that some of my fellow students were very smart and excellent young biologists. I have learned since that not all classes of the course are so endowed. Many years later, when we were both old men, Marc Singer told me that Viktor Hamburger had told him that ours was the best class he had taught in all his years of teaching in Woods Hole. As the years passed, an impressive number of our fellow students went on to notable careers, some distinguished. For example, Charles Metz, my lab table partner from Cal Tech, whom I called Chaz and who gave me the nickname “Jasper,” an appellation that stuck for many years. Others were Marcus Singer (Harvard), Martha Barnes (later Martha Baylor) (University of Illinois), Ray Watterson and Howard Hamilton (University of Rochester), Nevin Schrimshaw (Harvard), John Shaver (University of Pennsylvania), Louis Delanney (Stanford University), Frederick Ferguson (Wesleyan), Roy Gillette (Washington University) and Charles Wilde (Dartmouth). In particular, Marc Singer went on to do very elegant work on the quantitative role of nerves in amphibian limb regeneration, working at Cornell and Western Reserve. Chaz Metz later helped me get a job at Yale and still later ran the Fertilization and Gamete Physiology Course at the MBL, along with Alberto Monroy, Director of the *Stazione Zoologica* in Naples. Ray Watterson finished his career at the University of Illinois, where he did outstanding research on chick development. Nevin Schrimshaw changed fields and became the head of an important laboratory in tropical disease in Costa Rica. Martha Barnes-Baylor became an important member of the faculty at the State University of New York at Stony Brook and Charles Wilde was on the faculty at the University of Pennsylvania.

We worked morning, afternoon and evening. We went swimming. And, as we got to know each other, we fooled around and drank a lot of beer. Our classes (and Physiology, Botany, and Protozoology) were all coed, so there were plenty of young women around. Having classes with young women was a refreshing change for me after my monastic existence at Wesleyan. I even met Dr. Goodrich’s daughter, Mary, an extremely likable young woman. We became friends and enjoyed dancing together. I distinctly recall her once saying, “Jasper, I really enjoy dancing with you.” Well, me too, I enjoyed dancing with her, and with a number of others. Perhaps I was too young and too inexperienced to make anything of her remark, which was probably innocent. Mary and I simply
remained good friends. Eventually, she married one of my classmates in the Embryology Course, Nevin Schrimshaw.

Some of my most vivid memories of that summer were social events connected with the course. Two were embryological: collecting *Nereis* and towing for plankton. *Nereis limbata* (the sandworm) is attracted to the surface by light in some phase of the moon. At just the right lunar phase, huge numbers swarm near the surface in a sort of sexual minuet. We would grab them with a little flat net before they grabbed each other, keeping the sexes separate, and take them back to the lab to fertilize the eggs to observe penetration of the sperm and their typical annelid mosaic cleavage. The collecting on the unstable floating lab dock in the Eel Pond, with everyone trying to see the swarming, led to instability of the collectors, not a few of whom invariably found themselves in the Eel Pond with the *Nereis*. The other event involved crowding onto the biggest MBL collecting boat to go towing in the harbor for pelagic eggs, embryos and larvae; then came the excitement of trying to identify the varied collection in the lab. Trochophore and pluteus larvae in the wild! I remember Ballard being a big help here.

The biggest and most memorable social occasion was the annual course picnic of students, staff and families at Tarpaulin Cove on Vineyard Sound, a splendid sandy indentation of Naushon, the biggest of the Elizabeth Islands. This outing included a lovely boat trip, swimming, walking to the lighthouse and across the island to Kettle Cove on the Buzzard Bay side, and gorging ourselves on lobster, steamed clams, mussels and watermelon. It was a wonderful day. Toward the end of the Embryology Course, we students put together a softball team for the traditional game with Physiology. I don’t remember who played which position, except me of course. I was the talkative catcher and hit a double. It was a notable occasion—the game, we boisterous players, our loyal and loving fans, the idiotic chatter and the beer. To top it off, we won! All in all, this was certainly the best summer of my life up to that point. Now, in retrospect, after many fine summers since, it still remains among the best.

**Cold Spring Harbor**

The Woods Hole Embryology Course terminated near the end of July, so I had to make plans for the rest of the summer. I certainly did not want to spend August in Rockville Centre. I thought of genetics and this lead me to think of the Carnegie Laboratory of Genetics and Experimental Evolution at Cold Spring Harbor. So I wrote Dr. Blakeslee to see if I could work there as a technician. He responded right away, offering just that opportunity. So, when the MBL course was over, I took myself to CSH and found myself working on somatic mutations in the flowers of *Portulaca*. Nowadays, I suppose these
random streaks of color would be thought due to migratory genes called transposons. Cold Spring Harbor was pretty quiet after Woods Hole. I read a lot in their little library, when I was not working, and I would visit my parents on weekends.

In those days the Cold Spring Harbor Laboratory was small and bucolic, not at all like the posh, science-chic campus of today. Some of us summer technicians and research assistants were housed in tents, each set up on a wooden platform. My tentmate was named Taylor Hinton, an intense, voluble young man, who was working on a problem in cytogenetics with Milislav Demerec and was nuts about the problem and about genetics in general. We talked a lot about his research, which involved trying to localize a gene on a salivary chromosome of Drosophila, and he filled me in on some of the professional gossip in the field of genetics. Another young geneticist at the laboratory, Philip Bridges, was quite the opposite, rather quiet and shy. He stood out anyway because he had the misfortune of being the son of the brilliant geneticist, Calvin B. Bridges, the author of one of the pillars of genetics, Nondisjunction as proof of the chromosome theory of heredity (Genetics, 1916) and many other important papers. Bridges was also a philanderer of international note. An English geneticist was said to have remarked: "Bridges? Remarkable man. All brains and balls." Poor Philip! My stay at Cold Spring Harbor was an interesting but not very exhilarating period and I was happy to return to Wesleyan when the summer was over.

**Back to College Again**

Along with my full curriculum at Wesleyan, I also participated in certain extracurricular activities, mainly political, given my convictions. I considered theatre, building on my high school attachment to acting, but decided against it. Too time consuming. Politically, I was active in the then current national left-wing student organization, the American Student Union (ASU), I was its President during my senior year at Wesleyan. I don’t recall that we did very much. Also that year, I was the President of the John Wesley Club, a non-fraternity social organization, and was elected Secretary-Treasurer of the college body. This was a consolation prize; I was voted down for both President and Vice-President in the campus-wide election. My election to this position was a joke; I’ve never been interested in money or good at handling it. The membership of the John Wesley Club was a hodge-podge of students, who, for one or more reasons, did not belong to one of the so-called Greek-letter fraternities—lack of money, being Jewish, black, or maybe even Greek (the fraternities discriminated), or principle. In my case, being a WASP, the bigotry of the fraternities was no obstacle. For me, it was money and principle. I could not afford it and I would not associate with a club that so discriminated. And they
really did, completely out of keeping with the academic spirit of the college as a whole. I was also a member of the varsity swimming team, but only in a formal sense. I was the third breast-stroker. Unfortunately, try as I might, I never got better than the second breast-stroker, so I never competed in an actual intercollegiate meet. But the training in technique was excellent and it was healthful (as if, at that age, one needs to swim 50 laps a day in order to be healthy).

As a reward for having achieved the prestige of being a political big shot, BMOC, a big fish in a small pond, I was elected to one of the two honorary senior secret societies—Mystical Seven, whose meeting place was a small, brick, heptagonal building. I enjoyed it because the six other members were worth knowing. The most interesting for me was Charles Gillespie, a chemistry major, who went on to graduate school at Harvard to study history and then to a long, distinguished career as Professor of the History of Science at Princeton and author of the fine work *The Edge of Objectivity*.

I recall devoting a considerable amount of energy and enthusiasm to these various extracurricular activities but, strangely, do not now remember my participation in them in anywhere near as much detail as I do my course work, my research and my intellectual life. I bet if I had been a star athlete, especially in football, I would remember a lot about that. No matter. When all is said and done, my college did the essential things for me.

Being an outgoing person, I made many friends in college but for one reason or another only three of them lasted. Clifford Davenport was a good left-wing friend, but after he graduated I more or less forgot about him. He was a very fine person but too serious for me. He had little sense of humor. We saw each other again recently as old men in Mill Hill, California. He was still very serious.

David Jones was smart and a lot of fun. We saw each other a little after graduation in the military service in Italy and later at a cocktail party in his apartment in Harlem. He was a lawyer trained at Harvard Law School, and held an important administrative post for the city of New York. His wife was chic and as beautiful as a professional fashion model. I found her rather cold in comparison with her handsome husband and with Jewel Plummer, my warm, attractive friend from Woods Hole, who had brought me to the party. Dave’s wife was the daughter of the then President of the NAACP. Also, at this smashing party, I met a jolly young professor from Princeton, Charles Davis, who later joined the faculty at Yale University, where he became Head of African-American Studies and Master of Calhoun College (see p. 212). At Yale, we became close friends, enjoying many a merry lunch together. Very sadly, soon after I last saw him, Dave died very young and quickly from a very aggressive cancer.

Robert Cohen was a freshman when I was a senior and he quickly became a sort of little brother for me, and I a sort of big brother for him. He was
a very warm person and absolutely brilliant intellectually. Later, when I joined the Yale faculty, I was delighted to find Bobby there as a Fellow of the American Council of Learned Societies in the philosophy of science. I had many good times with him and his wife Robin before they left New Haven for Boston, where he eventually headed the Physics Department at Boston University. I also enjoyed associating with a number of other fellow students, but none of them were close or of enough interest for me to look them up after college. Except for Cliff, Dave and Bobby, they apparently felt the same about me, for I haven’t seen any of them since except at our fiftieth (and for me only) class reunion.

My senior year was the richest in many ways. I have learned since from many others that this is often so. I had grown up intellectually to a marked degree. I concentrated on my research and writing my honors thesis. I took seminars in political science and modern philosophy, the last being a class of but four of us meeting weekly with Professor Krusé. Each week each of us would read a paper on an assigned aspect of a particular philosopher. Our seminar took place in The Honors College, a fine, old, imposing Georgian mansion, with lovely gardens. We would always take a break for tea. I liked that. This was the regal, intellectual day of each week, an important part of the slow civilizing of J.P. Trinkaus.

During that year I was elected to Phi Beta Kappa, the American honorary scholastic society. Although I knew that this honor was based entirely and only on my course grades, I was nevertheless happy to accept it. Curiously at the time, the Wesleyan student body year after year voted overwhelmingly that this was the highest honor that could be bestowed on an undergraduate. Also, in those days, men frequently sported pocket watches rather than wristwatches, and in academic circles it was easy to identify a member of Phi Beta Kappa by the key suspended from his watch chain. Appropriately, my commencement present from my parents was a Hamilton pocket watch, for which I quickly bought a chain from which I could suspend my Phi Beta Kappa key. Later, in graduate school, when I learned that most of my associates had been elected to Phi Beta Kappa as undergraduates, the key lost its luster. I became a little embarrassed at my pretentiousness. I quietly put my key away and have since misplaced it.

Early in my senior year I needed to get my act together on the matter of where and with whom to do my graduate work. I was also still in a quandary about my chosen field of study. Should I choose embryology, cytology or genetics? Here too Dr. Goodrich was of much help. I find that even now, after all these years, I am uneasy referring to this great man simply as “Goodrich,” as if he were a peer. I still have great respect for his guidance and intellectual ability. I was imprinted early to think of him as Dr. or Professor Goodrich. My leanings were toward embryology because of the Woods Hole experience, and I was torn between studying with Benjamin H. Willier at the University of Rochester and Victor C. Twitty at Stanford University. I found myself thinking more about
Twitty. He was a first-rate embryologist, an extremely congenial man, one of Harrison's best students at Yale, and he was in California. Dr. Goodrich was gently pushing me toward Willier, also a first-rate embryologist, one of F.R. Lillie's best students at Chicago, and a proven successful mentor. He had attracted and trained students like Mary E. Rawles and Dorothea Rudnick and my fellow students in the Embryology Course, Ray Watterson and Howard Hamilton. But I had found him a little distant upon meeting him. Moreover, he was in Rochester up in upstate New York, with its challenging winters. In spite of the winters, however, I had to face an additional important factor. Unlike Twitty, Willier had as a faculty colleague one of the world's great geneticists, Curt Stern, who was also a superb human being. So I finally bit the bullet, applied to Rochester, and was accepted.

But, then, completely out of the blue, I received a letter from the Dean of Dartmouth College informing me that I had been awarded the Cramer Fellowship of that year for studies in genetics. I had not applied for this fellowship. In fact, I had never heard of it. What a colossal, marvelous surprise! With this fellowship I could go anywhere. I quickly thought of England but that was impossible. There was a war going on over there. So, where to go? I had learned just that year that Theodosius Dobzhansky at Cal Tech and Marcus Rhodes at Indiana, both leading geneticists, had just moved to Columbia University. These, with the faculty already there, made Columbia one of the most attractive universities anywhere for the study of genetics and cytology. Indeed, L.C. Dunn, a leading mouse geneticist, was a developmental geneticist. So, I decided to go to Columbia for a year and wrote Dr. Stern at Rochester about my decision (Willier in the meantime was moving to Johns Hopkins). Dr. Stern was characteristically gracious and reserved an assistantship for me for the following year.

During this search for a graduate school, I also thought of Princeton University. It had a good embryologist, Elmer Butler, and a good cytologist, Gerhard Fankhauser, and a reputably beautiful campus. Since Princeton was nearby, I went for a visit. Professor Butler, a quiet, dignified man, was a generous host and gave me the $5.00 tour of the campus. Princeton really is a beautiful university, almost out of Hollywood; but for my special interests it was not up to Stanford or Rochester. I also thought briefly of the University of Chicago and Paul Weiss. However, although Weiss was already the dominant figure in the field of development, he also had a very dominating personality and thus was not my kettle of fish.

At each Wesleyan Commencement the Rich Prize was offered for the best commencement oration. A friend from my philosophy seminar, Asher Moore, and I won the preliminary competition and thus competed for the prize at Commencement. The prize was $100, a fair amount of mazuma in those days, certainly well over $1,000 in today's dollars. Since we had both beaten out the
competition and did not find either better than the other, we decided secretly to split the prize. The title of my oration, *Science and the Crisis of Modern Thought* was, I am afraid, youthfully ostentatious and, as Commencement approached, seemed somewhat out of place with what was happening across the Atlantic. This was June 1940 and we were all shocked by the disintegration of the French armies and the imminent fall of France to the Nazis. In fact, Marshal Pétain sued for an armistice with Germany on the 16th of June, the very day of Commencement. I felt that I should somehow refer to this enormous event in some relevant way in my oration, but failed to find the proper words. Although this was an ominous and frightening event, as a matter of fact, life went on for us Americans then and for some time after in pretty much the same way, regardless of what was happening in Europe. Anyway, I won the Rich Prize, to the delight of my parents and myself. I graduated with High Distinction in Biology and soon thereafter I sent Asher his half of the prize money.

Recently, after completing a rough draft of this chapter, I consulted the report of the 50th Reunion of the Class of 1940 to see if I had overlooked something worth mentioning. To my amazement, I found something meritorious about myself that I had completely forgotten. “The results of the Argus Poll (of June 1940) showed that John Trinkhaus and Henry Anderson were tied in the judgement of the Class for having done the most for Wesleyan.” (Whatever that means.) This, in spite of my politics, which is interesting since the overwhelming majority of the Class were Republicans or became so as alumni. I cite this lapse of memory as testimony to my well-known, deep-seated sense of modesty, but it would have been nice if they had spelled my name correctly.