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NOT FOR PUBLICATION

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Dear Francis,

Yes, indeed, I did groan, but I am replying quickly to put an end (?) to this, at least for the time being.

I agree with you that there has been an over-reaction to Jim's book, in which Rosalind's qualities and achievements count less than her position in the story, as exemplified by Ann Sayre's book. However, I wonder whether you are not in turn reacting to this "misguided movement" and playing down her qualities. I believe that Rosalind was a first rate scientist, using the term "first rate" in the ordinary sense. If however, you confine the term to a select few like Bragg or Pauling, then clearly she was not.

She was an excellent experimentalist - sorting out the forms of DNA was witness enough, and confirmed by her work on TMV. She was also a good analyst with a pretty powerful mind and her knowledge of theoretical crystallography was far better than average. One can see this very clearly in her papers on carbons in Acta Crystallographica in 1950, and 1951, where both the quality of the experimental work and the analysis were models for years to come - she transformed a very messy field and discovered the difference between those carbons which form graphite and those which do not, something which has in these days of carbon fibres proved to be of technological importance.

I agree with you that Rosalind was the cautious type of scientist in her tactics, but strategically she was prepared to tackle large and difficult problems - in that sense she took risks. In the work on DNA she did not at first try to go beyond the crystallographic analysis, though I think it is because she knew very well that she was no Pauling (or, for that matter as it turned out, no Crick).

What distinguishes her from the select few was that she was not highly imaginative; but how many well known scientists are? This does not stop them making discoveries in their own way. You and I have often discussed in the past how long it would have taken Rosalind to solve the structure. We discussed whether she was one or two steps behind, but you have often said that she would have got it out on her own, step by step to be sure, e.g. firstly the two chains of a double helix, the separation between the chains, then the diad which she almost had, and finally to make the step from base interchangability to base

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pairing. As you once wrote, the structure would then have leaked out rather than coming out dramatically in the way that you and Jim solved it. So, in historical terms, it would have been a difference in style: there would have been no dash to the Pole. But then in those days, Rosalind and most scientists of her generation would not have thought of it as a race or as a dash. I think finally what her failing comes to is that she lacked the imagination to recognise the truth of what you had been telling her, and, moreover, stuck to the safer, analytical approach - what you call "too sound". She had no collaborator to help pull her out of the groove she had dug, but it is clear that by early 1953 she had already begun to do so by her own efforts.

Yours ever,

A handwritten signature in cursive script, appearing to read 'A. Klug'.

A. Klug

P.S. I would not mind if you send this to Charlotte Friend, but I would not like this letter to be used publicly.

P.P.S. Please could I have a reprint of your paper with Wang and Bauer.

P.P.S. Have just received copy of your letter of October 3 to the Editor of "The Sciences". I'm glad you have put this right.