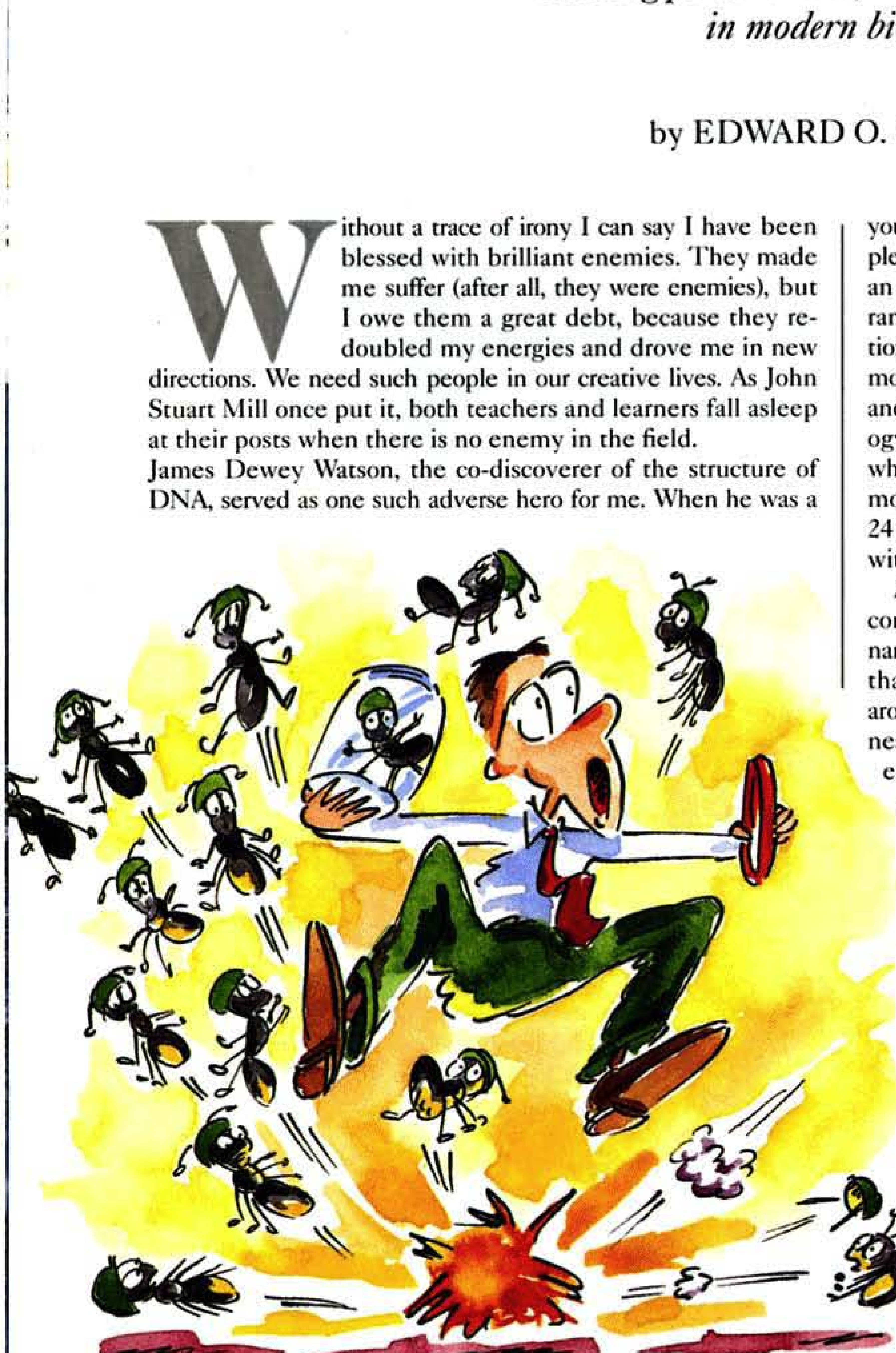
THE MOLECULAR WARS

Changing paradigms, clashing personalities, and the revolution in modern biology.

by EDWARD O. WILSON

Without a trace of irony I can say I have been blessed with brilliant enemies. They made me suffer (after all, they were enemies), but I owe them a great debt, because they redoubled my energies and drove me in new directions. We need such people in our creative lives. As John Stuart Mill once put it, both teachers and learners fall asleep at their posts when there is no enemy in the field.

James Dewey Watson, the co-discoverer of the structure of DNA, served as one such adverse hero for me. When he was a young man, in the 1950s and 1960s, I found him the most unpleasant human being I had ever met. He came to Harvard as an assistant professor in 1956, also my first year at the same rank. At 28, he was only a year older. He arrived with a conviction that biology must be transformed into a science directed at molecules and cells and rewritten in the language of physics and chemistry. What had gone before, "traditional" biol ogy—*my* biology—was infested by stamp collectors who lacked the wit to transform their subject into a modern science. He treated most of the other 24 members of the Department of Biology with a revolutionary's fervent disrespect.



At department meetings Watson radiated contempt in all directions. He shunned ordinary courtesy and polite conversation, evidently in the belief that they would only encourage the traditionalists to stay around. His bad manners were tolerated because of the greatness of the discovery he had made, and because of its gathering aftermath. In the 1950s and 1960s the molecular revolution had begun to run through biology like a flash flood. Watson, having risen to historic fame at an early age, became the Caligula of biology. He was given license to say anything that came to his mind and expect to be taken seriously. And unfortunately, he did so, with a casual and brutal offhandedness. In his own mind apparently he was *Honest Jim*, as he later called himself in the manuscript title of his memoir of the discovery—before changing it to *The Double Helix*. Few dared call him openly to account.

Watson's attitude was particularly painful for me. One day at a department meeting I naively chose to argue that the department needed more young evolutionary biologists, for balance. At least we should double the number from one (me) to two. I informed the listening professors that Frederick Smith, an innovative and promising population ecologist, had recently been recruited from the University of Michigan by Harvard's Graduate School of Design. I outlined Smith's merits and stressed

Illustrations by Chris Demarest

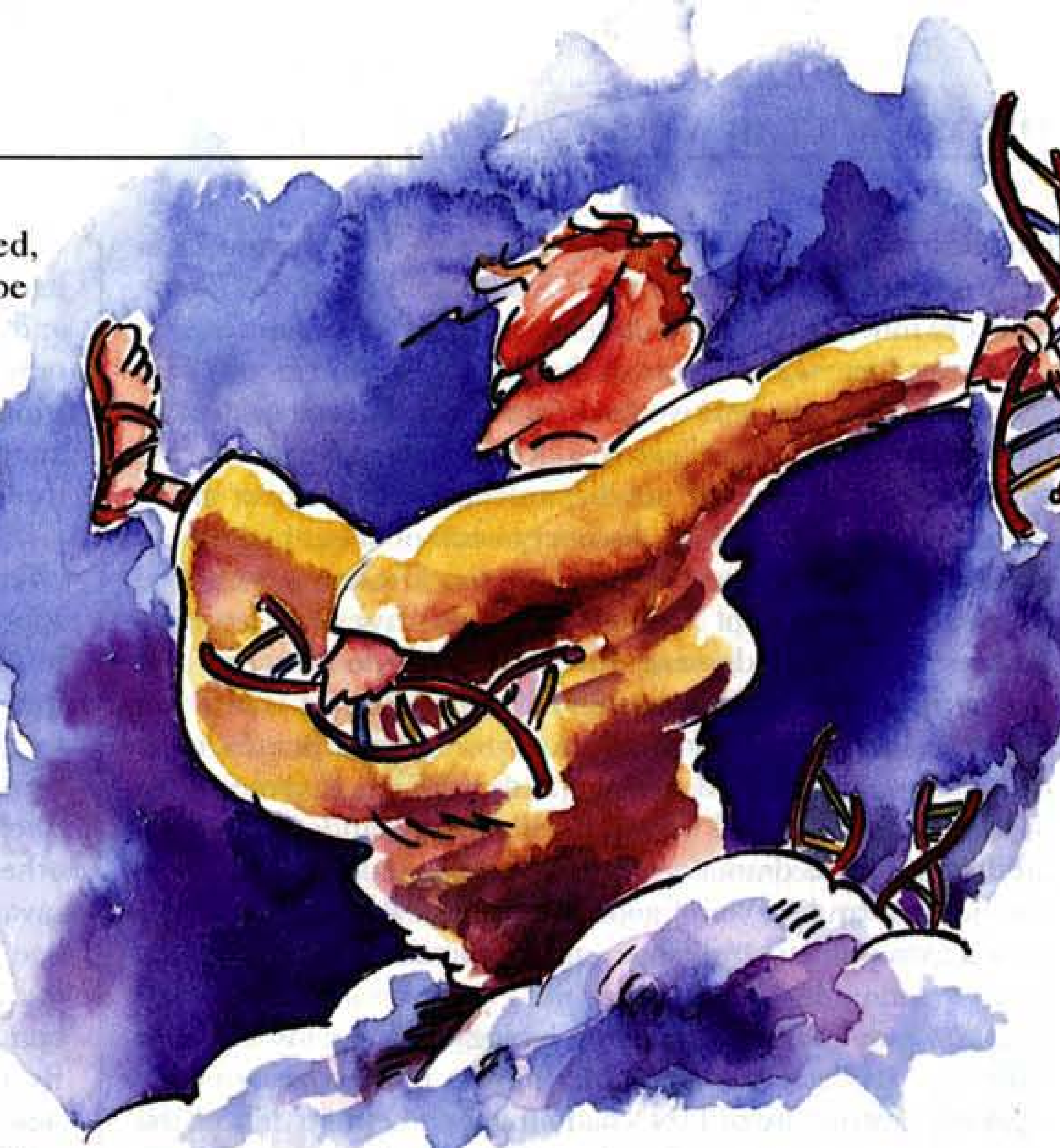
the importance of teaching environmental biology. I proposed, following standard departmental procedure, that Smith be offered joint membership in the Department of Biology.

Watson said softly, "Are they out of their minds?"

"What do you mean?" I was genuinely puzzled.

"Anyone who would hire an ecologist is out of his mind," responded the avatar of molecular biology.

For a few moments the room was silent. No one spoke to defend the nomination, but no one echoed Watson either. Then Paul Levine, the department chairman, jumped in to close the subject. This proposal, he said, is not one we are prepared to consider at this time. With documentation, we might examine the nomination at some future date. We never did, of course. Smith was elected a member only after the molecular biologists split off to form a department of their own.



After this meeting I walked across the Biological Laboratories quad on my way to the Museum of Comparative Zoology. Elso Barghoorn hurried to catch up with me. A senior professor of evolutionary biology, he was one of the world's foremost paleobotanists, the discoverer of Pre-Cambrian microscopic fossils, and an honest man. "Ed," he said, "I don't think we should use *ecology* as an expression anymore. It's become a dirty word." And sure enough, for most of the following decade we largely stopped using the word *ecology*. Only later did I sense the anthropological significance of the incident. When one culture sets out to erase another, the first thing its rulers banish is the official use of the native tongue.

The molecular wars were on. Watson was joined to varying degrees in attitude and philosophy by a small cadre of other biochemists and molecular biologists already in the department. They were George Wald, soon to receive a Nobel Prize for his work on the biochemical basis of vision; John Edsall, a pioneering protein chemist and a youngish elder statesman who smiled and nodded a lot but was hard to understand; Matthew Meselson, a brilliant young biophysicist newly recruited from the California Institute of Technology; and Paul Levine, the only other assistant professor besides Watson and myself promoted to tenure during the 1950s.

At faculty meetings we sat together in edgy formality, like Bedouin chieftains gathered around a disputed water well. We addressed one another in the old style: "As Professor Wetmore has just reminded us . . ." We used Robert's Rules of Order. Prestige, professorial appointments, and laboratory space were on the line. We all sensed that our disputes were not ordinary, of the academic kind that Robert Maynard Hutchins once said are so bitter because so little is at stake. Dizzying change and shifts of power were in the air throughout biology, and we were a microcosm. The traditionalists at Harvard at first supported the revolution. We agreed that more molecular and cellular biology was needed in the curriculum. The president and several successive deans of the Faculty of Arts and Sciences were also soon persuaded that a major shift in faculty representation was needed. The ranks of molecular and cellular biologists swelled rapidly. In one long drive, they secured seven of eight professorial appointments made. No one could doubt that their success was, at least in the abstract, deserved. The problem was that no one knew how to stop them from dominating the Department of Biology to the eventual extinction of other disciplines.

My own position was made more uncomfortable by the location of my office and laboratory in the Biological Laboratories, the bridgehead from physics and chemistry into which the richly funded molecular biologists were now pouring. I found the atmosphere there depressingly tense. Watson did not acknowledge my presence as we passed in the hall, even when no one else was near. I was undecided whether to respond in kind by pretending to be unaware of his own existence (impossible) or to humiliate myself by persisting with Southern politesse (also impossible). I settled on a mumbled salutation. The demeanor of Watson's allies ranged from indifferent to chilly, except for George Wald, who acquired an Olympian attitude. He was friendly indeed, but supremely self-possessed and theatrically condescending. On the few occasions we spoke, I could not escape the feeling that he was actually addressing an audience of hundreds seated behind me. He would in fact adopt political and moral oratory before large audiences as a second calling during the late 1960s. He was the kind of elegant, unworldly intellectual who fires up the revolution and is the first to receive its executioner's bullet. And on the future of our science he agreed completely with Watson. There is only one biology, he once declared, and it is molecular biology.

My standing among the molecularists was not improved by my having been granted tenure several months before Watson, in 1958. Although it was an accident of timing—I had received an unsolicited offer from Stanford and Harvard counteroffered—and in any event I considered him to be far more deserving, I can imagine how Watson must have taken the news. Badly.

Actually, I cannot honestly say I knew Jim Watson at all. The skirmish over Smith's appointment was one of only a half-dozen times he and I spoke directly to each other during his 12 years at Harvard and in the period immediately following. On one occasion, in October 1962, I offered him my hand and said, "Congratulations, Jim, on the Nobel Prize. It's a wonderful event for the whole department." He replied, "Thank you." End of conversation. On another occasion, in May 1969, he extended his hand and said, "Congratulations, Ed, on your election to the National Academy of Sciences." I replied, "Thank you very much, Jim." I was delighted by this act of courtesy.

At least there was no guile in the man. Watson evidently felt, at one level, that he was working for the good of science, and a blunt tool was needed. Have to crack eggs to make an omelet, and so forth. What he dreamed at a deeper level I never knew. I am sure only that had his discovery been of lesser magnitude he would have been treated at Harvard as just one more gifted eccentric, and much of his honesty would have been publicly dismissed as poor judgment. But people listened carefully, for the compelling reason that the deciphering of the DNA molecule with Francis Crick towered over all that the rest of us had achieved and could ever hope to achieve. It came like a lightning flash, like knowledge from the gods. The Prometheans of the drama were Jim Watson and Francis Crick, and not just by a stroke of good luck either. Watson-Crick possessed extraordinary brilliance and initiative.

For those not studying biology in the early 1950s, it is hard to imagine the impact the discovery of the structure of DNA had on our perception of how the world works. Reaching beyond the transformation of genetics, it injected into all of biology a new faith in reductionism. The most complex of processes, the discovery implied, might be simpler than we had thought. It whispered ambition and boldness to young biologists and counseled them: Try now; strike fast and deep at the secrets of life. When I arrived at Harvard as a graduate student in 1951, most outside the biochemical cognoscenti believed the gene to be an intractable assembly of proteins. Its chemical structure and the means by which it directs enzyme assembly would not, we assumed, be deciphered until well into the next century. The evidence nevertheless had grown strong that the hereditary substance is DNA, a far less complex macromolecule than most proteins. In 1953 Watson and Crick showed that pairing in the double helix exists and is consistent with Mendelian heredity. Soon it was learned that the nucleotide pairs form a code so simple that it can be read off by a child. The implication of these and other revelations rippled into organismic and evolutionary biology, at least among the younger and more entrepreneurial researchers. If heredity can be reduced to a chain of four molecular letters—granted, billions of such letters to prescribe a whole organism—would it not also be possible to reduce and accelerate the analysis of ecosystems and complex animal behavior? I was among the Harvard graduate students most excited by the early advances of molecular biology. Watson was a boy's hero of the natural sciences, the fast young gun who rode into town.

More's the pity that Watson himself and his fellow molecularists had no such foresights about the sector of biology in which I had comfortably settled. All I could sift from their pronouncements was the revolutionary's credo: Wipe the slate clean of this old-fashioned thinking and see what new order will emerge.

I was of course disappointed at this lack of vision. When Watson became director of the Cold Spring Harbor Laboratory in 1968, I commented sourly to friends that I wouldn't put him in charge of a lemonade stand. He proved me wrong. In 10 years he raised that noted institution to even greater heights by inspiration, fundraising skills, and the ability to choose and attract the most gifted researchers.

I was never able to suppress my admiration for the man. He had pulled off his achievement with courage and panache. He and other molecular biologists conveyed to his generation a new faith in the reductionist method of the natural sciences. A triumph of naturalism, it was part of the motivation for my own attempt in the 1970s to bring biology into the social sciences through a systematization of the new discipline of sociobiology.

The conflict set in motion another and ultimately positive effect of the molecular revolution. By the late 1950s the atmosphere in the department had become too stifling for members to plan the future of Harvard biology in ordinary meetings. So the professors in organismic and evolutionary biology prepared to exit. We formed a caucus and met informally to chart our own course. We began to think as never before about our future position in the biological sciences. I am reminded of another anthropological principle by this development. When savage tribes reach a certain size and density they split, and one group emigrates to a new territory. Among the Yanomamö of Brazil and Venezuela the moment of fission can be judged to be close at hand when there is a sharp increase in ax fighting. By the fall of 1960 our caucus had hardened to become the new Committee on Macrobiology.

Odd name that: *macrobiology*. In 1960 we realized that zoology, botany, entomology, and other disciplines centered on groups of organisms no longer reflected the reality of biology.

COURTESY EDWARD O. WILSON

Fieldwork: Edward O. Wilson with police escort during a long trek through the mountains of New Guinea in 1955.

The science was now being sliced crosswise, according to levels of biological organization, that is, oriented to the molecule, cell, organism, population, and ecosystem respectively. Biology spun through a 90 degree rotation in its approaches to life. Specialists became less concerned with knowing everything about birds or nematode worms or fungi, including their diversity.

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They focused more on the search for general principles at one or two of the organizational levels. To do so many contracted their efforts to a small number of species. Colleges and universities throughout the country accordingly reconfigured their research and teaching programs into departments of molecular biology, cell biology, developmental biology, and population biology, or rough equivalents of these divisions.

During this transitional period, which continued throughout the 1960s and into the 1970s, the expression "evolutionary biology" gained wide currency. It was meant to combine the higher strata of biological organization with multilevel approaches to the environment, animal behavior, and evolution. Conceding a spotty memory and not having undertaken archival research to improve upon it, I nevertheless believe that "evolutionary biology" was launched from Harvard and probably originated there. I know that in the spring of 1958 I concocted the term on my own and entered it in the Harvard catalogue as a course title for the following year. It was then spread at Harvard as follows.

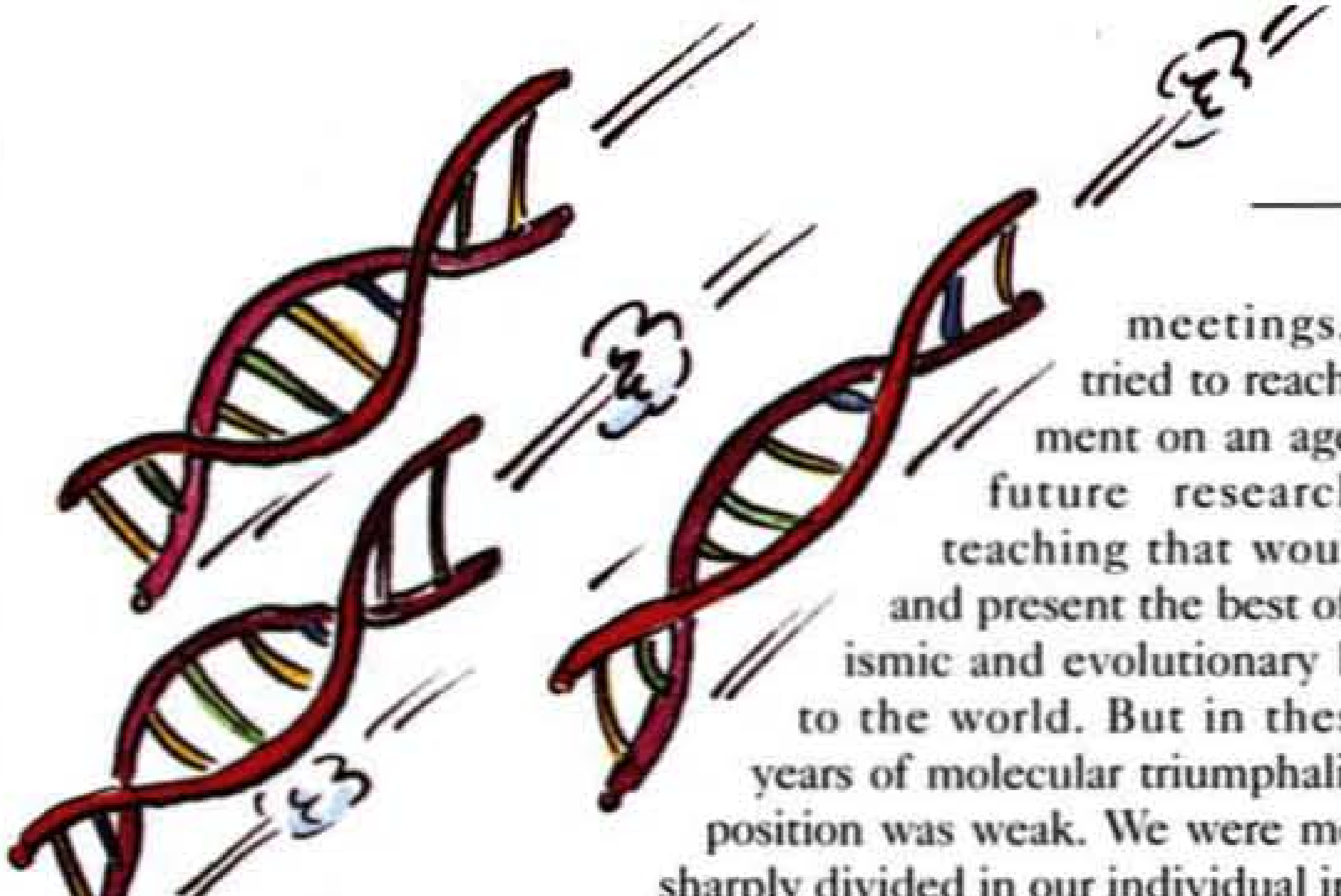
One fall day in 1961, after teaching the subject for three years, I was seated in the main seminar room of Harvard's new herbarium building across the table from George Gaylord Simpson, waiting for other members of the Committee on Macrobiology to arrive for one of our regular meetings. Simpson, considered the greatest paleontologist of the day, was then in the last years of his professorship at Harvard. I struck up a conversation, a necessity if we were not to sit looking at each other in silence: G. G., as we called him, almost never spoke first. He was shy, self-disciplined to an extreme, and totally absorbed in his work. I suspect that he prized every minute saved from talking with other people, which could then be invested in the writing of articles and books. He avoided committee work with stony resolution, refused to take graduate students, and gave lectures sparingly even by the cavalier standards of the general Harvard faculty. That day I approached him with a challenge. I was fretting about the proper name for our embattled end of biology. *Macrobiology*, we agreed, was a terrible word. *Classical biology* was out; that was what our molecular adversaries were calling it. Just "plain biology"? What about *real* biology? No and no. *Population biology*? Accurate but too restrictive. Well then, I said, what about *evolutionary biology*? That would cover the ground nicely. Given that evolution is the central organizing idea of biology outside the application of physics and chemistry, its use as part of the disciplinary name might serve as the talisman of intellectual independence. I tried the expression on others, and it was received very well. By the fall of 1962 we had a formal Committee on Evolutionary Biology.

As the time for a complete departmental split approached, our conflict with the molecular faction centered with increasing heat on new faculty appointments, taken up case by painful case. The Harvard faculty is a well-known pressure cooker in the sciences, in most subjects most of the time. Peer pressure among the tenured professors is superintended by vigilant deans and presidents determined to keep quality high. That combination of intent is responsible in large part for Harvard's lofty reputation. The explicit goal of all concerned is to select the best in the world in every discipline represented, or at least a workaholic journeyman toiling at the forefront. The probing questions invariably asked by both faculty and administration are, What has he discovered that is important? Does Harvard need someone in his discipline? Is he the best in that discipline? More than half the assistant professors either fail to make tenure or go elsewhere before being put to the test. Such was intensively the case in the Department of Biology in the late 1950s and early 1960s. Every appointment recommended by one of the two camps was scrutinized with open suspicion by the other.

The rising tension was due not just to the clash of megafaunistic egos. The fissure ran deeper, into the very definition of biology. The molecularists were confident that the future belonged to them. If evolutionary biology was to survive at all, they thought, it would have to be changed into something very different. They or their students would do it, working upward from the molecule through the cell to the organism. The message was clear: Let the stamp collectors return to their museums.

The evolutionary biologists were not about to step aside for a group of test-tube jockeys who could not tell a red-eyed vireo from a mole cricket. It was foolish, we argued, to ignore principles and methodologies distinctive to the organism, population, and ecosystem, while waiting for a still formless and unproved molecular future.

We were forced by the threat to rethink our intellectual legitimacy as never before. In corridor conversations and caucus



meetings, we tried to reach agreement on an agenda of future research and teaching that would soar and present the best of organismic and evolutionary biology to the world. But in these first years of molecular triumphalism our position was weak. We were moreover sharply divided in our individual interests and aspirations. Most of the caucus members were too specialized, too fixed in their ways, or too weak to resist. They sat through department meetings numbly, preferring to seek common ground by dwelling on subjects of lesser import: Who will teach the elementary course? What is the status of the Arnold Arboretum? Shall we be active partners in the new Organization for Tropical Studies? For their part the molecular biologists made little effort to articulate a philosophy of biological research. To them the future had already been made clear by the heady pace of their own progress. Unspoken but heavily implied was the taunt: Count our Nobel Prizes. Ernst Mayr and George Simpson, giants of the Modern Synthesis, heroes of my youth, and incidentally denied Nobel Prizes because none are given in evolutionary biology, seemed oddly reluctant to broach these central issues openly in the meetings. Why rile the molecularists, and make an unpleasant situation worse?

In the absence of strong statesmanship in evolutionary biology, our potential allies were falling away. One of the two most distinguished organismic biologists of the time, Donald Griffin, discoverer of animal sonar, was early on persuaded by the molecularist philosophy. We are all evolutionary biologists, he declaimed at one meeting, are we not? Doesn't what we learn at every level contribute to the understanding of evolution? The eminent insect physiologist Carroll Williams remained amiably neutral. A courtly Virginian who had spent his adult life at Harvard with tidewater accent intact, he insisted on maintaining the manners that had prevailed in the old department. More important than personality, however, was the plain fact that the evolutionary biologists could point to no recent great advances comparable to those in molecular and cellular biology swelling the pages of *Nature*, *Science*, and the *Proceedings of the National Academy of Sciences*.

There is a final principle of social behavior to help keep these many developments in perspective. When oppressed peoples have no other remedy they resort to humor. In 1967 I composed a "Glossary of Phrases in Molecular Biology" that was soon distributed in departments of biology throughout the country and praised—by evolutionary biologists—for capturing the strut of the conquerors. My samizdat included the following expressions, which I have changed here from alphabetical order to create a logical progression of the concepts:

Classical Biology. That part of biology not yet explained in terms of physics and chemistry. Classical Biologists are fond of claiming that there is a great deal of Classical Biology that individual Molecular Biologists do not know about; but that is all right because it is probably mostly not worth knowing about anyway, we think. In any case, it doesn't matter, because eventually it will all be explained in terms of physics and chemistry; then it will be Molecular Biology and worth knowing about.

Brilliant Discovery. A publishable result in the Mainstream of Biology.

Mainstream of Biology. The set of all projects being worked on by me and my friends. Also known as Modern Biology and Twenty-first Century Biology.

Exceptional Young Man. A beginning Molecular Biologist who has made a Brilliant Discovery (*q.v.*).

First-rate. Pertaining to biologists working on projects in the Mainstream of Biology.

Molecular Biology. That part of biochemistry that has supplanted part of Classical Biology. A great deal of Molecular Biology is being conducted by First-rate Scientists who make Brilliant Discoveries.

Third-rate. Pertaining to Classical Biologists.

First-rate, Brilliant, Wave of the Future... believe me, this was the phrasing actually used. Today those once oft-heard mantras clink with antique brittleness. The passage of 30 years has done much to close the divide between molecular and evolutionary biology. As I write, systematists, the solitary experts on groups of organisms, have unfortunately been largely eliminated from academic departments by the encroachment of the new fields. That is the worst single damage caused by the molecular revolution. Ecologists, pushed to the margin for years, have begun a resurgence through the widespread recognition of the global environment crisis. Molecular biologists, as they promised, have taken up evolutionary studies, making important contributions whenever they can find systematists to tell them the names of organisms. The surviving evolutionary biologists routinely use molecular data to pursue their Darwinian agenda. The two sides sometimes speak warmly to each other. Indeed, teams from both domains increasingly collaborate to conduct First-rate Work in what may now safely and fairly be called part of the Mainstream of Biology. The corridor language one overhears from molecular biologists has grown more chaste and subtle. Only hard-shelled fundamentalists among them think that higher levels of biological organization, populations to ecosystems, can be explained by molecular biology.

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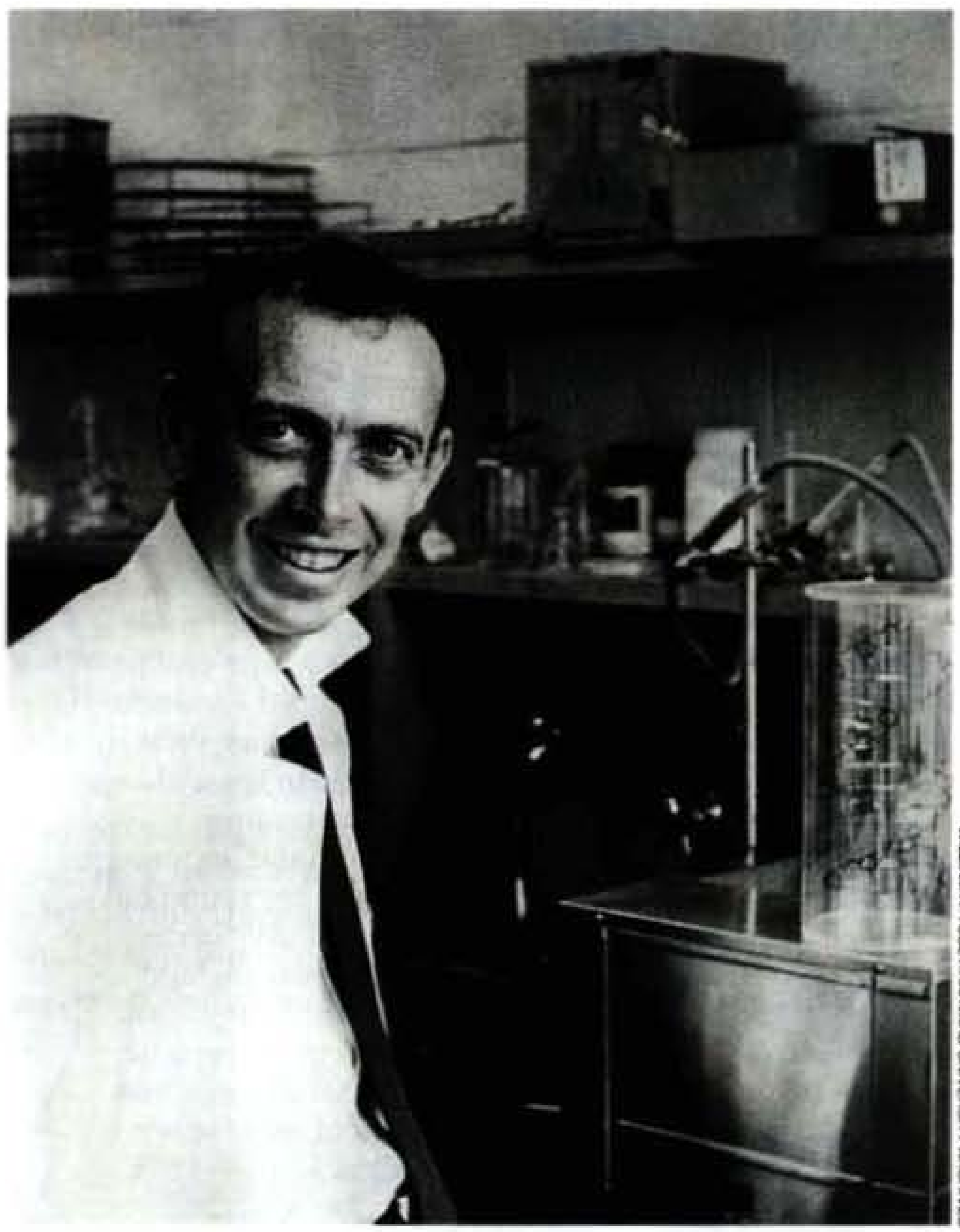
I did not foresee this accommodation in the 1960s, caught as I was in the upheaval. Worse, I was physically trapped in the Biological Laboratories among the molecular and cellular biologists, who seemed to be multiplying like the *E. coli* and other microorganisms on which their finest work had come to be based. In buildings a hundred feet and a world of ideas away were the principalities and margravates of the senior evolutionary biologists. They were mostly curators and professors in charge of Harvard's "Associated Institutions," comprising the Museum of Comparative Zoology, the University herbaria, the Botanical Museum, the Arnold Arboretum, and the Harvard

Forest. I envied them mightily. They could retreat to their collections and libraries and continue to be supported by venerable endowments bearing the names of nineteenth-century Anglo-Saxons.

What I desired most was to emigrate across the street to the Museum of Comparative Zoology, to become a curator of insects, to surround myself with students and like-minded colleagues in an environment congenial to evolutionary biology, and never have to pass another molecular biologist in the corridor. But I held off requesting such a move for 10 years, while Ernst Mayr was director. Perhaps I was overly timid, but the great man seemed forbiddingly stiff and cool toward me personally. There was also the 25-year difference in age, and the fact that I had felt filial awe ever since adopting his book *Systematics and the Origin of Species* as my bible when I was 18. We have since become good friends, and I speak to him frankly on all—well, most—matters (he is still fully active in his 90th year as I write), but at that time I felt it would be altogether too brash to ask for haven in his building. My self-esteem was fragile then to a degree that now seems beyond reason. I dared not risk the humiliation of a refusal. I figured the odds at no better than 50-50 he would give it. When a new director, A. W. ("Fuzz") Crompton, was installed and proved as approachable in personality as the nickname implies, I asked him for entry. Fuzz promptly invited me to the newly erected laboratory wing of the Museum ("You've made my day, Ed") and soon afterward had me appointed Curator in Entomology. I do not doubt that the molecular biologists were also pleased to see me leave. One day near the end, while I sat at my desk, Mark Ptashne, one of the younger shock troopers of this amazing group, walked into my quarters unannounced with a construction supervisor and began to measure it for installation of equipment.



WILLIAM H. TOBEY/HARVARD UNIVERSITY ARCHIVES

Lab work: Newly named Nobel laureate James Dewey Watson in his Harvard laboratory, October 1962.

By this time I had been radicalized in my views about the future of biology. I wanted more than just sanctuary across the street, complete with green eyeshades, Cornell drawers of pinned specimens, and round-trip air tickets for fieldwork in Panama. I wanted a revolution in the ranks of the young evolutionary biologists. I felt driven to go beyond the old guard of Modern Synthesizers and help to start something new. That might be accomplished, I thought, by the best effort of men my age (men, I say, because women were still rare in the discipline) who were as able and ambitious as the best molecular biologists. I did not know how such an enterprise might be started, but clearly the first requirement was a fresh vision from the young and ambitious. I began to pay close attention to those in other universities who seemed like-minded.

A loose cadre in fact did form. In January 1960 I was approached by an editorial consultant of Holt, Rinehart and Winston, a leading publisher of scientific texts, who asked me to referee the manuscript of a short book by Larry Slobodkin. The title was *Growth and Regulation in Animal Populations.* As I flipped through the manuscript pages I was excited by Slobodkin's crisp style and deductive approach to ecology. He advanced simple mathematical models to describe the essential features of population dynamics, then expanded on the premises and terms of the equations to ask new questions. He argued that such complex phenomena as growth, age structure, and competition could be broken apart with minimalist reasoning, leading to experiments devised in the postulational-deductive method of traditional science. He went further: the hypotheses and experimental results could be greatly enriched by explanations from evolution by natural selection.

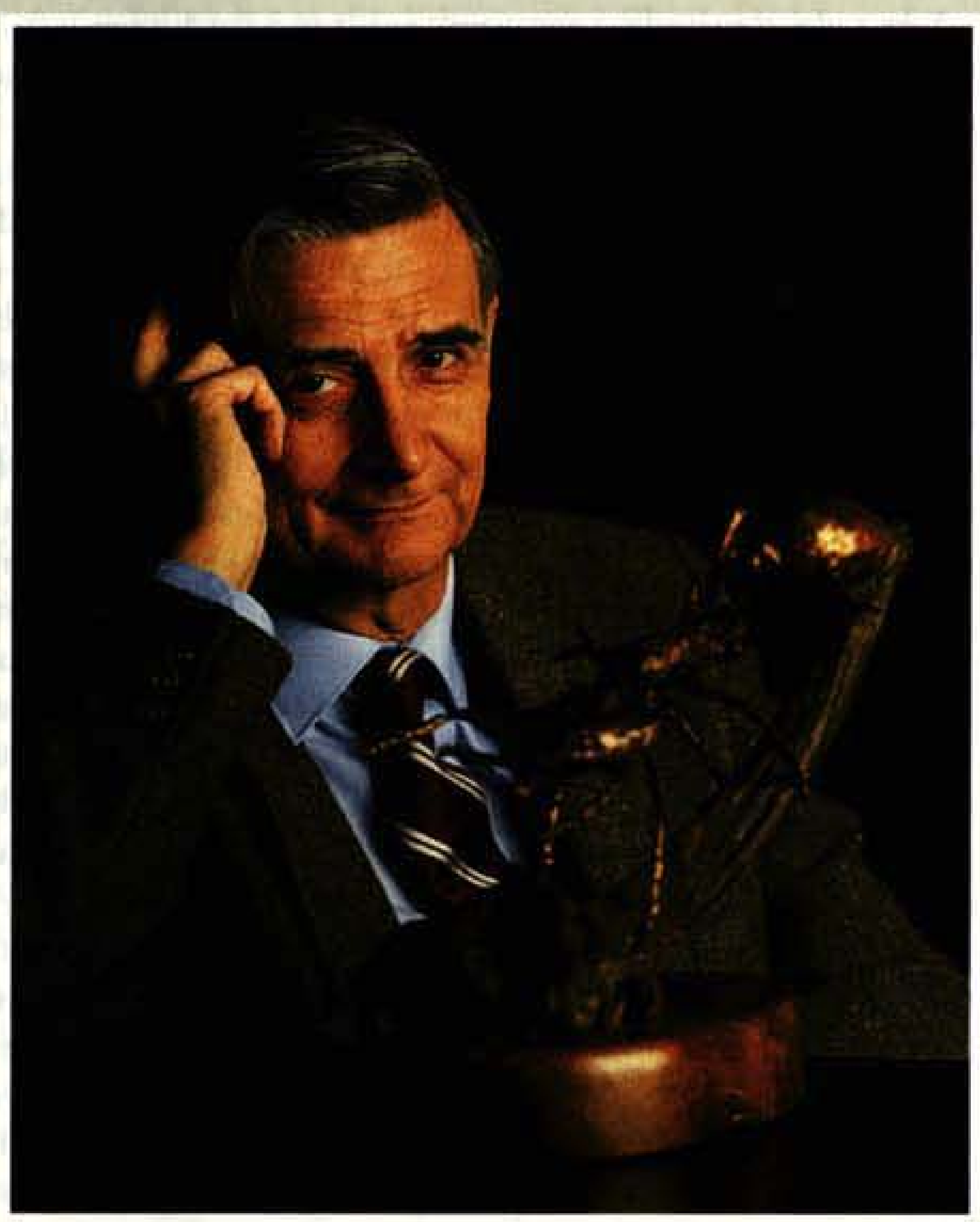
Slobodkin was not the first scientist to advance this prospectus for the invigoration of ecology, but the clarity of his style and the authority implied by a textbook format rendered the ideas persuasive. It dawned on me that ecology had never before been incorporated into evolutionary theory; now Slobodkin was showing a way to do it. He also posed, or so I read into his text, the means by which ecology could be linked to genetics and biogeography. Genetics, I say, because evolution is a change in the heredity of populations. And biogeography, because the geographic ranges of genetically adapted populations determine the coexistence of species. Communities of species are assembled by genetic change and the environmentally mediated interaction of the species. Genetic change and interaction determine which species will survive and which will disappear. In order to understand evolution, then, it is necessary to include the dynamics of populations.

With this conception in mind, and my hopes kindled that Slobodkin would emerge as a leader in evolutionary biology, I wrote an enthusiastic report to the editor. A short time later I approached Slobodkin himself, suggesting that the time had come to produce a more comprehensive textbook on population biology. Would he be interested in writing one with me? In such a collaboration, he might introduce population dynamics and community ecology, while I added genetics, biogeography, and social behavior. The material would serve as an intermediate-level textbook. It would also promote a new approach to evolutionary biology founded on ecology and mathematical modeling.

Slobodkin said he was interested. He would talk the matter over with me. Soon afterward we met in Cambridge to outline our prospective work. We went so far as to draw up individual assignments in the form of chapter headings.

Slobodkin was then an assistant professor at the University of Michigan. A rising star in the admittedly still depauperate field of American ecology, he was later to move to the Stony Brook campus of the State University of New York, where he founded a new program in evolutionary biology.

During the years to follow, I never failed to find Slobodkin's physical appearance arresting: red-haired, alternately clean-shaven and dramatically mustachioed, an ursine body relaxed in scholar's informality. Not given to easy laughter, he preferred ironic maxims over funny stories. His conversational tone was preoccupied and self-protective, and to a degree unusual in a young man tended toward generalizations about science and the human condition. It was leavened in the company of friends with discursive sentences and fragments of crude humor, seemingly contrived to throw the listener off balance, especially when combined with Delphic remarks of the kind philosophers use to stop conversations. These latter asides implied: There is more to the subject of our banter, much more; see if you can figure it out. Slobodkin in fact was a philosopher. I came to think of him as progressing through a scientific career



MUSEUM OF COMPARATIVE ZOOLOGY

Ants' best friend: Edward O. Wilson with a sculpture of *Daceton armigerum*. "Living with Ants and the Science of E. O. Wilson" opens May 5 at the Museum of Comparative Zoology.

A Distinguished Life in Science

Naturalist. The word has an antique ring, redolent of fieldwork, collecting bottles, and specimens mounted in display cases. As the unadorned title of Edward O. Wilson's memoir, the term usefully describes much of a distinguished life in science, the drives and discoveries of a man who writes simply, "Most children have a bug period, and I never grew out of mine." Wilson's infatuations proved hugely productive, giving birth to such works as *Sociobiology*, *On Human Nature* (a Pulitzer Prize-winner), and *The Diversity of Life* and making him a founder of both sociobiology and biodiversity studies.

Beyond documenting Wilson's life, *Naturalist* also makes a statement about the conduct and purposes of biology. In this article, excerpted from chapter 12, Wilson revisits the eruption of molecular biology and the near extinction of species- and population-based evolutionary studies of life. He chronicles changing paradigms, clashing personalities, and intellectual passions in conflict.

Today the battle lines have blurred. Interviewed just before trips to Washington, D.C. (to talk to House Speaker Newt Gingrich about the "mauling" of environmental regulations), and Madrid (to receive an honorary degree), Wilson said biologists are moving beyond "two cultures, really, with different languages and tools." As knowledge has grown, "The evolutionary biologists have incorporated cell and molecular biology into their armamentarium," while "the molecular biologists have also expanded into evolutionary studies," using those techniques to gain a better understanding of "the nature of the whole genome."

The result, as he sees it, if not yet routine collaboration in the laboratory, amounts to "a growing spirit of colleagueship." As evidence, Wilson mentions his appearance at Cold Spring Harbor last autumn, to speak at a conference on the brain and behavior. There he and his old antagonist James Watson agreed on the need to conduct research from "the molecular level to the cell to the brain to the organism and even at the level of the population and its social behavior." Each side noted the other's contribution, Wilson says, making the occasion "a coming together of the principals" that was more than symbolic.

Naturalist has met with extraordinary acclaim. *The New York Times Book Review* named it one of the 11 best books of 1994, and it was a National Book Critics Circle finalist. Remarkably, it was only one of the two books the prolific Pellegrino University Professor published last October. The other, *Journey to the Ants* (Harvard University Press), with Bert Hölldobler, makes more accessible to lay readers their earlier volume, *The Ants*, another Pulitzer winner.

Nor has Wilson's pace slowed. While planning to retire by 1998, he still teaches the undergraduate Core course on evolutionary biology. And as a writer, he is busy "overextending myself as always." Works in progress include a monograph classifying the *Pheidole*, the world's largest ant genus, with 604 known species in the Western hemisphere; and a book "attempting to join biology, social sciences, and the environment, including considerations of moral reasoning." This naturalist still carries a full rucksack. —J.S.R.

to a destiny somewhere in the philosophy of science, where he would become a guru, a rabbi, and an interpreter of the scripture of natural history. Some of our friends complained that his persona was a pose, and perhaps it was to some degree, but I enjoyed Slobodkin's subtle and penetrating mind, and his company. Not least, we were opposites in cultural origin, which made him all the more interesting to me. He was a New York intellectual, a Jew, as far in every dimension of temperament and style as it is possible to get from the sweat-soaked field entomologist I still fancied myself to be, then, in the early 1960s.

Slobodkin was heavily influenced by his Ph.D. adviser at Yale, G. Evelyn Hutchinson, himself as different from Slobodkin and me as Larry and I were from each other: our relationship formed an equilateral triangle. Born in 1903, the son of Arthur Hutchinson, the Master of Pembroke College at Cambridge University, Evelyn—"Hutch" to those who dared call him an intimate—was a creation of British high-table science. True to the Oxbridge prize Fellow tradition, he never bothered to earn the doctorate but instead trained himself into a polymath of formidable powers. He was a free spirit, an eclecticist who proved brilliant at fitting pieces together into large concepts. He never seemed to have met a fact he didn't like or couldn't use, somewhere, to start an essay or at least place in a footnote. He began his career as a field entomologist studying aquatic "true bugs," as experts call them—members of the order Hemiptera. He worked as far from home as Tibet and South Africa. Then he turned to pioneering research on algae and other phytoplankton of lakes and ponds. He broadened his scope to include the cycles and stratification of nutrients on which life in these bodies of water depends. He was among the first students of biogeochemistry, a complex discipline combining analyses of land, water, and life. Still later, after becoming professor of zoology at Yale in 1945, he turned to the evolution of population dynamics, which also became Slobodkin's forte.

Hutchinson's insights were deep and original, and, notwithstanding that such tropes have been worn to banality through overuse, he deserves to be called the father of evolutionary ecology. Among his notions that proved most influential was the "Hutchinsonian niche." Like most successful ideas in science, it is also a simple one: the life of a species can be usefully described as the range of temperatures in which it is able to live and reproduce, the range of prey items it consumes, the season in which it is active, the hours of the day during which it feeds, and so on down a list as long as the biologist wishes to make it. The species is viewed as living within a space defined by the limits of these biological qualities each placed in turn on a separate scale.

Hutchinson's independence was such that he remained unperturbed by molecular triumphalism; at least I never heard of his protesting in the manner of his colleagues in Harvard's overheated department. In his later years he metamorphosed gracefully from field biologist to guru, seated in his office with wispy white hair and basset eyes. Beside him presided a stuffed specimen of the giant Galápagos tortoise. In a teaching career spanning nearly three decades, he trained 40 of the best ecologists and population biologists in the world to the doctoral level, including, of course, Larry Slobodkin. They all seemed to admire and love the man, and to have drawn strength and momentum from his example. Fanning out across the country to represent the many growing fields of ecology, they exerted a crucial influence in American biology.

I asked several after they became my friends what "Hutch" did to inspire such enterprise in his disciples. The answer was always the same: nothing. He did nothing, except welcome into his office every graduate student who wished to see him, praise everything they did, and with insight and marginal scholarly digressions, find at least some merit in the most inchoate of research proposals. He soared above us sometimes, and at others he wandered alone in a distant terrain, lover of the surprising metaphor and the esoteric example. He resisted successfully the indignity of being completely understood. He encouraged his acolytes to launch their own voyages. It was pleasant, on the several occasions I lectured at Yale before Hutchinson's death in 1991, to encounter him and receive his benediction. Head bobbing slightly between hunched shoulders, a wise human Galápagos tortoise, he would murmur,

Lover of the surpising metaphor and the esoteric example, Hutchinson resisted successfully the indignity of being completely understood.

Wonderful, Wilson, well done, very interesting. It would have been pleasant to stay near him, the kindly academic father I never knew. I came to realize that the overgenerous praise did not weaken the fiber of our character. Hutchinson's students criticized one another, and me as well, and that was enough to spare us from major folly most of the time.

Hutchinson and Slobodkin were then what today are called evolutionary ecologists. In my formative years they caused me to try to become one as well. Through them I came to appreciate how environmental science might be better meshed with biogeography and the study of evolution, and I gained more confidence in the intellectual independence of evolutionary biology. I was encouraged to draw closer to the central problem of the balance of species, which was to be my main preoccupation during the 1960s, as the molecular wars subsided to their ambiguous conclusion.