Book Reviews

A Quick Climb Up Mount Olympus

The Double Helix. A Personal Account of the Discovery of the Structure of DNA. JAMES D. WATSON. Atheneum, New York, 1968. xvi + 238 pp., illus. $5.95.

Unfortunately, I hear it very often said of a scientist, "He's got charisma." What is meant by "charisma" is not easy to say. It seems to refer to some sort of ambrosial body odor: an emanation that can be recognized most easily by the fact that "charismatic" individuals expect to be paid at least two-ninths more than the rest, unless Schweitzer or Einstein chairs are available. But what does one do if two men share one charisma?

This would certainly seem to be the case with the two who popularized base-pairing in DNA and conceived the celebrated structural model that has become the emblem of a new science, molecular biology. This model furnishes the title of this "personal account," and Watson describes it, without undue modesty, as "perhaps the most famous event in biology since Darwin's book." Whether Gregor Mendel's ghost concurred in this rodomontade is not stated. The book as a whole testifies, however, to a regrettable degree of strand separation which one would not have thought possible between heavenly twins; for what is Castor without Pollux?

This is the beginning of chapter 1 of Watson's book:

> I have never seen Francis Crick in a modest mood. Perhaps in other company he is that way, but I have never had reason so to judge him. It has nothing to do with his present fame. Already he is much talked about, usually with reverence, and someday he may be considered in the category of Rutherford or Bohr. But this was not true when, in the fall of 1951, I came to the Cavendish Laboratory of Cambridge University. . . .

As we read on, the impression grows that we are being taken on a sentimental journey; and if the book lacks the champagne sparkle of Sterne's garrulous prose, it bubbles at least like soda water: a beverage that some people are reported to like more than others. The patter is maintained throughout, and habitual readers of gossip columns will like the book immensely: it is a sort of molecular Cholly Knickerbocker. They will be happy to hear all about the marital difficulties of one distinguished scientist (p. 26), the kissing habits of another (p. 66), or the stomach troubles of a third (p. 136). The names are preserved for posterity; only I have omitted them here. Do you wish to accompany the founders of a new science as they run after the "Cambridge popsies"? Or do you want to share with them an important truth? "An important truth was slowly entering my head: a scientist's life might be interesting socially as well as intellectually."

In a foreword to Watson's book Sir Lawrence Bragg praises its "Pepys-like frankness," omitting the not inconsiderable fact that Pepys did not publish his diaries; they were first printed more than a hundred years after his death. Reticence has not been absent from the minds of many as they set out to write accounts of their lives. Thus Edward Gibbon, starting his memoirs:

> My own amusement is my motive and will be my reward; and, if these sheets are communicated to some discreet and indulgent friends, they will be secreted from the public eye till the author shall be removed beyond the reach of criticism or ridicule.

But less discreet contemporaries would probably have been delighted had there been a book in which Galilei said nasty things about Kepler. Most things in Watson's book are, of course, not exactly nasty—except perhaps the treatment accorded the late Rosalind Franklin—and some are quite funny, for instance, the description of Sir Lawrence's futile attempts to escape Crick's armor-piercing voice and laughter. It is a great pity that the double helix was not discovered ten years earlier: some of the episodes could have been brought to the screen splendidly by the Marx brothers.

As we read about John and Peter, Francis and Herman, Rosy, Odile, Elizabeth, Linus, and Max and Maurice, we may often get the impression that we are made to look through a keyhole at scenes with which we have no business. This is perhaps unavoidable in an autobiography; but then the intensity of vision must redeem the banality of content. This requirement can hardly be said to be met by Watson's book, which may, however, have a strong coterie appeal, as our sciences are dominated more than ever by multiple cliques. Some of those will undoubtedly be interested in a book in which so many names, and usually first names, appear that are known to them.

This is then a scientific autobiography; and to the extent that it is nothing else, it belongs to a most awkward literary genre. If the difficulties facing a man trying to record his life are great—and few have overcome them successfully—they are compounded in the case of scientists, of whom many lead monotonous and uneventful lives and who, besides, often do not know how to write. Though I have no profound knowledge of this field, most scientific autobiographies that I have seen give me the impression of having been written for the remainder tables of the bookstores, reaching them almost before they are published. There are, of course, exceptions; but even Darwin and his circle come to life much more convincingly in Mrs. Raverat's charming recollections of a Cambridge childhood than in his own autobiography, remarkable a book though it is. When Darwin, hypochondriacally wrapped in his shivering plaid, wrote his memoirs, he was in the last years of his life. This touches on another characteristic facet: scientists write their life's history usually after they have retired from active life, in the solemn moment when they feel that they have not much else to say. This is what makes these books so sad to read: the eagerness has gone; the beaverness remains. In this respect, Watson's book is quite exceptional: when it begins he is 23, and 25 when it ends; and it was written by a man not yet 40.

There may also be profounder reasons for the general triteness of scientific autobiographies. *Timon of Athens* could not have been written, *Les De-*

moiselles d'Avignon not have been painted, had Shakespeare and Picasso not existed. But of how many scientific achievements can this be claimed? One could almost say that, with very few exceptions, it is not the men that make science; it is science that makes the men. What A does today, B or C or D could surely do tomorrow.

Hence the feverish and unscrupulous haste that Watson's book reflects on nearly every page. On page 4: "Then DNA was still a mystery, up for grabs, and no one was sure who would get it and whether he would deserve it. . . . But now the race was over and, as one of the winners, I knew the tale was not simple. . . ." And on page 184: "I explained how I was racing Peter's father [Pauling] for the Nobel Prize." Again on page 199: "I had probably beaten Pauling to the gate." These are just a few of many similar instances. I know of no other document in which the degradation of present-day science to a spectator sport is so clearly brought out. On almost every page, you can see the protagonists racing through the palaestra, as if they were chased by the Hound of Heaven—a Hound of Heaven with a Swedish accent.

There were, of course, good reasons for the hurry, for these long-distance runners were far from lonely. They carried, however, considerably less baggage than others whom they considered, sometimes probably quite wrongly, as their competitors. Quite a bit was known about DNA: the discovery of the base-pairing regularities pointed to a dual structure; the impact of Pauling's α-helix prepared the mind for the interpretation of the x-ray data produced by Wilkins, Franklin, and their collaborators at King's College without which, of course, no structural formulation was possible. The workers at King's College, and especially Miss Franklin, were naturally reluctant to slake the Cavendish couple's thirst for other people's knowledge, before they themselves had had time to consider the meaning of their findings. The evidence found its way, however, to Cambridge. One passage must be quoted. Watson goes to see the (rather poor) film *Ecstasy* (p. 181):

> Even during good films I found it almost impossible to forget the bases. The fact that we had at last produced a stereochemically reasonable configuration for the backbone was always in the back of my head. Moreover, there was no longer any fear that it would be incompatible with the experimental data. By then it had been checked out with Rosy's precise measurements. Rosy, of course, did not directly give us her data. For that matter, no one at King's realized they were in our hands. We came upon them because of Max's membership on a committee appointed by the Medical Research Council to look into the research activities of Randall's lab. Since Randall wished to convince the outside committee that he had a productive research group, he had instructed his people to draw up a comprehensive summary of their accomplishments. In due time this was prepared in mimeograph form and sent routinely to all the committee members. As soon as Max saw the sections by Rosy and Maurice, he brought the report in to Francis and me. Quickly scanning its contents, Francis sensed with relief that following my return from King's I had correctly reported to him the essential features of the B pattern. Thus only minor modifications were necessary in our backbone configuration.

Rosy is Rosalind Franklin, Max stands for Perutz.

As can be gathered from this astonishing paragraph, Watson's book is quite frank. Without indulging in excesses of self-laceration, he is not a "stuffed shirt" and seems to tell what he considers the truth, at any rate, so far as it concerns the others. In many respects, this book is less a scientific autobiography than a document that should be of interest to a sociologist or a psychologist, who could give an assessment that I am not able to supply. Such an analysis would also have to take account of the merciless persiflage concerning "Rosy" (not redeemed by a cloying epilogue) which goes on throughout the book. I knew Miss Franklin personally, as I have known almost all the others appearing in this book; she was a good scientist and made crucial contributions to the understanding of the structure of DNA. A careful reading even of this book will bear this out.

It is perhaps not realized generally to what extent the "heroes" of Watson's book represent a new kind of scientist, and one that could hardly have been thought of before science became a mass occupation, subject to, and forming part of, all the vulgarities of the communications media. These scientists resemble what Ortega y Gasset once called *the vertical invaders*, appearing on the scene through a trap door, as it were. "He [Crick] could claim no clear-cut intellectual achievements, and he was still without his Ph.D." "Already for thirty-five years he [Crick] had not stopped talking and almost nothing of fundamental value had emerged." I believe it is only recently that such terms as the stunt or the scoop have entered the vocabulary of scientists, who also were not in the habit before of referring to each other as smart cookies. But now, the modern version of King Midas has become all too familiar: whatever he touches turns into a publicity release. Under these circumstances, is it a wonder that what is produced may resemble a Horatio Alger story, but will not be a *Sidereus Nuncius*? To the extent, however, that Watson's book may contribute to the much-needed demythologization of modern science, it is to be welcomed.

ERWIN CHARGAFF
Department of Biochemistry, Columbia University, New York City

Alaska: The Measureless Wealth

Glacier Bay. The Land and the Silence. DAVE BOHN. DAVID BROWER, Ed. Sierra Club, San Francisco, 1967. 165 pp., illus. $25.

In *Glacier Bay*, the Sierra Club once again turns to the task of stimulating public awareness of the natural world and of imparting respect for the land. This magnificently illustrated and sensitively written volume, along with such earlier Sierra Club books as those on the Grand Canyon, the Big Sur coast, and the High Sierra, allow one to *see* and to marvel.

The wondrous scenes these volumes contain are themselves the best of all arguments for resisting needless encroachment on them by the mining companies, the loggers, and the dam builders. Although economic analysis is becoming increasingly useful in shaping policy on the use and conservation of natural resources, economists know no way to make benefit-cost analysis adequately reflect the intangible values of wilderness and other natural environments. A view of, say, the Grand Canyon's inner gorge is indisputably of value, but it is not a marketable masterpiece to be sold at auction. Indeed, to put a price on such a scene is to play into the hands of those who would plug the gorge with concrete and flood it. In the realm of benefit-cost analysis, as in the marketplace, the demand is not for abstractions but for ready coin.

Although some of them are keenly appreciative of natural values, economists seem not to have had much suc-