

typical size and the features which distinguish that species from those with which it might be confused; and notes on habitat and geographical distribution, often with qualitative comments on abundance. Usually there is additional information on what is known of the animal's natural history. Each entry closes with literature citations, sometimes only one or two, in other cases several dozen. Detailed references are then provided at the end of each chapter.

The introduction suggests that this book was intended as a primary guide for identifying the animals, but this emphasis is, I think, misplaced; it will simply not do as a field manual. Its unusual format and sheer weight (about six pounds) militate against carrying the book along to the shore. Beyond the problems of getting book and animal together (since the reader is, rightly, urged not to bring specimens home with him), there are none of the traditional "keys" for identification; and the coverage is intentionally restricted to the more common and conspicuous elements of the fauna, meaning that using the plates and text for primary identification would be not only time-consuming but often misleading.

For field identification, then, one should instead take along Smith and Carlton's revision of *Light's Manual* (University of California Press, 1980). Thereafter, *Intertidal Invertebrates of California* can be treated as something akin to a biographical dictionary. Knowing, or at least having a strong hunch about the name of the creature, the reader can here confirm that he has, indeed, probably seen this or that member of the community, and then learn some of what the experts know about it, as well as where to find further information.

A conspicuous contrast with a biographical dictionary is that no historian is ever likely, on the basis of some arbitrary legal point, to decide that henceforth Madame Curie will be known only as Marie Skłodowska. Many of my old friends in the intertidal zone, however, regardless of how well established their reputations were, are now assigned names different from those I learned 25 years ago. In some cases (e.g. *Mitella polymerus* and *Aletes squamigerus*) the older names are also included in the index to this volume; but for others (e.g. *Acmaea digitalis* and several congeners, and *Lepidopa myops*) there is no hint about former identity. This is, of

course, not so much a complaint about this book as about the problems inherent in nomenclature, and it can be argued that such changes represent "progress"; but I sometimes wonder. . . .

The neophyte will find the chapter introductions and the detailed text entries in this book most useful; the more advanced student will find the literature citations a good starting point for a research project; everyone will, I think, enormously appreciate the coloured plates. The professional biologist who knows some part of the literature on a particular group or species will no doubt note — as I did —

cases of mistaken conclusions, incomplete or outdated citations, and unevenness in coverage. But it would be petty to emphasize such deficiencies. The central point is that this book is an indispensable addition to the library of all who are interested in the common intertidal invertebrates of the California shoreline — and it contains a very attractive collection of colour portraits for those who will never get there. □

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Old twists to an old tale

Jerry Donohue

The Double Helix: Text, Commentary, Reviews, Original Papers. Edited by Gunther S. Stent. Pp.298. ISBN hbk 0-297-77899-4; ISBN pbk 0-393-95075-1. (Weidenfeld & Nicolson/W.W. Norton: 1981.) Hbk £10; pbk \$5.95.

AN OLD proverb, as given by Gibbon, tells us that "happy is the nation that has no history". If this applies to substances, then DNA, surely, is the unhappiest of molecules, for it is the subject of innumerable biographies.

This new book contains: the complete *The Double Helix*; reprints of 12 reviews of it from 1968 and discussions of these by Gunther Stent; reprints of three papers from *Nature* by James Watson, Francis Crick and Aaron Klug; three letters to *Science* from 1968 by Max Perutz, M.H.F. Wilkins and Watson (see below); reprints of six papers from 1953–54 describing the original DNA work done in Cambridge and King's; and a name index the usefulness of which is somewhat marred by careless inaccuracies.

Should the fact that only about 5% of the contents of this new edition is previously unpublished material be cause for discontent? Probably not, as it brings together *The Double Helix* and some of the important writings it spawned. There are, however, other causes for discontent which are hardly contentious. The original text of *The Double Helix*, including the photographs and diagrams, ran to 233 pages; in this reprint these occupy 135 pages. This drastic compression was accomplished by abandoning the elegant format of starting each chapter on a new page; by taking the plates of the first (Signet, 1969) paperback edition, which was itself a compression of the original, and compressing it still further by putting more lines per page; and by reducing the sizes of the photographs, some by more than 50%, so that the individuals in the group photographs are virtually unrecognizable. Also reduced in size is the photographic reproduction of an historic

letter of Watson to Delbrück which is legible only with great difficulty and, in some places, totally undecipherable, a circumstance not caused by Watson's crabbed hand. Surely a book as important as this deserves a better production.

Something very mysterious should also be pointed out. In the diagram on p.111 the enol structure for thymine incorrectly has an oxygen atom where there should be a hydrogen atom. The same diagram on p.191 of the hardback first edition of *The Double Helix* is correct, and identical in every other respect, right down to the pattern of dots inside the rings of the bases. Now, admittedly, some of the early difficulties in cracking the DNA structure concerned the keto-enol business, but what sinister force, in Alexander Haig's terminology, took it upon itself to change a correct structure to an erroneous one?

There are several misconceptions and misreportings that ought to be dispelled before remarking on a few of the reviews included in this new volume.

● *The Myth of the Invention of Base Pairing.* Throughout the reviews runs the thread of the notion that Watson and Crick would never have discovered their structure had it not been for "rules" of Erwin Chargaff that in DNA (molar amounts of) cytosine = guanine and thymine = adenine. However, Chargaff himself had not yet referred to this relationship as rules, and, indeed, his own published analytical data gave merely the merest hint of these regularities, and he himself averred the possibility that the regularities might be accidental. Stent (pp. 168–169) has more to say on this.

The fault for these misconceptions must be squarely on the shoulders of Watson. We find him writing (p.75) "The moment [Spring 1952] was thus appropriate to think seriously about some curious regularities in DNA chemistry first observed by Chargaff", and then subsequently cheerfully disregarding them, and building a like-with-like, non-complementary, model. In addition, he had been assured by his friend, the biochemist Markham, that if

Cold Spring Harbor Laboratory have recently published the proceedings of the symposium on viral oncogenes which was held last year. The two-volume set of proceedings, comprising Vol. XLIV in the series *Symposia on Quantitative Biology*, is available from CSHL, Fulfillment Department, PO Box 100, Cold Spring Harbor, NY 11724, or through booksellers, price \$130 (\$156 outside the USA).

Review of James Watson's *The Double Helix* by J. Field

Hear the song of how the spiral
Complex, twisted, double, chiral
Was discovered. Its construction
Following some shrewd deduction
Was accomplished. From all sides "Oh
Tell it as it was" they cried so
Then he took his pen and paper
And composed the helix caper
Writing of the Cambridge popsy
Turned the world of science topsy
Turvy like had not before been
Done. The story is no more than
How the structure was discovered
Secret of the gene uncovered
Biological prediction
Better than a work of fiction
Stylistically breezy
Everything appeared made easy
And it was. The bases' pairing
Found at last — a small red herring
Notwithstanding. How he found them

And the hydrogens that bound them
Was a stroke more accidental
Than a work experimental
Chemistry was not his calling
He had read *one* book by Pauling
Models of the bases he'd made
With them on his desk top he'd played
Shuffling them and putting like with
Like he made no lucky strike with
Guanine, thymine, adenine. A
Letter came from Pasadena
Horrors! then the day was won for
Pauling's model was quite done for
Extra atoms he had set in
Where no atoms should be let in
With some phrases less than modest
Pauling's model's called the oddest
Back now to the basic pairing
How's the structure building faring?
Faster, faster goes the race, then
Everything falls into place when

Tautomers which nature chooses
Are the ones our author uses
Two chains (but here we can't be sure)
Plus this extra structure feature:
Pauling's outside bases inside
Pauling's inside phosphates outside
Now they all were quite ecstatic
Soon became they quite dogmatic
Having nature's secret later
Checked against the x-ray data
This was difficult, for Rosie
Found our scientist rather nosey
Bragg compares the book with Pepys. Is
This the verdict of Maurice? His
Thoughts alas we cannot gainsay
Trumpet blowing's not his forte
Tales like this do have a moral
Whether printed whether oral
Find the right man to advise you
Then you'll get a Nobel prize too.

Chargaff said that guanine equalled cytosine, he, Markham, was equally certain that it did not, because Chargaff's experimental methods inevitably underestimated the true amount of cytosine. On numerous other occasions Watson refers to Chargaff's "rules".

It is, on the other hand, abundantly clear from the narrative that these "rules" had nothing whatsoever to do with the construction of the DNA model. The model came first, and made the rules, not the other way around.

● *The Myth of the Undercover Agent.* In some of the reviews selected by Stent the role which Peter Pauling played is displayed as not quite what it should have been. Thus Watson, according to Merton (p.215), reports "with temerity and self-mocking wit the occasions on which Linus Pauling's son, Peter, then a student at Cambridge, became a prime source of information about what his father was up to". Morrison (p.176) says that the idea for the double helix came "... out of hearing from the young American expert upstairs [actually, I was not upstairs at all, but in the same office as Bob Parrish, Watson, Crick and Peter] that the textbook was probably wrong, or out of rumors about what Pauling was thinking — relayed by his son Peter at Cambridge". Lear (p.195) goes considerably further in stating that Watson "discloses how he used his young friend Peter Pauling to spy on Pauling's brilliant father, Linus". And Lwoff (p.227) observes that "Peter Pauling works in the Cavendish, receives detailed letters from Pasadena and informs his colleagues of the evolution of his father's work".

On careful reading of *The Double Helix* we find that Watson says nothing of the sort. Consult, if you will, the name index for the citations to verify this, and come to the conclusion that these statements and innuendoes in the reviews are pure fiction. So much for spies!

● *The Myth of the Race.* Another

thread, nay, hawser, which twines through the reviews, is the "breathless race" between Watson and Crick on the one hand, and Linus Pauling on the other, towards that legendary ceremony, established by the dynamite tycoon, in the gloomy, sunless Stockholm of December. The same strand snakes its way through *The Double Helix*. How, though, can there be a race with only one team and no antagonist? At this stage (1952–1953), Pauling, whose reputation as a scientist was unequalled, had well over 200 publications under his belt, including a dozen or more books universally appreciated and widely read. His ticket to Sweden was long overdue. (He did, in fact, make this journey the very next year in 1954 to collect his first Nobel prize.) Was he racing Watson? Certainly not. To Pauling, in Peter's view (*New Scientist*, May 31 1973, p.558), DNA was just another chemical, intrinsically equal in interest to, say, sodium chloride; both presented interesting structural problems, so why race with DNA?

● *The Myth of the Filched Photographs and the Dipped Data.* The fourth and final myth concerns the X-ray data, and whether Watson and Crick came by them by unorthodox methods. Among the reviewers we find Sinsheimer referring to "caded data" and a "privileged report", Lear to a "clandestine communication channel" and supposedly "confidential data", and Merton to "an odd expedient for gaining access to badly needed information". Lwoff and Chargaff (in his review, not included in this volume) quote a crucial paragraph from *The Double Helix* which led to both of them to comment on ethics. The same passage is reproduced by Perutz, the supposedly clandestine channel, in his letter, pp.207–210, in which, as well in the letters by Wilkins and Watson which follow, it is made abundantly clear that the report was not confidential, and the data had already been

disclosed in open seminars.

As a matter of fact, an X-ray diffraction photograph which, although not of the high quality of those produced by the two groups at King's, clearly shows the characteristic helical pattern of the B structure, had been published by Astbury years earlier, so all the fuss over stolen data was really unnecessary.

Five of the reviews of *The Double Helix* included here were previously reviewed by Gunther Stent, the editor. The article by Chargaff, that *vieux terrible* of the DNA saga, although not reprinted because he withheld permission, is reviewed. (Anyone wishing to read the original will find it in *Science* 29 March 1968; p.1448). Of the seven new reviews, one of them warrants comment, that by the bipseudonymenous "F.X.S.", originally "F.R.S.". This is not a review at all, but verbal virtuosity by a polylingual sesquipedalianist, and why Stent chose to include it is an enigma, as it adds little insight into DNA or those connected with it. F.X.S. is coy about his identity, but really, who cares? His piece is worth reading, but hardly reprinting.

There are numerous other important reviews not included by Stent — by Lord Todd in *Chemistry in Britain*, Sokolov in *Newsweek*, Bernstein in *The New Yorker*, Maddox in *Nature* and so forth. Another not selected by Stent, by J. Field, appeared in the *Journal of Irreproducible Results*, (Vol.17, December 1968, p.53). Because it expresses as well as anything the flavour of Watson's book, it is reproduced above (with the permission of the editor).

Anyone who hasn't yet read *The Double Helix* should certainly do so, but, even if you own a copy, the additional material in the present volume amply justifies its purchase. □

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