CHAPTER 12

EPILOGUE

My First Book

Not many years after publication of the first edition of my book, *Cells into Organs*, it became clear that expansion and progress of the fields of morphogenetic cell movements and tissue cell motility in general demanded a highly revised and substantially enlarged second edition. I embarked on this with some zest for there had been enough new and exciting discoveries on cell movements for me to change my organization. I proceeded by classes of cell movement, with a chapter on individual cells, cell clusters and cell streams, another on cell sheets, another on invagination, involution, etc., rather than proceed organism by organism, as in the first edition of the book. The similarities in the ways cells move in the embryos of organisms that are widely separated phylogenetically seemed to me to be more impressive than the differences, and hence the fundamental mechanisms were conserved during evolution. Conservation of a DNA sequence, protein structure, or particular kind of cell behavior immediately alerts any thoughtful biologist to the importance of the conserved feature.

For example, once animals came up with autonomous locomotion, they slowly evolved into creatures with heads, placed on the leading end of the moving organism. The heads eventually had eyes and chemical sensors and brains with collections of neurons to integrate sensory input from receptors in the head with motor structures in the rest of the body. Instead of passively waiting for food to drift by, or blindly wandering about until they happened to encounter food, animals could now become predators able to find, pursue, capture, kill, and eat other organisms. And, most importantly, they could reproduce, forming similar offspring. Heads, eyes, and brains at the front end of animals were conserved in evolution. Other variants arose, but some of the earliest and most successful autonomously motile predators (probably some kind of primitive worm) had crucial elements of a body plan that persists to this day in highly evolved predators such as lions or *Homo sapiens*. Students of natural history had noticed this basic body plan and knew that it was fundamentally important.

In recent years, molecular biologists have isolated and manipulated *homeobox genes*, the highly conserved genes responsible for assembling this basic body plan. These genes are remarkably similar (conserved) in worms, flies, frogs, mice, and humans. Once Nature discovers a good plan, she holds on to it and improves it. There is one other intersting sidelight here. Homeobox genes were first observed in action by the classical studies of so-called homeotic mutations in fruit flies. Certain mutants had wildly misplaced body parts; antennae where feet should have been. At that time, decades before the discovery that DNA contained the genetic information, the geneticists who observed these remarkable flies knew they had found something important, they just didn't understand what was going on. With this as background, I decided to use more of an evolutionary approach to my second edition of *Cells into Organs*.

Again, I worked on the manuscript mainly in faraway, fanciful places, putting it together in bits and pieces over a span of several years. I wrote during the summers in Woods Hole and in the village of Le Castellet and the vineyards of la Cadière d'Azur in the South of France. I also worked on the book during a number of my academic leaves in several other spots, including my study in the woods at our home in Guilford, Connecticut. Finally, in the last year of production, I finished it off at Yale University. It was a rewarding effort and, because of its considerable expansion, it was really a new book. In the final year, bringing it to completion, I devoted myself to it entirely (with the indispensable aid of my wife), excluding everything else professionally except, of course, teaching my undergraduate course in Developmental Biology. The book finally came out in 1984, 15 years after the first edition, and, if I may say so, many parts of it are still worth perusing. Most of the questions I raised have not yet been answered and most of the projects I proposed have not been attacked or only partially so. This second edition, with its over 500 pages of text and illustrations, is for me a great source of personal pride. It still stands alone as the major comprehensive effort to understand how cells move during the course of embryonic development. In recent years it has even been referred to as a classic.

Some Students

In all those 40 years in the Biology Department at Yale, before I became Emeritus in 1988, I had many students, postdoctoral, graduate, and undergraduate, a number of whom, to their credit and my joy and pride, did excellent research and went on to distinguished careers. Indeed, a good portion of my own reputation internationally rides on the high quality of a number of my students. I have not mentioned some of them before in this volume because their research was not intimately related to my own personal research program or in the context of what I was discussing. Nor have I collaborated in research with any of them since. Two of these students were among my earliest and my best, Norman Wessells and Richard Whittaker.

Norman Wessells graduated from Yale College. I first knew him as an outstanding student in Zoo 23, where he developed his interest in embryology. I also knew him as a resident of Branford College. After spending two years in the Navy, he returned to Yale to do his graduate work with me. He did a fine piece of work on the differentiation of chick embryo epidermal cells *in situ* and in chemically-defined media, but his best research accomplishments came later at Stanford University where he was a postdoctoral fellow with my old friend Clifford Grobstein. There he was soon appointed to the faculty and built a distinguished career for himself. He trained some excellent graduate students, won a prize for excellence in undergraduate teaching, and ultimately became Dean of the Faculty of Arts and Sciences. With his ever blossoming research on branching morphogenesis in vertebrate glandular organs, and his pioneering work on the role of microfilaments in invagination, he became recognized as a leading embryologist, and was elected President of the American Society for Developmental Biology. The last time I saw Norm was about 10 years ago at the banquet of a zebra fish meeting in Eugene, Oregon, where he was Provost of the University of Oregon. He, Bill Ballard, my old friend and an important role model for me, and I cornered a table *behind* the screen so that we wouldn't be disturbed by the after-dinner lecture. We had a pleasant, intimate dinner together, with all three of us ultimately becoming lightly and happily inebriated. Bill, though in his eighties, loved his *vin rouge* and Norm and I had always been drinking buddies. I remember Bill's slight stagger as he left to walk home.

J. Richard Whittaker arrived at Yale after excellent preparation at Queens University in Ontario. Following the already established tradition of independence in my laboratory, he, being an independent cuss, wished to follow his own idea for dissertation research. He had plenty of time to work at it when I was away in Paris (1959-60) but when I returned it was clear that his proposed study hadn't worked out. He reluctantly accepted something I suggested, namely, an analysis of the dedifferentiation and redifferentiation of retinal pigment cells in culture. His discoveries earned him a Ph.D. but soon after, when he left to take a faculty position at UCLA, his real and life-long interest in classical problems of development came forth. He began his beautiful analyses of the relation of determinate cleavage to development in the ascidians (primitive sessile marine protochordates commonly called sea squirts), pursuing his research mainly at marine stations such as Woods Hole, Friday Harbor in Puget Sound and Roscoff. He also followed some of my other footsteps, initially as an assistant and later as a faculty member of the MBL Embryology Course, where he ultimately he became a co-director. He moved from UCLA to the Wistar Institute in Philadelphia early in his career and then directed the Boston University Marine Program at Woods Hole. Now, he is at the University of New Brunswick, happy, I think, to be back home in Canada. In view of his research on ascidian development and his outstanding qualities as a zoologist, particularly of the invertebrates and prochordates, he is now considered a foremost authority on the evolutionary origins of the Phylum Chordata (of which both ascidians and humans are members). Although Dick is often dour on the surface, his dry sense of humor has served him and his friends well through the years. The following Whittakerism was posted in his lab at Yale. "One night when I was sad and lonely and without a friend a voice came to me from out the gloom saying , 'Cheer up! Things could get worse.' So I cheered up and, sure enough, things got worse." He graciously left this treasure with me when he left. I read it back to him years later, when I had the pleasure of introducing him at his MBL Friday Evening Lecture. I am forever indebted to Dick for his regularly appearing gift of the latest collection of essays by the brilliant English essayist and biologist, P.B. Medawar.

Although outshown by the strong personalities of Wessells, Weston and Whittaker, with whom he was a partial contemporary, Robert Hilfer in his quiet, unassuming and retiring way possessed the persistence necessary for the problem I gave him and made some interesting discoveries, leading to the award of his Ph.D. His project was to study the stability of differentiated chick embryonic thyroid cells *in vitro*, an offshoot of my then interest in tissue cell stability. I mention Robert here for the distinguished career he has subsequently built for himself after he left my lab and Yale. Apparently I did something right in his training. He joined the faculty of Temple University in Philadelphia, where he has had a number of fine students and with them has done truly outstanding work on (you guessed it) the development of the thyroid gland and other related problems. Growing up in science is a complicated business. I have had some students who were bright lights as graduate students and then afterward virtually disappeared. Others have been slow developers. Robert was one of these to his and my great satisfaction and, incidentally, it has done wonders for our friendship.

Kurt Johnson was another of my students who found a thesis problem for himself. He came to Yale at the suggestion of Malcolm Steinberg, then one of Kurt's mentors at Hopkins. He found his project by a careful literature search, after I told him that amphibian gastrulation was ripe for reinvestigation with modern new tools. He started working on something or other in my lab, mostly making messes, as soon as he came to Yale. He also started on antagonizing many of my faculty colleagues. On the day of his oral dissertation prospectus defense, I was surprised to see professors who were not only the expected developmental biologists, but also ecologists and even plant physiologists, for the love of Mike. I wondered innocently why there was such a crowd. His proposal was basically sound. In this case, the student's desire for independence in the selection of a project was successful. Thereby hangs a tale neither of us will ever forget. Several professors, whom Kurt had disrespected in classes and seminars, came to his defense prepared to rake him over the coals. He helped them by being unprepared. My colleagues made mince meat out of him. He later told me that he was sweating hard five minutes into a session that lasted some hours. Then we asked him to leave the room and wait in my office. Many of my colleagues wanted to throw him out then and there, but after an hour or so of at times heated debate, I persuaded them to give him another six months to show that he deserved to stay at Yale. I had to call in some markers that day. The examination had started at 2:00 P.M. and came to its awful conclusion well after 5:00 P.M. I marched down to my office and blistered Kurt. I was angry and embarrassed by his poor performance and told him that he had six months to produce some serious work or he was out. Then I told him that I was going on academic leave for the next six months and that we would talk when I got back. He buckled down quickly, writing me long letters describing his results while I was away. It was quickly clear to me that this near disaster had lit a flame under him, but I decided to let him sweat a little more. On my return, we sat down in my office and Kurt was prepared with a two-foot high stack of results. It was afternoon and, in spite of my interest, I was drowsy and soon nodded off. I was jolted back awake when Kurt said, "Jesus Christ Trink, I'm standing here talking as fast as I can trying to save my life and you are asleep. The least you could do is tell me that I am done at Yale and let me get the hell out of here."

"What are you talking about so angrily?" I said.

"You told me that I had six months to prove myself! Am I in or out?"

"In, of course, I thought you knew that already."

"No, I didn't, but thanks, I guess!"

Greatly relieved, he gathered his dog and pony show and left my office. You see, I had neglected to tell *him* that his letters and results, sent to me in France, had long-since satisfied me that he had come around. Well, anyway, the end result was a fine dissertation and a well-earned Ph.D. He has now for many years been Professor of Anatomy and Cell Biology at the George Washington University. Kurt and Albert Harris were contemporaries, both very bright, articulate, and a little crazy. They were a constant source of entertainment around the lab and sometimes a bit upsetting for guest speakers.

Of the two Union College students I mentioned earlier, Gary Conrad preferred Salisbury Cove in Maine to Woods Hole for the summer (and still does) but nevertheless had the good sense to choose me as his mentor. I suggested a tough problem: to study the synthesis of the connective tissue elements, collagen and glycosaminoglycans, in the developing cornea of the eye of the chick embryo and also in clones of cornea cells *in vitro*. With the steadiness and capacity for hard work I learned to appreciate from this young man of well-organized intelligence, Gary learned a great deal about these syntheses. His dissertation work was published in two papers in *Developmental Biology*, the most rigorous and prestigious journal of the field of that time. He has spent his entire academic life in far away Manhattan, Kansas at Kansas State University (where he is now a "Distinguished Professor"). He married a graduate student colleague, Abigail Hooper, and together they prospered, raising two fine children. They chose to live in Manhattan, Kansas because they saw it as a good place to raise a family, but I suspect they would have been just as good at raising their family if they had lived in Manhattan, New York. We have had the delight of seeing him and his family on many occasions since: at KSU, Salisbury Cove, Guilford (his daughter went to Yale College) and even Paris and Uppsala. Like me, he took full advantage of the freedom for research and the change of atmosphere provided by the American university system of sabbatical leave and took them all, living and working happily in places like Sweden and Paris for a full year.

When Ray Keller was in the lab we saw a lot of *Xenopus* embryos and it occurred to someone, Ray or me, I suppose, to take advantage of the strikingly transparent dorsal fin of the *Xenopus* larva for a study of cell movement. So I suggested to Gary Radice, a new graduate student (a Danforth Fellow no less), that he study epidermal wound closure in this exceptional material. The result was an elegant and, at the time, definitive study of the spreading and contacts of epidermal cells as they closed a wound in the skin of the *Xenopus* larval fin both *in vivo* and *in vitro*. Cells at the margin of the wound begin migrating within seconds after wounding, by extending broad lamellipodia across the wound surface. Because these cells showed contact inhibition of movement, their lamellipodia would stop moving when they met lamellipodia coming from the other side of the wound. Gary's study demonstrated how normally stationary cells can engage quickly in rapid motile activity that would never had occurred had the skin not been wounded. Thus, in intimate cellular detail he presented a graphic picture of the quick restorative regenerative powers of a vertebrate organism. The result was an exceptionally beautiful dissertation.

Around 1980, John Kolega, a reserved, very together young man arrived with an outstanding undergraduate record at the Massachusetts Institute of Technology and decided to work with me. It took a while to get to know him personally because of his shyness but I knew well his strong intellectual capacities from his MIT background. I had just the problem for him: explain how small groups or clusters of cells move and do so directionally. We had long known that cell clusters move directionally during morphogenesis, as in the migrations of the neural crest, but no one had analyzed these movements. John really sank his teeth into this problem, using a fish melanoma whose cells separate in tissue culture to form small independently motile clusters that tend to move long distances in the same direction. He soon learned that these clusters vary greatly in speed of movement, depending on the distribution of their broad leading lamellae. As these lamellae pull the cluster forward, the cells at the trailing edge are pulled taut under the tension thus created and in consequence form no lamellae, no opposing pulling force. This observation is really quite neat, for it explains the persistence of movement in a particular direction once it has begun. It is self-reinforcing; no exogenous attracting forces are necessary and there is no evidence for any. John had a smart analytical mind. Since mechanical tension clearly prevents the formation of spreading lamellae, he went on to study the structural basis for this by analyzing the role of microfilaments and intermediate filaments in the cytoplasm of moving clusters. The result was in effect the production of two Ph.D. dissertations. Quite a feat! Since they were published 5 years apart, I suspect that John found much to like about Yale and found it hard to leave. MIT would find it interesting to learn that one of its products is having a fine time teaching gross anatomy in a medical school. He just loves it.

My last graduate student, Charles Ettensohn, just dropped in the other day, taking a couple of hours off from his chores as a member of the faculty of the MBL Embryology Course, where he handles sea urchin development. As he was leaving, he guessed, correctly, that he was my last student. I added under my breath "...and one of the best." He came to Yale from the University of Illinois with a highly competitive National Science Foundation Fellowship in his pocket. Ray Keller also came from Illinois. That great university has been good to me! Chuck's wish was to work on the neural crest. I agreed, with hesitation, and soon we had a charming colony of quail in the lab. You see, quails and chickens will easily accept transplantations between their embryos, but the cells of each have a distinctive nuclear morphology, allowing certain identification of cells originating in transplants of quail neural crests into chicken hosts. Nicole Le Douarin, a student of Wolff when I was in Paris, made this wonderful discovery. Since then, Nicole has become, I would say, one of the most well-respected embryologists working in Europe, particularly for her excellent research on the neural crest. But again the student's research idea didn't work out and, convinced that it was my fault due to my incompetence with the neural crest, I suggested that Chuck transfer to the University of Oregon and work under Weston, as Carol Erickson did as a postdoctoral fellow. But no, he preferred to remain at Yale, even though it presented him with a wrenching alternative. Get rid of his pet quails. So I offered him another problem—explain the clear, obvious, beautiful, much-worked-on invagination of the vegetal plate during sea urchin gastrulation. This got him hooked on early sea urchin development and his highly original research at Yale and especially at Duke University in the lab of David McClay, one of Moscona's protégés, and later at Carnegie-Mellon University has made him one of the stars of the field.

I have of course had other graduate students throughout the years, who performed satisfactorily, some of them writing excellent Ph.D. dissertations. But

I have since lost track of them. Some left science to practice medicine and are certainly excellent physicians. One got completely lost in the technology of science. Two others simply quit research. Two did not even publish their Ph.D. research, which is inexcusable. So be it. We can't win them all. I am exceedingly fortunate and deeply pleased that most of my students went on to carve distinguished careers in science and teaching for themselves and as a gift have provided me with many excellent scientific grandchildren and by now great-grandchildren as well. Some of them refer to me as "The Godfather." Around the lab, some of my students referred to me as The Chief. This is a bit over the top, because Professor Ross Granville Harrison always was and always will be The Chief. Nevertheless, I am honored by these titles, whether or not they are actually deserved. And before I spend too much time tooting my own horn, I recall that once Albert Harris told me that he and others working in my lab were reluctant to have me describe their results to others—at meetings I would attend but they did not—because they were afraid that I would get their results wrong!

Choosing a System and Family Trees

In pursuit of research on how multicellular organisms develop, it is often fruitful to stick with the organism or the system one began with, not only because it is familiar, but, more important, because it is amenable to research on the concepts of interest or the questions being asked. John Saunders did this with the limb bud of the chick embryo, Ray Keller with gastrulation in *Xenopus*, JimWeston with chick neural crest, Chuck Ettensohn with sea urchins, Albert Harris and Wen-Tien Chen with tissue cells in culture, Bob Hilfer with the thyroid, Gary Conrad with the chick cornea, and so on. But in other cases, with the maturity that comes with the completion of graduate studies, it is sometimes better to ask oneself once again whether this thesis material and this problem has a future for me, given my innate abilities and interests. Thus, I shifted from chick feather pigment patterns to gastrulation in *Fundulus*. Norm Wessells to salivary gland morphogenesis and the role of microfilaments, Dick Whittaker to "determinate" development in ascidians, Carol Erickson to neural crest, Cheryll Tickle to chick limb development, and Rachel Fink from sea urchins to *Fundulus*—all with profit. Sustained effort is required to get answers to important questions, as everyone knows, but a little tinkering with other material from time-to-time can be both refreshing and scientifically revealing, as I have found with fibroblasts in culture and with *Blennius* and Albert Harris has found with sponges. In any case, the guiding principle for serious research should always be the problem or concept involved, not the organism, whether one is an embryologist or ornithologist.

When I was younger some people interested in my research on *Fundulus* would wonder with whom I took my doctorate. On responding I occasionally found myself spicing the answer with a recitation of my impressive scientific lineage. Like many, if I stand taller, it is not only because I stand on the toes of my competitors but principally because I stand on the shoulders of giants, my renowned scientific ancestors. I took my degree with B.H. Willier (1890 - 1972) at Johns Hopkins. Willier had taken his degree with F.R. Lillie (1870-1947) at Chicago. Lillie had taken his degree with C.O. Whitman (1842-1910), also at Chicago. Whitman had gone to Germany for his studies, where he took his degree under K.G.F.R. Leuckart (1822-1898), the great parasitologist, at Leipzig. Leuckart took his degree at Göttingen with R. Wagner (1805-1864), who did his studies in Paris with none other than Baron Georges Cuvier (1769-1832), the great French zoologist and paleontologist (see Appendix II). Once when lecturing at the *Faculté des Sciences* in Paris (in my lousy but enthusiastic French), I regaled the audience in my introduction with this lineage. I started with myself at the bottom of the blackboard and ending with Cuvier at the top, then hamming it up with, "*Vous voyez, nous sommes tous descendus de Cuvier.*" (pause). "*Vive la France!*" Vigorous applause. They loved it. I could also play this lineage game with my professor in college, H.B. Goodrich, who was a student of E.B. Wilson at Columbia, who took his degree from W.K. Brooks at Johns Hopkins, and Brooks from Louis Agassiz at Harvard. The great Agassiz, of course, was Swiss but took his degree at the University of Erlangen in Germany.

Children

While on the subject of ancestry, this is perhaps a good place to bring you up to date on what had happened to my own children. Yes, I have fully grown-up children. Those three kids I read stories to, went swimming, bird watching, and boating with and took to Paris so long ago are now in their fifties. They are all fine people with many good friends. Each had an excellent college education at the University of Wisconsin (Madison) and is now consumed with and successful at his or her profession. Gregor, the oldest, works for the Massachusetts Board of Library Commission as a librarian and preservation specialist. Tanya, the middle one, went to the Rhode Island School of Design after Madison and is a painter. She exhibits her work frequently. Erik, the youngest, is a paleoanthropologist, specializing in the Neandertals. He went to graduate school at the University of Pennsylvania and now is on the faculty at Washington University in Saint Louis. Each of my children is happily married and we enjoy the company of all three couples, and their children: Jennifer and Christopher of Gregor, Alex and Trina of Tanya, and Sasha of Erik. My children and their children are all healthy, bright, handsome, and affectionate. What a lot of good fortune they and I have had. None of them has had any serious diseases, accidents, or devastating personal problems. Not all parents have been so lucky.

Anne-Laure, Madeleine's daughter, also a very fine person, is likewise married well and has three young children, all boys, our French grandchildren: Clément, Victor, and Antoine. We recently had a joyful, 10-day visit from her entire family in our (it turned out not so) big Woods Hole home. Anne-Laure is a *Chef de Cabine* for Air France and can easily take or send her family everywhere. She often sends segments of her family to visit us.

Grant Support

It occurs to me that I have not explained where the financial support came from for my research and that of my students and postdoctoral fellows. In the beginning, in the early 1950s, it came from the then newly founded National Science Foundation (NSF), a magnificent step forward for American science. My first grants were small, appropriate for a small operator. However, as my laboratory grew a little I needed more support and was advised to apply to the National Institutes of Health (NIH). For a while I had grants from both agencies but then the NSF informed me that I should shift entirely to the NIH, which I did. The National Cancer Institute has been funding me ever since, including a MERIT award in 1987, which supported me for 9 years. I was naturally very pleased and honored by this recognition for it came from the Cell Biology Study Section, a tough reviewing committee of colleagues in the field. They not only thought highly of my grant proposal and my ability to carry through on it but laid the money on the table. I was really pleased to receive a Merit Award, which supported me until I was 77, when the NIH finally turned me down, for the first time. Then the NSF took me on again with a baby grant for three years. Like everybody else, I hated the intensive, often exhausting chore of applying every 3 or 5 years (except during the MERIT Award). But I was always funded, except, of course, when Nixon stopped all grants temporarily in the '70s. The competition for grants is basically a good system, with its peer review. Also, it makes one think more incisively and critically about one's research. So, I have nothing to complain about. Indeed, I didn't even have to pay for it by serving on a study section. I don't know why. Perhaps stupidly because of my left-wing political background, or perhaps because the powers that be thought little of my judgement. I never looked into it. "*Ne réveille pas le chat qui dort.*" Whatever the reason, I am fortunate to have received this crucial support, without which my life's work would not have been possible.

I am also thankful for not to having been asked to serve in any major administrative capacity during my entire academic career other than the very enjoyable part-time years as Director of Graduate Studies in Biology (one year) and as Master of Branford College (seven years). I also was the organizer of one Gordon Conference (on *Cell Contact and Movement*), which was also a pleasure. I have never been nominated to be President of the American Society for

Development Biology or any other society. Best of all, I was never asked to Chair the Yale Department of Biology. I guess they knew I didn't want it and perhaps they didn't trust me. It seems, in retrospect, that I have been very lucky. I have been largely left alone during almost my entire professional life to savor my work as a biologist and professor, fulfilling surprisingly well the naive, romantic daydreams of my teenage years.

In my forty years as an active member of the Yale Biology Department and my fourteen years since, as Professor Emeritus, I have had so many colleagues that I momentarily forget some of them. I knew many well and worked well with them, but only a very small number stand out as life-long friends. Nevertheless, I should be grateful for this much, for as far as I know, I have had no enemies. Bitter enemies sometimes share the same department. Nor has our department been beset by the acrimonious splits and cliques that have plagued some departments (except recently when evolutionary biology and ecology split away, to my regret). This intramural squabbling can be very distracting and distressing, even depressing.

Local 34

Since those intense political years of my youth and mid-life, my overt political activity has been sporadic. Indeed, I thought it was probably over when we left Branford College and my intellectual life became more and more dedicated only to science. However, the political animal in me reawakened briefly in the mid-1980s, when the Yale clerical and technical workers formed a union, Local 34. Because the administration refused to meet the worker's demands, mainly for increases in their poverty-level salaries, they went on strike—an event that gained national attention, it being at Yale and because the union members were mainly women. For the faculty, the strike struck home, as it were. These were our technicians, our secretaries, our librarians, and many of them our friends. Many faculty members supported their just demands. We came mainly from the Law School, the social sciences and the humanities, some from the sciences but fewer (not surprisingly) from the Medical School. We joined their picket lines, formed our own picket line once a week at Woodbridge Hall (the President's office), petitioned the administration and so on. Many faculty members moved their classes off campus. I gave the lectures to my class in Developmental Biology in the basement of one of the churches facing the New Haven Green. Along with this support I was asked to give a few speeches, this being one of my talents. One was a sermon of revival at a rally of the Union in Center Church, one of the fine old churches on the New Haven Green. My Uncle Henry, the Methodist minister, would have been proud to find me speaking from that pulpit. I certainly was proud to be there, addressing those good people. Because of my visibility, many union members came to call me "our professor." Well, the union finally won, after months of a bitter strike, from a stubborn and, in my opinion, stupid administration. A. Bartlett Giamatti, a friend and fellow former master, was then President Giametti and in many respects an excellent one. I never understood how he could be so dumb and unfeeling about the union and its demands. There were also amusing aspects. For many years after the strike I profited from a small but delightful fringe benefit. Union members in varied places campus-wide, like the Library, the Treasurer's Office, the Parking Office, etc. greeted me warmly by name and were especially happy to take care of whatever need I was there for. Incidentally, my old friend and political comrade, Clem Markert, did not support the union. This cooled our relationship considerably. I now regret that I never discussed the matter with him. However, his lack of support was consistent with his increasing personal arrogance and his status as a sort of upper-class scientific "statesman."

At tense times like that, terms like conservative, liberal and radical take on special political significance and are bandied about loosely, often without really telling you much about the person or taking into account the subject, the context or the milieu. I don't like these designations as applied to a person. "She's a liberal. He's a conservative." Let me cite an example. Socialized medicine is believed by many Americans to be a liberal even radical idea. But in Europe, where the practice of medicine has been socialized for many years, there is nothing radical about it. It is happily accepted as an essential feature of democratic life in an advanced, civilized society. Moreover, I know from personal and family experience in France, the one European country with which I am intimately familiar, that socialized medicine works extremely well. I therefore find my position as a strong supporter of socialized medicine for every American to be very reasonable, as well as compassionate, and not particularly liberal or radical at all; indeed, one might say "conservative." You don't have to be a socialist or a communist to understand the major reason why the US is so backward in this regard—the heated opposition of the capitalist health industry and its control over much of the media and many members of our government. Another area in which I might be considered conservative is my strong support for conservation of our national resources—our forests, oceans, the beauty of the countryside. Here again the main enemies are big industries like lumbering, mining, fishing, and their acolytes in the government. In other areas, I would be considered liberal or radical. For example, like many compassionate people, I consider it the obligation of responsible government to take care of people who cannot take care of themselves, principally children, the mentally ill and handicapped, and the very elderly. One of the wonderful features of France is the state-run *Ecoles maternelles* for little children. Here again, incidentally, Europe is well ahead of us. So what am I politically? I am conservative, liberal or radical, depending on the area of dispute, as indeed are very many people.

This brings me to another big controversial matter. It arose on a gorgeous California day in a gorgeous setting, the elegant house of Cliff and Ruth Grobstein in Rancho Santa Fe, a posh suburb of La Jolla. We were at the wedding reception of one of Ruth's daughters, and I found myself in conversation with a couple from Woodbridge, Connecticut. They were "nice" people—white, educated, middle-aged and affluent. I forget how the conversation began but at some point they had reason to ask me "Aren't you proud to be an American?" My answer was clearly stated, "No. What do I have to be proud of?" Their eyes opened; they were shocked. Then, I went on, saying I was born American by chance. That's nothing to be proud of. There are wonderful aspects, like some of the countryside, our Bill of Rights and some of our great universities. I enjoy working at one of them. But I didn't found them and barely fashioned their history. There are also awful aspects, like the abject poverty of many people in the midst of fabulous wealth, or the treatment of homeless people, many of whom are schizophrenic, for no fault of their own. I am an American, of course, by birth, speech and in a number of cultural traits, but that is just because I have lived and worked here most of my life. This gives me no reason to be proud and patriotic, however, and I am not. Besides, I am a scientist and for this reason, as well as others, I am deeply international in spirit. We scientists admire or disapprove of the research of others utterly regardless of their nationality. A beautiful experiment is a beautiful experiment by whomever and wherever. Being international has added great richness to my life, enjoying to the best of my ability the best of whatever country I am in, including the United States of America.

"Retirement"

I became professor emeritus in 1988 at the age of 70, as obliged by law. This meant retirement from teaching and all other academic responsibilities like committee work. The advent of my becoming emeritus was celebrated in two events, one in the magnificent dining hall of Branford College, principally for Yale colleagues, former students, members of Local 34, and friends and family. All three of my brothers and their wives attended. Even Madeleine's daughter, Anne-Laure, came from Paris. It was good for everybody to see everybody. Like weddings and funerals, retirements serve an excellent communal function.

The other event was a much more elaborate—a two-and-one-half day international symposium on *Cell Movement and Morphogenesis* at the MBL in Woods Hole (see Appendix III) with a research lecture by me and a banquet with speeches. Almost all my former graduate students and postdoctoral fellows were there plus a good sprinkling of former undergraduates, from both Biology and Branford College. In addition, there was a considerable number of colleagues and friends from all over the States, even Iowa and California, and from the UK, France, Israel, Austria, and Sweden. They called it "Trinkfest '88" and issued everyone a button emblazoned with my picture at the tiller of our boat, my resolute jaw jutting forward (**Figure 12.1**).

By neat planning, Trinkfest '88 was built around my 70th birthday, May 23, which occurred right in the middle of the symposium. By incredible coincidence, the symposium itself was part of the celebration of the 100th Anniversary of the Marine Biological Laboratory. It was a wonderful occasion

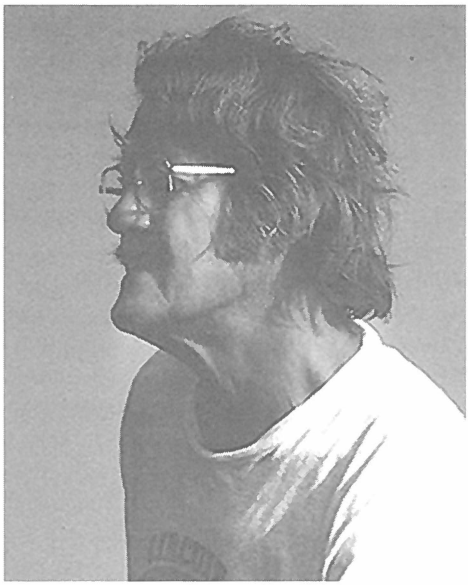

Figure 12.1. A photograph of the author at the tiller of his boat, used to make a commemorative button for Trinkfest '88.

—serious science and the gay camaraderie of old friends getting together. Even the weather was fine and some of the speeches at the banquet had a light touch. Norm Wessells, the only dean among us, was the roastmaster. Jim Weston was in full flower, playing with the title of my book. John Saunders was his gracious, generous self. Dick Whittaker had some sharp things to say about my sharp wit. Sandra Mayerson, a dear friend from the class of 1973 of Branford College, stole the show with her tales of what a naughty boy I was as Master Trinkaus. Even my Yale colleague, Joel Rosenbaum, bestowed a warm accolade, thanking me for never having bored him. My children were there and our lovely granddaughter Trina sat with Madeleine and me. Well, it was a grand occasion and we all had a good time. Afterward, everyone came out to our house to continue the party. Cheryll Tickle in London couldn't come but sent me a sawed off mammoth molar by way of Graham Dunn. Adam Curtis from Glasgow brought me a model of the Loch Ness monster. Carl-Olaf Jacobson from Uppsala brought a seal of the University of Uppsala and a record of their singing group. Bathed in all this honor, love and gaiety, I relished every minute of it. A normally sober, careful cell biologist from the Yale Medical School, Gabriel Godman, bumped into me in the drug store in Woods Hole and said to me incredulously, "You know, they really love you!" I imagine that this is true, except, of course, for those who didn't want to show up.

I learned afterward that Carol Erickson secretly organized the celebration, with the equally clandestine help of various interested parties like Madeleine, Jim Weston, Albert Harris, and Rachel Fink. Carol, it turned out, was stuck in bed in her home in Davis, California for the last months of a delicate pregnancy and thus able to devote herself almost entirely to arranging everything. Dear Carol. It's so sad that she missed the fun. But soon after the party she received her reward, a beautiful (and more importantly, healthy) baby girl named Julianne Caitlin.

Retirement was not a traumatic event for me, mainly because I didn't bother to retire. Since I no longer had graduate students or postdoctoral fellows I needed less space. However, our department has a tradition of providing small space for its emeriti and I was adequately taken care of. This has been exceptional good fortune. In some departments, emeriti are often simply thrown out. Intellectually, retirement has been quite easy. Because I had never left the bench in my research, I simply turned the part-time research of a professor into full-time research. For colleagues, who had left the bench a long time ago to run big labs, retirement could pose a problem. Not for me. I actually was quite productive and did some good research in my seventies. Indeed, I consider my papers on convergence (with Madeleine and Rachel Fink) and on the yolk syncytial layer to be among the best of my career. I also got into an exhilarating controversy with Chuck Kimmel over involution versus ingression in the zebra fish and *Fundulus*. We both won and have agreed that there is marginal ingression in zebra fish and submarginal ingression in *Fundulus*. In addition, I have had fine collegial relations with a number of zebra fish people, especially young people. Oh yes, everybody is now "young" compared to me.

I have often been asked, "Don't you miss the teaching?" Yes, I miss the freshness of the undergraduate students and my orating before them. But I am glad to be rid of preparing lectures, making up exams, and reading term papers. Thus, in sum, I do not miss teaching. To make up for the lecturing I have lectured a fair amount on our research at various meetings and universities until recently.

One of the rewarding features of a life in science is that, like all work, if you are any good at it, you gain the respect of the people you respect. I have had that respect, I believe, and it has had the usual beneficial effect of adding to my self-esteem—and confidence, my sense of well-being. It has also enhanced my personal and scientific relations with other biologists. This is basically all that one should need in the way of honors. If there is some overt recognition beyond this, it is fine and welcome, but basically icing on the cake. As I have already mentioned, I was honored by being granted a MERIT Award by the NIH and greatly moved by the grand tribute of the Trinkfest '88 symposium, people and all together. Then, to top it all, I was awarded the Edwin Grant Conklin Medal by the American Society for Developmental Biology in 1995. It was a new award to "a developmental biologist who has made and is continuing to make outstanding contributions to the field". I was the first recipient. I had never heard of the award but certainly knew and admired Conklin's work. So it was a wonderful surprise. To my great pleasure, the very next year the medal was awarded to John Saunders. Following him, the recipients have been, in order, Elizabeth Hay, Thomas Kaufman, Clement Markert, Charles Kimmel, John Gurdon, and Gail Martin. I find myself in very distinguished company. I have always had an uneasy feeling about prizes to individual scientists because one of the beauties of science is its collectivity. Yet, on the other hand, some individuals do play a leading role in the international collective. Anyway, I accepted. This award was particularly pleasing because it represented the opinion of other embryologists who knew professionally about my career and our research. Taking in account all I have written in this paragraph it seems that I have been generously honored.

Certain friends have expressed surprise or regret during the last 25 years that I have not been elected to membership in the National Academy of Sciences (NAS). This is a complicated business. Most members of the Academy certainly merit their membership on scientific grounds and we all know a few who do not. Most of us are pleased when a deserving colleague is elected and very happy if he or she is a friend. And, we all know many who merit membership but who for some reason have not been elected. My esteemed Yale colleague, Sidney Altman, was not elected until *after* he had won a Nobel Prize. Honestly, my own lack of membership has not bothered me, for, as I have written, I have for a very long time enjoyed the respect of those whom I respect and I have always enjoyed doing science for its own sake. In addition, I have less of an ego problem than some others. It could be that I'm not important enough for the Academy. Whatever the reason, I actually like myself as I am and have been and do not intend to change. Exclusion from the NAS is a bitter pill for some fine scientists, but not for all of us.

Old Age

I have been an old man for quite a number of years now so perhaps I owe the reader a brief progress report on how things have gone. I retained the good health and physical vigor that had been my good fortune all my life to a considerable degree up to the age of 74. On the basis of this we made reservations to return to the Club Med in 1992 for more diving. But we had to cancel at the last minute because of a cardiac accident, a leaky mitral valve. That was taken care of quickly but I decided that this meant the end of diving. During the next several years nothing life-threatening happened. I had a severe case of trigeminal shingles, herpes zoster, a very painful malady and I still have it, but only sporadically. I live with it as most elderly people do with all the various inconveniences imposed by an aging organism. Old age is not for sissies. At the time that all this happened, I was heavily engaged in research, studying ingression of deep cells during early *Fundulus* gastrulation and the role of microtubules during formation of the yolk syncytial layer and in epiboly with a former student of Ray Keller. Then, at the age of 79, the shit hit the fan. I was dangerously smitten—kidney failure, followed by a bleeding gastroduodenal ulcer, apnea, inability to swallow, one after the other. All this required four months of incarceration in the Yale-New Haven Hospital, often in intensive care, during which I was close to death a few times. It was a terrible ordeal for my poor Madeleine, who visited me every day, often not knowing for periods if she would ever see me again. For me it was actually less of an ordeal. I did not suffer pain and was out of touch a great deal of the time because of persistent hallucinations. The upshot was that I miraculously survived. I count it as due to some excellent medical care, much spontaneous healing, the love of my wife and the devotion of some dear friends (particularly of some alumni of Branford College), and my personal purity of spirit. I was of course considerably weakened physically and with a tremor after all that travail but with some of my usual good luck, my intellect remained intact and my hands regained their steadiness. I can still operate on fish embryos, returned to *Fundulus* at Woods Hole and to *Blennius* at Roscoff, and composed and published a couple more papers and still go swimming at Stony Beach. And now, I have written this book. Given all that has happened and could have happened I have experienced a fruitful old age, so far.

It must be clear to the reader that, by and large, I have lived a rich and happy life and still do, even at my advanced age. Although there are many reasons for this, including my optimistic temperament and ebullient personality, the most important reasons for my continuing success at mastering the subtle art of living have been the constant loving presence and intelligence of my wife, Madeleine, and my work, my fulfilling research, writing, and teaching. This is how it has been since we began building our lives together (**Figure 12.2**). Somehow, I was wise and fortunate enough back then to be able to organize my life according to a plan that has worked marvelously well. Number one in importance has been my wife (and then some close friends and family, including our cats, by the way). Number two has been my work. All other things that we enjoy, and there are many, are tertiary in importance. If something goes wrong with number one, the roof falls in. Nothing else matters. If something goes wrong

Figure 12.2. The author and Madeleine, his wife, after returning from a vacation in Tahiti, circa 1983.

with number two, it is upsetting and requires serious attention. All tertiary activities, though important, are readily adjustable or dispensable. I have not been confused. I know my priorities. A big help in carrying out this master plan has been my unquenchable optimism. I don't know or understand how I came by this optimism, but I have certainly been optimistic most of my life. In large measure, I have been lucky, to be born in a part of the world where one has enough time left over to contemplate Nature rather than spending every moment just surviving it. After all, water, food, housing, considerable personal freedom, and adequate domestic tranquility were just there for me, unlike most of the other inhabitants of our planet. I was lucky to have had parents who nurtured me enough so that I could succeed. Many people are not so fortunate. I have been lucky to have received a good enough early education and fortunate that I could leverage my early experiences to receive a fine education at some of the world's truly great learning environments. I sought out and found inspiring mentors, colleagues, and protégés. I have also had many supportive friends, partly by luck and partly by working at being a friend. I think my good sense of humor has been important in my optimistic nature. I succeeded at many different things I tried to accomplish and so moved on to greater challenges. I grew up with an inquisitive spirit and was usually rewarded handsomely for my endeavors. My optimism and success have not been entirely the result of luck. I have also worked hard and effectively at times. I have also followed religiously the adage, "All work and no play makes Trink a dull boy." But surely, good fortune played a role as my life unfolded. I at least had the good sense to answer the door when opportunity knocked. I try to be intelligent and realistic about life and try to keep my head up and eyes open. My optimism has been a huge lift for me and for Madeleine and has certainly made me a more positive and pleasant personality. If you are pessimistic, you might not try. If you are optimistic, you try.