While involved in this research on determination and cell sorting my academic and personal life moved along inexorably but with a number of important developments. I had arrived at Yale with the rank of instructor, the lowest faculty rank, so low indeed that it has long since disappeared, except in medical schools. After two years I was promoted to a three-year term as assistant professor, which was then repeated for three more years. I was grateful for these promotions for the small recognition they signified but particularly because each one increased my salary slightly, although not enough. With post-war inflation (no more war time price controls), I really didn’t make enough to sustain our growing family (three small children) and felt forced to seek additional compensation. There was at that time a small women’s college in Hartford called Hartford College that depended for much of its teaching staff on part-time teaching by underpaid faculty of nearby, established, larger institutions. I was one of those, driving to Hartford twice a week to lecture to young women on Introductory Biology. I thought it necessary for the small monetary compensation, but it certainly drained time and effort from other activities, mainly my research. I was already devoting little more than the necessary minimum as a teacher at Yale and as a husband and father at home.

What about the importance of money in my life? I did not then, nor have ever since, placed much value on the accumulation of gelt as such. When as a youth I was dreaming and planning to become a biologist and a professor, I never gave a thought to how much money I would make. Later, the very low salary offered by Yale as an instructor did not faze me. I knew it would be difficult but I couldn’t turn down an opportunity like that. Well, I taught that course at Hartford College for a few years when we felt a bit desperate. When Yale increased my salary a little more, I quit my moonlighting in Hartford. Some people, I gather lots of people, who already have good salaries (I’m not talking about the poor), enjoy the accumulation of more money than necessary in order
to satisfy expanding expensive tastes or for its own sake, as a status symbol, or for having been poor as children. Not I. I need enough, and perhaps a little more, to keep healthy and have fun in my own way. As everyone who knows me knows, I have mostly had a ball living out my life, in a spite of a constantly low income typical for a professor in a faculty of the arts and sciences. I would certainly have been more affluent if I had been on the faculty of a medical school or law school.

Tenure

Yes, there is more than money to academic life, much more, such as tenure, a much discussed but poorly understood feature of essentially all American colleges and universities. Tenure means permanence of position with a guarantee of academic freedom. Once one has gained tenure, at either the associate or full professor level, one not only has an assured job for the rest of his or her career but also, and very important, freedom to express one’s mind on almost everything. This latter feature, I believe, was the reason tenure was established in the first place. Certainly freedom of expression is essential for the optimal operation of a university with a creative, independently minded faculty. This critical aspect of tenure is often forgotten in the tense drive of young faculty to climb successfully the ladder leading to it. It’s the security of a permanent job that is on their minds and, of course, their egos as well.

Personally, I hardly gave tenure a thought in my young years as a nontenured assistant professor. I lost no sleep over it. I was happy in the present and not much concerned about the future. However, an event very close to me suddenly reminded me that there is a future. When I was in my second term as assistant professor, my colleague, Charles Metz, my old friend and Woods Hole buddy, was denied tenure. I was not particularly surprised because his research was lagging and he showed much less interest in his teaching than I, for instance. After receiving the bad news, he left quickly in a huff, well before the year was out, to take a temporary research position at Cal Tech with his old mentor Albert Tyler, with whom he had taken his Ph.D., never to be seen at Yale again. Chaz was so deeply embittered by this rejection that he hated Yale and everything about it for many years, perhaps forever. His ego, it seems, was irreparably wounded. This setback even sullied his friendship with me and rendered our old relationship at Woods Hole a little touchy, unless we confined our conversation to fishing or sailing, at both of which he was very skillful. I tell this sad story of Charles Metz to illustrate how corrosive denial of tenure can be to the victim. Metz ended up on the faculty of the University of Miami for the rest of his impressively productive academic life.

The period of waiting as the tenure decision approaches can also be psychologically damaging, indeed profoundly stressful for the agony one usually
goes through. It is a genuinely important event in one’s young life. I can report, however, that in my own case, it was pretty much a matter-of-fact issue. For whatever the reasons, I was not really worried about it. When I was promoted to associate professor with tenure in 1956 I was not surprised. Whether this was due to exaggerated self-confidence, immature bravado, my happy-go-lucky nature, or all three, I’m not sure. It was certainly not due to any conscious sense of nobleness on my part, being above it all. From my years at Wesleyan on I had been blessed with a chain of repeated good luck, so why not now? I was glad to have tenure but, as I look back on it now, I believe that there was indeed an element of good luck, more than I thought at the time. As I have pointed out, the Zoology Department at Yale at that time was not a very strong department and weakness tends to perpetuate weakness. I am afraid that my scholarly record then would not have assured me of tenure today in our much stronger department. On the other hand, I had shown genuine promise in the originality of my research and, if I may say it, excellence in teaching; I was widely recognized as the leading and by far the most popular teacher in the tough and demanding course—Zoology 23. Whatever the reasons, my record since in scholarly research and in the outstanding research of some of my graduate students and postdoctoral fellows has certainly validated the correctness of the decision for my tenure.

The question of tenure often poses agonizing problems for the department and the university as well as for the candidate. How to be sure that this bright, productive young person, who is sometimes an outstanding teacher, will continue that way? If a mistake is made, for many reasons, including soft-heartedness, you are stuck with a colleague who is not a productive scholar, often for many years to come. They usually hang on and, in effect, become parasites of the university. Some become deans. Tenure decisions are exceedingly important for the university as well as for the individual and are often tough for both sides.

Anyway, I was glad to have been granted tenure for three good reasons. First, it bolstered my already reasonably intact ego. Second, it assured me the academic freedom that my political principles and obstreperous character would surely need. Third, it gave me economic security, not much in dollars, but enough for us to be able to think immediately of buying a house.

After renting and living in four other people’s houses over an eight year period, we all had a yen for settling down in a place of our own. Quite by chance, we had met a man named Milton Wolff at an art show in New Haven. I was bored by the paintings and so was he. As I wandered away from the crowd I encountered Wolff, alone like me. We exchanged comments and thus started a friendship. Milt, a big, robust, dominant man, was sympatico and charismatic, instantly attracting my attention. I soon learned that Milt was also a kindred spirit politically and really quite famous. He had not only fought in the Spanish Civil
War but was the Commandant of the Abraham Lincoln Brigade, the American contingent of the International Brigade (the same unit in which Clem Markert had fought). I bring him up here, however, for another reason. He lived in Stony Creek, a small unpretentious village on Long Island Sound about 10 miles east of New Haven to which, through Milt, we had become quite attached. It so happened that as we began looking for a house he told us of one that had just become available down the street and around the corner from his own, facing directly on Long Island Sound and the Thimble Islands off shore. It was an ideal place for raising children—swimming, sledding, bird-watching, exploring the woods and the islands, fishing, name it. So we bought it. We brought our old Aimesbury dory down from Woods Hole. We visited the islands (after their summer people were gone). Our son Erik used the dory to tend several lobster pots he had put out. I raised chickens and occasionally boozed it up with Milt and two lobstermen, Clare and Milton Bradley, whose shack on the water was just below our house. We had three cats and a dog.

**Graduate Students**

As I became more established as a faculty member and scientist, I began attracting graduate students. Since the Doctor of Philosophy degree is a research degree, it involves training in original, independent research. This is a complex and subtle business, varying somewhat from student to student and from professor to professor and with the relationship established between the two. In my opinion there are two extremes to be avoided. The first is to permit a student to select a problem outside the area of expertise of the mentor and give the student so much independence that he or she may falter drastically and become lost. Unhappily I must admit guilt in committing the first extreme with my very first graduate student. This bright young woman came to me with the idea of doing an immunochemical analysis of the developing lens of the chick embryo. I knew quite a lot about chick embryos, even their eyes, but very little immunochemistry. So she struggled, sought and obtained good help from others, and, in the end, produced a thoroughly acceptable dissertation. Her name was Ruth Beloff. Perhaps the greatest benefit of our student-professor relationship was a warm, life-long friendship of immense value to both of us. Later on our friendship was enhanced and extended by her secret, torrid romance with my dear friend Cliff Grobstein in the 1960s, a romance that eventually led to marriage. What a handsome couple they were and what good times we had with them, mainly in La Jolla and thereabouts. Cliff by then was Dean of the Medical School at UCSD and not long after Ruth became Director of Oncology at the Scripps Clinic.

The other extreme in the student-teacher relationship leading to the Ph.D. degree is to assign the student a problem that is a limited segment of the research program of the mentor, who then supervises the student’s progress very
frequently, even on a weekly or daily basis. This method gives the students virtually no independence and hence little opportunity to learn from their own mistakes, learn how to correct them, and develop their own ideas. It is in part a means of using the student as a high-level technician to advance the research program of the professor, a violation of the purpose of a graduate education—the creation of a new independent scholar. A university is our most advanced educational institution, not another research institute. At its worst, this method of training can be intensely stressful for the student. There is good reason to suspect that the much-publicized recent suicide of a graduate student in the Chemistry Department at Harvard was at least in part caused by the pressure of such daily research conferences with his professor. It is also questionable how well this close supervision equips candidates for going out on their own, where they are confronted with the necessity, and I hope the pleasure, of building their own research programs. Needless to say, I have assiduously avoided this military approach in the training of graduate students. Extreme though it is, this way of training students in research seems to be rampant today. Perhaps it is a necessary adaptation to the team nature of much modern research in biology. Is the epoch of the “independent investigator” drawing to an end? I am afraid that it is. This trend is an inevitable result of the increasing complexity and expense of modern biological investigation but I confess to a strong nostalgia for the old days when scientists could focus more on research and less on funding their “group” of technical experts.

Since I have been reasonably successful in the training of graduate students, as judged by the number who have had successful research careers and fine academic positions after leaving Mother Yale, my approach to training students for research and teaching careers may be of interest. After my initial mistake with Ruth Beloff, my approach became a modification of the old-fashioned European method I had been exposed to under Willier. After some discussion, assign the student a problem that has a high probability of being soluble in my general area of research, or accept a problem proposed by the student subject to the same requirements. Usually, the latter had to be abandoned after a good trial because the proposal was unrealistic. I tried to err in the direction of giving the students latitude, even if I thought their ideas were not very good. In some cases, I tried to coax the students into a more fruitful direction while making them think my ideas were theirs. I didn’t hesitate to use this kind of subterfuge, because I felt it was in the student’s best interests—as they say in the NBA, “No harm, no foul.” I required that they chose problems in “my general area of interest” so that I would possess enough expertise to be of use when needed but not precisely in my own program of research. I was busy at the bench myself and didn’t need help from a graduate student; moreover, there was the danger of using the student’s energy and enthusiasm to further my own research goals rather than their own.
Once a problem was agreed upon, the old approach of independence set in. We would consult once in a while, once every few months or at least once a year when I was away on leave, but still communicating with the students via extensive letters. Former students have kidded me that these letters were more valuable than personal meetings because I was more often awake while writing. Essentially, the students were on their own, free to do their own thing. Professor Harrison, who used this approach with famous success, called it the "sink or swim approach." The idea was that if a degree candidate is able "independently" to produce research that clearly advances the field and is acceptable for publication in a reputable journal, the student merited the award of the Ph.D. I am afraid that quite a few potential students sank or sought guidance from other mentors. It is my impression that, in general, the best students prefer this high degree of independence. A number of my own students have told me so, and that this was one of the reasons they chose me as their adviser. This "independent research" was not performed in a vacuum. Science is a highly social human endeavor. The students, depending on their needs and inclinations, were in frequent contact with the literature, other students in the laboratory and, when needed, other workers in their specialty. When I was at Johns Hopkins, I almost never had discussions of my research with Willier, my professor, but discussed it freely and often with great profit with some of my excellent fellow graduate students. This has led me to believe that there are four major functions of the professor. The first is to be distinguished enough to attract highly intelligent, highly motivated students who possess great energy. The second is to assign interesting, soluble problems. The third is to give the students much independence. The fourth is to set high standards by example in one's own research and especially in the composition of the dissertation. Members of such a group of students then get much of their daily education from each other.

Not coincidentally, leaving students alone was of great benefit for me, the professor. The students had the time to make their own mistakes, learn from them, develop new ideas and entries into the problem, and prepare for their own research careers after Yale. Conveniently, I also had time to pursue my own interests, curricular and extracurricular. Although it was self-serving if you wish, it gave me freedom of mind and lots of time for my own research and is one of the reasons why I have been able to enjoy being at the bench myself my entire academic life. Also, so as not to appear overly self-congratulatory about the success of my graduate students, let me point out that the ability to work independently is usually a reliable predictor of later success after graduate school. My system was selective for success and my failures weren't really counted against me. In addition, the students were selected for high intellectual capacity before they ever go into the graduate program. How convenient.

Later on, as I had more students doing their dissertation research with me and an occasional select undergraduate and a postdoctoral fellow or two, we
would all meet together once a week in my large, comfortable office to discuss our current work. We were often joined by a graduate students or postdoctoral fellows from another lab in the Biology Department or from the Yale Medical School. Occasionally guest investigators who happened to be in town would discuss their own research. These seminars were highly informal with one of us (including myself) discussing what we were doing or thinking of doing at the moment. I tried not to dominate and believe I mostly succeeded. Discussion was free and open-ended with frequent interruptions. We met at 4:30 on Friday afternoon, followed by beer and whatever with spouses and friends. These TGIF seminars were always enriching intellectually and added spirit to the lab. The discussions were always spirited and at times acrimonious, but everyone knew that if you could pass through this intellectual Parris Island unscathed, you could excel making presentations in less critical company. The role of Drill Instructor usually fell to some senior graduate student. I was most often more laid-back, trying to learn something new. These sessions, although grueling, were excellent training for the students and an efficient way for me to keep abreast of my field.

Toward the end of each graduate student’s career there was the problem of writing the dissertation, most often a big problem. During this period I invariably became deeply involved. Being products of American schools, most of these very bright young people had never been taught to write clearly in a well-organized way, so it was up to me to teach them how to use the English language, as well as how to write a scientific paper. This was often pretty damaging to the egos of some, but they survived and profited from it and most, but not all, thanked me afterward. For example, one of my students had to write seven drafts of his dissertation over the course of a full year while one of his slightly junior colleagues (and chief rivals) wrote his dissertation in a few months without requiring substantial revision. You see, one student could write well and the other could not. The poor writer was nearly driven to distraction by what he assumed was favoritism. Years later, after he had matured, he confided to me that he really appreciated my high writing standards, painful as they were at the time. The poor writer eventually came to recognize the superiority of his rival’s writing style and the two have been good friends ever since that difficult time. In most cases, the reward was not only a well-written dissertation and a Yale Ph.D. degree, but also a published paper or two of their own.

Following tradition and principle, I never added my name to the authorship of the published papers based on the doctoral research of any of my graduate students. Willier did not add his name to the papers of his students, nor did Harrison, nor T.H. Morgan and so on. The students are proud of the research and happy to be the sole author of its publication, often their first. By this, the students are introduced as a new, full-fledged member of the scientific
community and could proudly send a reprint to their parents, siblings, and friends.

This ancient practice needs emphasis since it seems nowadays that the big professors of every lab, wherever, invariably and automatically add their name to the list of authors of every paper published by anyone and everyone in their laboratory (especially in medical schools).

I encountered a good example of this widespread segment of contemporary scientific culture just a couple of years ago at cocktails before a Fellows Meeting in Branford College. Jokingly, I asked the chairman of one of the important departments in the Yale School of Medicine, who had previously bragged to me about how many papers he had published that year, “How many papers did you publish this year?”

He answered, “I have published 14.”
“Are you sure? Not 13 or 15?”
“Yes, 14!”
“Were you involved in the bench work for all of this research?”
“No, but I was involved intellectually and all these people are in my lab.”

He never got the joke. Or maybe he did. He hasn’t showed up at a Fellow’s Meeting since. Childish, this hang-up on the number of papers published. However, he is by no means alone. E.O. Wilson, the ant man, a truly great biologist and gifted writer, who should know better, brags in his memoirs that he had published something like 55 (I don’t recall the exact number) papers by the age of 29! It is easy to understand why there is so much emphasis on numbers of publications, rather than, say, the quality of the papers or their importance and impact on the field. Promotions and tenure decisions are often made by groups of colleagues who have difficulty making critical evaluations of the quality of work (because of increasing specialization and fragmentation of scientific disciplines), so they seek refuge in the more easily measured quantity of work. Pressure for academic advancement has also led to the questionable practice of MIRVing papers, i.e., turning one study into numerous papers (cynically named for the intercontinental ballistic missiles known as Multiple, Independently targeted Reentry Vehicles), stealing other’s ideas, or even simple fabrication of results. The same pressures currently apply to obtaining grants to support research. Granting agencies use detailed review by a panel of peers in awarding grants. Often, the peers rely on numbers of papers and reputation of journals in which these papers appear, rather than strictly evaluating the quality of the proposed experiments. Decisions on grant awards, like promotion and tenure decisions, are often made intuitively and quantitatively because the technical details of the science can be so specialized that few evaluators are authentic intellectual peers.
One of the several ingredients of the good life of a professor is academic leave, the opportunity to be free of teaching and other academic duties for a semester or even a full year in order to devote oneself entirely to scholarly pursuits. Indeed, one of the perquisites of tenure was the right to take academic leave from time to time. My first thought when receiving tenure was to buy a house. That being done, my thoughts turned to taking a year leave of absence in a foreign country. My brief tour in Italy, when in the military, gave me a taste of Europe. But this was unthinkable for me in 1957-58 because of the infamous Loyalty Oath. To obtain a passport I would have to pledge “that I am not now nor ever have been a member of the Communist Party” etc. I could not make such a pledge on two grounds. First, and most important, it was a clear violation of my rights under the First Amendment to the Constitution. Second, the pledge would be an outright lie about my past. Then around 1958, the Supreme Court ruled that the Loyalty Oath was unconstitutional, a violation of the First Amendment, thus freeing me to apply for a passport, which I did immediately.

But where to go and how pay for it? I decided on Paris. Why Paris? Principally, because it was exotic enough to be exciting and refreshingly intriguing but familiar enough to be comprehended and managed. It was fascinating without being intimidating. Also, there was a good embryologist there, Etienne Wolff, an expert on embryonic sexual differentiation at Collège de France, whose work I knew from my graduate student days. And finally, women. I had the typical romantic male American idea of the special attractiveness of French women and was eager to find out if it were true. My marriage to Galya was in disarray, slowly falling apart over the last several years, and I was looking around and susceptible.

I should emphasize that while our marriage was deteriorating, we maintained a deep feeling of love and responsibility for our children. There was essential agreement between us on how to raise the children. I think we were both excellent loving parents and were generally regarded as such. Anyone who knows any of our children now will testify to what fine people they are. Anyway, Galya and I thought that Paris would be wonderful for our children, as well as for us.

The next step to tackle was how to pay for this grand voyage of not only myself but of my whole family. I applied to the John Simon Guggenheim Foundation for a Guggenheim Fellowship and to the State Department of the Federal Government for a Fulbright Fellowship. I was awarded a Guggenheim but was turned down for a Fulbright. There is a story to tell here. It so happened that my friend Ed Zwilling was a member of the committee appointed by the
State Department for reviewing candidates applying for a Fulbright in biology. As he told me, the review committee approved my application and recommended that I be awarded a Fulbright. Knowing that my political background might create problems at the State Department (in spite of the Supreme Court’s ruling on the Loyalty Oath), they put me emphatically at the top of their list. Nonetheless, the State Department turned down my application, obviously on political grounds. I have often told this story of how the private Guggenheim Foundation ignored my political background in awarding me a fellowship, whereas the still McCarthyite State Department clearly took it into account, even in the case of a biologist. The same thing happened to Bert Lowenberg, an historian at Sarah Lawrence College and a friend and colleague of my brother Charles. Lowenberg’s review committee of historians was outraged and protested this blatant and illegal political discrimination openly in a letter to the New York Times. I don’t recall the outcome. My committee of biologists was also angry but apparently did nothing about it.

There is a sequel to this story. Clement Markert, my friend and fellow student at Johns Hopkins, applied for a Guggenheim Fellowship some years later, when he was a colleague in our Department at Yale, and was turned down. Those who knew Clem can imagine how outraged he was by this affront and will not be surprised to learn that he quickly looked into it. What he discovered was that the Guggenheim Foundation apparently regretted its award of a fellowship to me because of having overlooked my radical background and was not about to make the same mistake again because of Markert’s even more radical background. Clem recently assured me that his source for this information was impeccable. So much for the purity of the Guggenheim. Clem wrote me just last year, completely in character, months before he died of lung cancer, that he was “...annoyed by the Guggenheim and never again asked them for anything.”

It was an honor to be awarded a Guggenheim Fellowship, but the stipend unfortunately was not very generous and insufficient for our needs. Yale took care of this through the offices of the Master of Branford College, Norman S. Buck, who had just become Provost of the University. He found some hidden source of university funds for me that, added to the Guggenheim, made our dream to spend a year in Paris come true.

Incidentally, because of my approach to the training of graduate students, I gave little thought to abandoning my students for the year. Robert Hilfer, J. Richard Whittaker, and James Weston did very well without me. Norman Wessells, who was in his last year and writing his dissertation, sent drafts to me in Paris for corrections and suggestions. All four survived and eventually went on to distinguished careers. So my method of benign neglect worked, especially given the high quality of Yale graduate students.
Paris

A year in Paris! That was some year. I was in awe of this great city straight off, without quibble or doubt. I loved it, like most everyone. We lived in a very ordinary apartment on Boulevard Lefèvre in the rather ordinary 15th Arrondissement (except for the Institut Pasteur) but that made no difference. Paris is a large but compact city. We could easily go everywhere by Metro and of course see more by just walking. Like good tourists we got a Guide Michelin Vert and, with its help and the help of friends, eventually visited most of the usual attractions. I found myself to be a typical avid tourist. But the best part of Paris for me was to be found in aimlessly wandering around exploring the innumerable charms not found in the guidebooks. My favorite haunt soon became the Left Bank, the 5th Arrondissement, where I worked at Collège de France, and the adjoining 6th (Saint Germain des Prés). I was particularly charmed by Isle St. Louis, with its almost labyrinthine, secretive streets.

We sent our children to an excellent, well-known, French private school, l’Ecole Alsacienne, where they were plunged immediately in the French language and, as children, quickly picked it up. They also learned a lot of other things. Their new French school was quite demanding, a big contrast to the American public schools they had been attending in Connecticut. I too had to work seriously on my rudimentary knowledge of spoken French so I went regularly to Alliance Française, a superb French government school for teaching the French language to foreigners. Everything was in French, everything. My classmates in the beginner’s class consisted of about a dozen young German women (probably jeunes filles au pair) and one young American. He was a friendly chap and as we laughed, stumbling over our fractured French, I soon learned his name, Donald Brown, and what he was doing in Paris. In addition to being a pleasant classmate, he turned out to be a useful intellectual contact. He was spending the year as a postdoctoral fellow at the Pasteur Institute in the famous laboratory of André Lwoff, Jacques Monod, and François Jacob, where so many Americans and others got their start in molecular genetics. Don let me know when an interesting seminar was coming up at the Pasteur and introduced me to its excellent library, which I frequented a lot during the course of the year. He certainly profited from that year’s experience. Later he did outstanding research on the molecular basis of development at the Department of Embryology of the Carnegie Institute of Washington in Baltimore. Incidentally, I really enjoyed those classes at Alliance Française and found trying to communicate in French at the lab (easier, because when speaking to me, my French colleagues used more proper and simpler French) and in the street (more difficult because of the widespread use of difficult slang) to be great sport, a challenging daily contest.
After we got settled in I looked up Professeur Etienne Wolff at Collège de France, a marvelous old building on la rue des Ecoles, with a statue of Champollion, the discoverer of the Rosetta Stone, in its courtyard. Collège de France is just across la rue Saint Jacques from the Sorbonne. Following the directions of the concierge, I soon found my host, a very formal but gentle and cordial man. He greeted me warmly and gave me a choice of a personal laboratory there at Collège de France in Paris or at the main operation of his Institute of Embryology at the edge of the Bois de Vincennes in Nogent-sur-Marne, a suburb of Paris. Without hesitation, I chose to stay in Paris and quickly moved into a little room of my own at Collège de France. There is nothing special to report about my quarters for I intended to spend the year analyzing data from work done in New Haven and Woods Hole and writing papers. However, I cannot resist mentioning some essential facilities down the hall—les toilettes. There were two bowls, each half concealed by partial doors, and a lavatory. And, it was coed. I frequently met a colleague, female or male, entering or leaving. Coming from puritanical America, it took me a while to get used to it. The French are so grown-up and realistic about the bodily functions. While using these facilities, I could usually detect the approach of a woman by the click-clacking of her high heels out in the hall.

Although I worked in Paris at Collège de France and in the library at the Pasteur I would also visit the laboratory in Nogent-sur-Marne regularly to attend the weekly seminar. This worked out nicely thanks to the hospitality of a colleague in the next lab, Françoise Dieterlen, who had a car and would take me there. Luckily for me, she spoke English fluently and with great patience, she became a wonderful instructor in the French language. She was also an invaluable source of inside information on the Wolff laboratory in particular, and the wider pleasures of Paris. We quickly became friends. The seminars were held in the little library of the laboratory. I'll never forget the first one. We arrived early so I sat down to browse a journal. Suddenly it was quiet and I looked around. Everybody was standing, so I stood up too. Monsieur le Professeur Wolff had just entered and went directly to take his place at the head of the table. Only after he had sat down did the rest of us do so. Then he introduced the topic and the speaker of the day. At the end of the talk he would commence the discussion with a “Maintenant, qui veut prendre la parole?” (“Who would like to begin the discussion?”) (You could also say, “Who would like to speak first.”). I gradually learned that this was old-fashioned French academic protocol where there was always one professor, “le Professeur,” per department. Fred Wilt, a bright, fun-loving American visitor at Nogent for the year, and I were quite amused by the repeated show each week. Seminars at the Pasteur, in contrast, were quite relaxed—American style.

As time passed and I got to know other biologists, like Charles Devillers at the Sorbonne, I learned that Monsieur Wolff was well-known as a remnant of
the old academic formalities and a devotee of correct French. Often when one of
these less conventional colleagues taught me some new slang or popular phrase,
it was followed by a warning, “Mais attention! On ne dit pas ça devant Wolff.”
(“But by no means say that when speaking with Wolff.”)

A lively feature of many a day was lunch with an American student
who had been introduced to me by my old buddy Tom Yost. This young man,
Thomas Benjamin, had just graduated from Amherst College, where he had
been one of Tom’s best students, and, like me, was spending a year in Paris.
Unlike me, he was fluent in French. He would find me in my lab at Collège de
France and off we’d go, buying bread and cheese or saucisson and some lousy
French beer at the marché in the nearby Place Maubert, and then go straight to
our lunch site of the day. The places varied greatly but it usually was one park
or another, like the Jardins du Luxembourg or Place des Vosges (a favorite) or
down by the Seine watching the barges go by. Tom had a very probing
intelligence, full of curiosity. This made for constant, interesting, questioning
conversation. He was a splendid, sympathique companion. Among the many,
many subjects discussed there was one that bothered him a lot. Like many bright
premedical students, he had become fascinated by biology in college and was in
a quandary whether to become a physician or a biologist, particularly a
 geneticist. We discussed the pros and cons and the upshot was his pursuing
graduate studies in genetics at Cal Tech with the great Italian geneticist Renato
Delbecco. Tom eventually landed a faculty position at Harvard Medical School,
where he has carved an outstanding career in virology. Since we both have
houses in Woods Hole, we’ve had the pleasure of each of other’s company every
summer now for many years.

Like many families spending a year abroad, we wished to take
advantage of the proximity of neighboring countries by making a tour. This
became reality one day when Jim Shelburne, a friend and former Yale student in
France, then in the American military, said, “Take my car and go.” We couldn’t
afford to rent one. Thus, we toured Spain, Italy, Germany, the Low Countries,
and, of course, the French countryside. We also spent Christmas in London with
friends from my student days at Johns Hopkins. All of this was fascinating but
for me what stood out was the French countryside—the little villages, the fields,
the churches, the caves, such as Lascaux, with its fabulous upper paleolithic
paintings and engravings, and the rivers. As someone who had grown on the edge
of the ocean, France was my introduction to the spell of rivers—the Seine, the
Loire, the Rhone and many little ones.

In the winter, I bought myself a motor bike, a Peugeot vélomoteur, a
smart little decision. When it wasn’t raining, it was more convenient than the
Metro for short distances and much more fun. In the summer I went on a
camping trip by myself on my little vélomoteur to visit the caves in the Dordogne
and their prehistoric art. The real paintings at Lascaux were still open to the
public (later they were closed and a replica site was produced to prevent further deterioration of the real cave paintings) so I took advantage for a second visit. My God, what a stunning, mind-blowing show that is!

I had never got to Great Britain back in 1940 on my Cramer Fellowship because of the war but now there was no such obstacle and I decided to make a biological tour, i.e., a tour of embryologists, their work and where they worked. My first stop was in London to see Ruth Bellairs and Michael Abercrombie at University College London. Ruth worked on early chick development and had visited with me at both Yale and Woods Hole. She and her husband, Angus, also a biologist, invited me to stay with them, a gracious gesture that I accepted with pleasure. Ruth is a very warm, feisty person whose friendship and collegiality I have enjoyed through the years. I don’t remember much about Angus except that he was a jolly fellow and took me to a charming Edwardian pub, splendid with all its mirrored walls.

I spent a few profitable hours in the laboratory with Michael Abercrombie because his discovery with Joan Heaysman of contact inhibition of cell movement was for me then and since one of the most elegant studies of tissue cell motile behavior. They discovered that fibroblasts (connective tissue cells) cease moving when they contact each other in tissue culture. This finding has great significance for the study of invasive movements of embryonic cells during morphogenesis and carcinogenesis. Abercrombie showed me some of their time-lapse films. I knew from his work and reputation that Michael possessed an outstanding intellect. In addition on meeting with him I discovered to my delight that he was exceedingly kind, considerate, and a modest gentleman. All in all, he was a truly remarkable person. Another treat in visiting Abercrombie was meeting Adam Curtis, who was then a postdoctoral fellow and was to become a stimulating colleague and firm friend in later years.

From London on to Cambridge with its magnificent colleges and a detailed guided tour of them by my host, Sidney Smith, a rotund, bubbling bon vivant, whom I knew because of his dabbling in fish embryology. He was a generous, enthusiastic host, bestowing on me not only his adoration and detailed knowledge of Cambridge but also use of his rooms in Saint Catherine’s College, of which he was, appropriately, the wine steward. I discovered after a night of carousing, that the colleges close at midnight. One of my most memorable escapades in England was being helped to climb over the gate of Saint Catherine’s after hours by some of my recent Yale students with whom I had been carrying on all evening. Sidney’s energetic hospitality even extended to London where he took me to his gentleman’s club and gave me a detailed tour of the National Gallery, explaining with unhesitant certainty the significance of each painting to this uncultured American, as we paused interminably. After we said good-bye at the entrance and he had disappeared, I returned to the Gallery to judge and enjoy the paintings on my own.
My visit to Oxford was mainly scientific and important embryologically, for this is when I first met John Gurdon, who, as a graduate student, was in the beginning phases of his nuclear transplantation work on *Xenopus*. I saw little of the Oxford colleges, having no guide as I had in Cambridge. No matter, at the time I was less interested. I had just come from Cambridge and Oxford, unlike Cambridge, is embedded in a rather ugly city, and it didn’t have the same lure for me. I made up for this by living in New College some years later.

From Oxford I was off to Edinburgh to visit the eminent embryologist C.H. Waddington, Director of the Institute of Animal Genetics. He had arranged for me to stay at a charming rooming house in Edinburgh, took me to a country pub where he introduced me to a perfectly clear scotch whisky, my first, and, of course, showed me around the Institute. Wad had been one of the embryologists chasing the chemical nature of the organizer but by then, like everyone else, he had given up. What turned out to be the main event of this nice trip to Scotland was a cocktail party at the Institute. While in Scotland, I had a yearning to visit the Highlands and, in particular, the Isle of Skye. Mentioning this wish brought quick fulfillment. A man named Ken Jones came forward saying he had a few days on his hands and would be delighted to take me on the trip. The ride to the Isle of Skye was a speedy, scary pub crawl. Ken’s car was a little English sports car, an MG, and he did some pretty tight passing on those curvy Scottish roads. As my hands gripped my seat, I thought I might die in Scotland. My tension was intermittently relieved, however, by frequent stops at country pubs where we aroused a robust “on the house” welcome by quickly revealing our nationalities, he Welsh and I American, and by dropping a few snotty hostile comments about the uppish English. Upon arrival at Skye and full of juice we climbed the mountain at midnight. It was late June and there was plenty of daylight. I remember being greeted by a sea gull sitting at the summit. That was some pub crawl and Ken was just the right companion.

For all my fanciful, optimistic imagining about women in France, nothing had really worked out. Then, one evening early in September something utterly improbable happened. I was attending a banquet in the restaurant of the zoo in the Bois de Vincennes at the end of a colloquium that Monsieur Wolff had organized and was looking around for my assigned place at one of the tables. Couples were separated by design by Madame Wolff. When I finally found my seat and took a fast look around the table I found my eyes lingering on the woman just next to me on the right. She was dark-haired, beautiful, chic, and looked very French! Our eyes met. Thus commenced the most pleasant, most relaxed, most sparkling evening I could remember. She was indeed French, spoke no English and her name was Madeleine—music, magic! We quickly ignored everyone else at the table; I have absolutely no recollection of the person sitting on my left. Madeleine was so beautiful, so vivacious and so
warm. I liked everything about her. And, obviously, she liked me! I know it sounds corny, but I literally basked in her smiles, her laughter, her dark eyes. I felt terrific. We drank a lot of wine and toasted with champagne, our hands entwined. She had lovely hands. Disregarding everyone else at the table, indeed hardly aware of them, we arranged to meet again. She suggested a rendezvous at the famous Drug Store of the Champs Elysees close to the Arc de Triumph at 9 o’clock in the morning a few days later, after I had returned from a meeting in Holland. We were in love. It was a coup de foudre. “Strangers in the Night....” I arrived at the Drug Store a few minutes before nine. It was empty, its void intensified by her absence. Would she show up? I kept checking my watch. She did, a very few, excruciatingly long minutes later. Thus began the rest of my life.

In love in Paris in September (Figure 8.1). Paris never seemed so beautiful. I was living as if my life were a novel. But soon I had to return to the States. Fear not. I returned to France as soon as possible, the very next June. This time I needed wheels for two and so bought myself a fine Italian scooter, a Lambretta. Some friend who seemed to know said it was more durable than the more popular, less expensive Vespa. Anyway, it served me and Madeleine well, both in Paris and in the South of France. And, I had good times driving myself from Paris to the Côte d’Azur and back. Madeleine had mixed feelings about that machine in Paris. It was OK when the traffic was light, but when it was heavy and chaotic, e.g., at l’Etoile (Place Charles de Gaulle), with cars jockeying for advantage by rapid and unpredictable lane changes, she was scared out of her wits. She would cling to me for dear life screaming, “There’s a car to the right, one on the left, another passing in front!” On the more bucolic roads of Provence, however, it was a different story and she hugged me tenderly as we cruised through the vineyards often along the edge of the Mediterranean Sea. Once we went to Venice by train. What a marvel that city is!

Finally, her husband had the wonderful idea to spend a year at the Rockefeller Institute and to bring his family with him to New York. At the end of that year, Madeleine and I both left our spouses and I took her to Woods Hole, with its sea, science, and social life. She had never known any place like it. There isn’t. On arrival, I lifted her in my arms and carried her across the threshold of our little house. Small and unpretentious though it was, she fell in love with it. It was her house, her first American home. My crowd of friends accepted her immediately and quickly introduced her to the easy social scene of Woods Hole. In the midst of this, Mike Bennett, newly wed, arrived with his bride, Ruth. It was a happy occasion not only for Mike but for us as well. It was wonderful meeting Ruth and good luck for Madeleine. For, in addition to her other fine qualities, including her very Latin beauty, Ruth spoke some French. She and Madeleine took to each other like sisters and so they have been to the present day. I’ll write more about our other Woods Hole friends later.
After our difficult divorces were finalized (are they ever easy?), we married. We wished to have a secular ceremony, but with class and elegance. Who could do this for us and where? I immediately thought of Bob Goldburgh, who was the only member of the clergy in my entourage and a person who would certainly compose a beautiful ceremony. The place was the Memorial Room of Branford College, a lovely, intimate, gothic chapel at the base of Yale’s Harkness Tower, a room long popular for marriages. Bob agreed enthusiastically to officiate and to compose a secular sermon just for us. Tom Yost, my best friend, was my best man and Ruth Bennett was Madeleine’s matron of honor. It was October 6, 1963, a gorgeous fall day, the foliage in full color. Our wedding was a beautiful, moving marriage of a French Catholic and an American atheistic, former Protestant, officiated over by a Jewish rabbi in an English gothic chapel in twentieth century America. I have often fancied that our lovely little marriage symbolized in its own way the unity of Western Civilization. At the conclusion of the ceremony, our rabbi saluted all of us in the wedding party with a broad smile and a lusty, “Mazel tov.”

Fate works in strange and unpredictable ways but for us she was certainly smiling. If Madeleine’s husband had not been invited by le Professeur
Wolff to attend the banquet after the colloquium, and if he had not insisted that his decorative wife cut short her vacation in La Ciotat on the Mediterranean near Marseilles to return to Paris and accompany him to the banquet, she and I would certainly never have met.

**Professional Life**

I was in my mid-forties, fifteen years on the Yale faculty, and still an associate professor. Most of my friends and colleagues in my age group at other universities were full professors. Although I had tenure, it was embarrassing for me to have not yet been advanced to a professorship. It offended my sense of personal dignity, my pride. Well, first things first: first our courtship, then our divorces, then our marriage, now the professorship. Since I detected no sign that my department was about to do anything about it, I decided to take the reins myself and immediately after our marriage let various friends know that I was available for an appointment at another important university. In addition, I systematically approached each full professor in our department to tell him the same. Results came rapidly: a professorship at Case-Western University (whose star faculty member in Biology was my old pal, Boris Ephrussi); a membership at the Wistar Institute in Philadelphia, with an adjunct professorship in the Anatomy Department at the University of Pennsylvania across the street; and a professorship at Brooklyn College of the City University of New York. All of this extraterritorial activity stimulated my department and Yale to such a degree that by Christmas I was promoted Professor of Biology at Yale University, effective July 1, 1964. I guess they decided that they would miss me and would not be able to stand the quietude. I wonder how long it would have taken my dear colleagues to get around to promoting me if I hadn’t decided to take the ball and carry it myself? I was very tempted by the Wistar-Pennsylvania offer, but felt in my heart that Yale was, after all, the best for me and so I decided to stay.

Having made a definitive decision to remain at Yale, I felt an obligation to try to improve the place in my own small ways. The first step of this new professor was to see the new Director of Undergraduate Admissions of Yale College appointed by the new President of the University, Kingman Brewster, Jr. I was not very sanguine about how far I would get with the new Director, since I knew only his name: R. Insley Clark Jr. (nicknamed “Inky”), a typically upper-class WASP Yale name, if there ever was one. My proposal to him was to admit students solely on their scholarly and personal merits, regardless of their financial need, so-called “needs-blind admissions.” To my astonishment, he agreed with my proposal immediately and said it would be done. “This is going to cost the University some money,” he added. This young, handsome Yale gentleman and I were henceforth friends, in the same tune on many matters concerning admissions to Yale College (including the famous Jewish question,
recruitment at excellent inner city high schools, etc.). How wrong one can be in stereotyping people. Getting to know Inky was also my introduction to how fine a President Kingman Brewster would be. It was he, after all, who had appointed Inky.

Next I learned that a departmental decision had been made to ask me to replace G. Evelyn Hutchinson, the long time Director of Graduate Studies (DGS) in Biology. Evelyn was a very great scholar and an outstanding ornament of the Department, but he was a terrible DGS, changing nothing as science changed and progressed. I accepted. At the same time as my appointment as DGS, Clem Markert was attracted to Yale to chair our Department. At my request he appointed a blue-ribbon committee chosen by me to revise graduate education in biology at Yale (Hutchinson, Joseph Gall, Charles Sibley and Alvin Novick). We went to work immediately, totally revising and modernizing the graduate curriculum and the requirements for students, something that had not been done within memory, if ever. Apparently, I was effective at this really massive job (which I enjoyed), for we got it done in spite of a need for considerable tedious treading on the private curricular property of some conservative faculty. At the conclusion of our deliberations and our squabbling with various colleagues, I was complimented by the eminent Professor of Ornithology, Charles Sibley. He knew that I was a former bird-watcher. Like many ornithologists, he had nothing but contempt for bird-watchers. To counter his playful scorn, I enjoyed razzing him about some of his feathered friends, e.g., the double-breasted seersucker, the full-breasted dowager, and the hairy-chested nut-scratcher. He did not find this amusing. Hard-nosed, crusty curmudgeon that he was, he nevertheless told me that anytime thereafter whenever he was asked to serve on any important committee he would be delighted to serve if I were the chair. Sibley and some of his colleagues later used molecular techniques to revolutionize (controversially) bird systematics. In the late 1960s, I had a lab near Sibley’s in Kline Biology Tower. We shared a coldroom that was invariably stuffed with all manner of dead wild birds used in Sibley’s molecular studies of bird systematics. The naturalist in me was appalled by this carnage but I didn’t say anything about it to Charles. In my time, I had done in more than my share of birds (chickens).

Big things happened to me in rapid succession after my marriage to Madeleine. First the full professorship, then the appointment as DGS in Biology, then appointment as Master of Branford College and, most important, a rejuvenation of my research. I will discuss the mastership later. Now let's get back to embryology.