Fro. "Naturalist"
By E.O. Wilson

chapter twelve

THE MOLECULAR WARS

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 codiscoverer 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 twenty-eight, 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" biology—*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 twenty-four 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 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 biophysi-

cist 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. Levine soon deserted population biology and began to promote the new doctrine aggressively on his own. Zeal of the convert, I thought to myself.

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. At the height of the campus turmoil at Harvard and elsewhere, Wald was the speaker of choice before cheering crowds of student activists. 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 only one of a half-dozen times he and I spoke directly to each other during his twelve 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 only sure 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, and a few younger colleagues aped his manners, 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. It is further a singular commentary on the conduct of science that (according to Watson in a later interview) no other qualified person was interested in devoting full time to the problem.

For those not studying biology at the time 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. ("It has not escaped our notice," they wrote teasingly at the end of their 1953 letter to *Nature*, "that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material.") 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 (he kept his Harvard professorship by joint appointment until 1976) I commented sourly to friends that I wouldn't put him in charge of a lemonade stand. He proved me wrong. In ten years he raised that noted institution to even greater heights by inspiration, fund-raising skills, and the ability to choose and attract the most gifted researchers.

A new Watson gradually emerged in my mind. In October 1982, at a reception celebrating the fiftieth anniversary of Harvard's Biological Laboratories, he pushed his way across a crowded room to compliment me on a throwaway remark I had made during a lecture earlier that afternoon. "The history of philosophy," I had said, "consists largely of failed models of the brain." Afterward I realized that my phrasing was the kind of preemptive dismissal he would have made twenty years earlier. Had I been corrupted in the meantime?

Yes, a little perhaps. I had never been 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. 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 ninety-degree rotation in its approaches to life. Specialists became less concerned with knowing everything about birds or nematode worms or fungi, including their diversity. 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 catalog 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.