

Citation patterns of the four seminal DNA double-helix model papers by Watson and Crick in 1953–54

Sachi Sri Kantha

Properly citing the contributions of rival groups in research papers is a contentious issue which touches many inter-related themes such as due acknowledgement, priority and deception. To felicitate the 60th anniversary of the announcement of the accurate double helix model of DNA by James Watson and Francis Crick, I analysed the citation patterns of rival groups in their four seminal papers that they published in 1953 and 1954. I found that proper citations of the previous model-building studies of William Astbury and Bruce Fraser appear somewhat sloppy in the four seminal papers.

The 60th anniversary of one of the most influential publications in biomedical sciences, entitled 'A structure for deoxy-ribose nucleic acid' by James Watson and Francis Crick¹ was celebrated on 25 April 2013. The significance and relevance of this brief note by Watson and Crick has been discussed in numerous books, memoirs, editorials and assorted media publications. This seminal publication was followed by three additional contributions by the same pair in 1953 and the following year^{2–4}.

A recurrent criticism about Watson and Crick on the guile they used to collect the raw, unpublished data on X-ray crystallography as well as related information from sources like personal letters from rival scientists such as Linus Pauling and Rosalind Franklin prevail^{5,6}. Another aggrieved party was biochemist Erwin Chargaff⁷. But his biochemical data had been published; as such Watson and Crick made valuable deductive use of Chargaff's published data. In this study, I focus on the largely ignored theme relating to the citation patterns of rival scientists in their four seminal publications (especially that of Robert Donald Bruce Fraser). Was the approach

adopted by Watson and Crick, in reporting the 'great discovery in biology'⁸ above reproach?

Method

I reviewed the published literature: autobiographical reminiscences of three principal scientists (namely Watson⁹, Crick¹⁰, and Maurice Wilkins¹¹) and biographies of Rosalind Franklin⁶ and Crick¹² as well as related compilations¹³ for background data covering the events between 1951 and 1953.

Comment

Table 1 provides a compilation of the number of publications cited by Watson and Crick in their four seminal papers. A few salient themes can be observed. First, the number of total citations shows an increase from 7 to 38. Secondly, five rival groups could be identified. Among these, Astbury, Franklin, Fraser and Pauling were the four rival model-builders for DNA. Chargaff, a biochemist, was less interested in this angle of

DNA research. Thirdly, while the total citations to the papers of Pauling and Franklin are seven each, and Astbury was cited five times, that of Fraser appears reduced and somewhat deceptive. Why?

To probe this issue, I quote below the relevant interpretations that appeared later in the autobiographies and biographies of participants in the race to propose an accurate structure for the DNA. Watson's version appeared in the final chapter of his 1968 memoir⁹. To quote, 'In the next week the first drafts of our *Nature* paper got handed out and two were sent down to London for comments from Maurice and Rosy. They had no real objections except for wanting us to mention that Fraser in their lab had considered hydrogen-bonded bases prior to our work. His schemes, until then unknown to us in detail, always dealt with groups of three bases, hydrogen-bonded in the middle, many of which we now knew to be in the wrong tautomeric forms. Thus his idea did not seem worth resurrecting only to be quickly buried. However, when Maurice sounded upset at our objection, we added the necessary reference.' Maurice and Rosy refer to Maurice Wilkins and Rosalind Franklin respectively. Though Watson mentions that the 'necessary reference' as added, it was not exactly so for the very first short report that appeared in the 25 April 1953 issue of *Nature*. Fraser's work was mentioned in the main text as follows: 'Another three-chain structure has also been suggested by Fraser (in the press). In his model the phosphates are on the outside and the bases on the inside, linked together by hydrogen bonds. This structure as described is rather ill-defined, and for this reason we shall not comment on it.' But Fraser's study was not referenced at the end among the seven references. In

Table 1. Number of publications cited in the four seminal papers by Watson and Crick

Parameters	<i>Nature</i> (25 April 1953)	<i>Nature</i> (30 May 1953)	CSHS ^a (1953)	PRSL ^b (1954)
Total citations	7	11	26	38
Astbury-cited ^c	1	1	1	2
Chargaff-cited	1	1	1	2
Franklin-cited	0	1	3	3
Fraser-cited	0	1	1	0
Pauling-cited	2	2	1	2
Self-citations	0	1	3	3

^aCSHS, *Cold Spring Harbor Symposia on Quantitative Biology*.

^bPRSL, *Proceedings of the Royal Society (London)*.

^c'Cited' in this column, wherever it appears, refers to complete citations that appear in the references section of the four publications.

addition, Fraser's report of his work was not submitted to any journal⁶!

Franklin's biographer Maddox⁶ had described the events that transpired during March 1953. I quote two short paragraphs from her book. Note that reference to King relates to King's College, London where Wilkins and Franklin were affiliated. 'In the ensuing panic to get King's papers ready, Wilkins cabled – a dramatic gesture in those austere times – Bruce Fraser in Australia and asked him to write up his model as quickly as possible in a note suitable for publication in *Nature*. Fraser complied. In view of the urgency, he sat up all night typing the very paper Wilkins had suppressed two years earlier and sketching the diagrams. There were no photocopying machines in 1953, and had Fraser not done it by hand, the manuscript would have lain several days in a queue before being photographed for duplication. Then, even more expensively, Fraser had his work cabled off to London next morning.'

'But when the Fraser paper arrived, Crick vetoed it: what was the point of publishing wrong data? Instead, he and Watson appended an acknowledgement to Fraser at the end of their own paper, describing the Fraser model as "rather ill-defined" and the Fraser paper as "in the press". It was not, and was never published⁶.'

Thus, there is the spurious citation to Fraser's work (as 'in the press') in the text of the short paper that appeared in the 25 April 1953 issue of *Nature* and also included as part of reference 3 (as 'in preparation') in the longer paper that appeared in the 30 May 1953 issue of *Nature*. In fact, Bruce Fraser with his wife Mary Fraser had published a preliminary account of his study on nucleic acid in *Nature* two years before¹⁴. In fairness to Fraser, one may think that Watson and Crick should have cited this account in their first two seminal papers^{1,2}. This Fraser and Fraser account does receive citation once in the third seminal paper³.

Peculiarly, the Fraser episode is not at all mentioned by Crick in his 1988 autobiography¹⁰. But Crick's biographer Olby¹² had described Crick's reluctance in including Fraser's study as follows: '... He (Crick) drew the line at Wilkin's request to include yet another contribution – this time from a former colleague of Wilkins. This was Bruce Fraser, who more than 1 year earlier had put together

a three-stranded model of DNA with its sugar-phosphate chains on the outside and the three sets of bases on the inside, hydrogen-bonded to one another.' About Crick's reluctance, Olby had mildly stated without any critical comments, 'Here we witness Crick in the business of directing, making decisions and demanding standards, much as he must have been when in charge of the firing mechanisms for mines during the war.' The war analogy seems apt to an extent; to win the 'race' in being the first to propose a double-helical structure of DNA, by trampling on the priority concerns of rival groups.

In his autobiography, Wilkins¹¹ refers to Fraser's DNA model (which anticipated the Watson and Crick model by one year) favourably. Rather than paraphrasing isolated selections, I quote the relevant paragraph. 'The strength of our position was illustrated by a model of DNA that Bruce Fraser built. He was a research student in Bill Price's group and used infra-red absorption to study the chemical bonds in DNA ... Fraser worked in the room next to mine and, soon after the colloquium, he appeared at my door with a mysterious smile and beckoned me silently. Following him into his room, I saw that he had built a helical model of DNA. Bruce had done a good job: the model was very interesting. The three helical chains had the right pitch, diameter and angle, and were linked together by hydrogen bonds between the flat bases which were stacked on each other in the middle of the model. But the three chains were equally spaced, and that was contradicted by the X-ray diffractions. There were also basic difficulties with the hydrogen bonding between the bases: the bonds could only exist for special groups of three bases. The structure did not fit with Chargaff's 1 : 1 base ratios.'

Citations to Astbury's work on DNA before the Second World War, in the four seminal papers by Watson and Crick also deserve a check. Compared to Fraser's work, Astbury's earlier studies had received a total of five citations in the four seminal papers. However, citation to Astbury's earlier work with Florence Bell which was published in *Nature*¹⁵ in 1938 appears only in the fourth seminal paper⁴, and was excluded in the first three seminal papers. What deserves mention is the inference paragraph in Astbury and Bell's report of 1938, which had marginally anticipated the much

popularized Crick's 1953 version. 'It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material.'

Considering the facts that Astbury and Bell's report preceded the experimental findings of Oswald Avery's group¹⁶ and that of Chargaff's base pair rules¹⁷, one may consider the following sentences prophetic in recognizing the genetic role of DNA. 'The significance of these findings for chromosome structure and behavior will be obvious. It seems difficult to believe that it is no more than a coincidence that thymonucleic acid consists of a long succession of nucleotides spaced at a distance so nearly equal to that of the long succession of amino-acid residues in a fully extended polypeptide. Rather is it a stimulating thought that probably the interplay of proteins and nucleic acids in the chromosomes is largely based on this very fact, and that some critical stage of mitosis, involving elongation of the protein chains, is realized in close cooperation with the dominating period of the interacting nucleotides.¹⁵'

Conclusion

Here are my two inferences. First, it appears to me that proper citations to the previous studies of Astbury and Fraser (even though some of the data presented may be incomplete and erroneous) appear somewhat sloppy in the four seminal papers on DNA structure by Watson and Crick. Secondly, one never knows whether it was an inadvertent oversight as the first two papers in *Nature* were produced in haste, or an intentional act by two young researchers to claim greater credit towards their finding. Lastly, I emphasize that my criticism on the citation patterns of Watson and Crick in their four seminal papers are not a nitpicking attempt to smear their deserved fame. I give due credit for their tenacity and achievement in solving an important problem faced by biologists in the early 1950s. But, the citation patterns practised by them in these four seminal papers were incomplete and less correct.

1. Watson, J. D. and Crick, F. H. C., *Nature*, 1953, **171**, 737–738.
2. Watson, J. D. and Crick, F. H. C., *Nature*, 1953, **171**, 964–967.

-
3. Watson, J. D. and Crick, F. H. C., *Cold Spring Harbor Symp. Quant. Biol.*, 1953, **18**, 123–131.
 4. Crick, F. H. C. and Watson, J. D., *Proc. R. Soc. London, Ser. A*, 1954, **223**, 80–96.
 5. Sayre, A., *Rosalind Franklin & DNA*, Norton, New York, 1975.
 6. Maddox, B., *Rosalind Franklin – The Dark Lady of DNA*, Harper Collins Publishers, London, 2002.
 7. Chargaff, E., *Science*, 1971, **172**, 637–642.
 8. Lwoff, A., *Science*, 1968, **219**, 133–138.
 9. Watson, J. D., *The Double Helix – A Personal Account of the Discovery of the Structure of DNA*, Atheneum, New York, 1968.
 10. Crick, F., *What Mad Pursuit – A Personal View of Scientific Discovery*, Basic Books Inc., New York, 1988.
 11. Wilkins, M., *The Third Man of the Double Helix: The Autobiography of Maurice Wilkins*, Oxford University Press, Oxford, 2003.
 12. Olby, R., *Francis Crick – Hunter of Life's Secrets*, Cold Spring Harbor Laboratory Press, Cold Spring Harbor, New York, 2009.
 13. Stent, G. (ed.), *The Double Helix* (Text, Commentary, Reviews, Original Papers), Norton & Co, New York, 1980.
 14. Fraser, M. J. and Fraser, R. D. B., *Nature*, 1951, **167**, 761–762.
 15. Astbury, W. T. and Bell, F. O., *Nature*, 1938, **141**, 747–748.
 16. Avery, O. T., MacLeod, C. M. and McCarthy, M., *J. Exp. Med.*, 1944, **79**, 137–158.
 17. Chargaff, E., *Experientia*, 1950, **6**, 201–209.
-
- Sachi Sri Kantha is in the Center for General Education, Gifu University, 1-1 Yanagido, Gifu City 501-1193, Japan.
e-mail: srikanth@gifu-u.ac.jp*
-