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Miss Rosalind Franklin,
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Birkbeck College,
Torrington Square,
London, W.C.1.

Dear Miss Franklin,

I am returning the two papers which you so kindly lent to Jim, which as you can imagine I read with very great interest. I am sorry not to have sent them before, but Jim and I had to write a paper before he left for the States, and this kept us very busy. I am enclosing a few comments on the papers and some longer remarks on salt, on Riley and Oster's results, and on calculating structure factors. What a problem it is!.

With best wishes,

Yours sincerely,

Francis Crick

F. H. C. Crick.

NOTES ON TWO PAPERS BY FRANKLIN AND GOSLING

Paper I

p. 5 I am not really clear about fibres which only give Structure B. How do the water content, intensities and the equatorial spacing change with different R.H.?

p. 7 Comparison with Riley & Oster. I take it this is Column 4 of their Table 1, or is it Column 5?

p. 9 I agree that the phosphates must be accessible. The general evidence suggesting they are on the "outside" seems plausible, but less compelling.

p.12 I had not realised that calf thymus was the only material to give Structure A. Is this still true? Is it perhaps due to the superior method of preparation used for calf thymus?

Paper II

I am not quite clear how one can be certain that the unit cell is truly face-centered monoclinic, and not really triclinic, with two angles 90°. The point is important because if the unit cell is strictly C_{2}^{2} one must have the DNA chains in pairs, running in opposite directions.

p.11 I take it that "12.4 Å" is a slip.

p.11 Jerry Donohue is worried because in Fig. 3 there is so little vector density at x = 40 Å, z = o. A more natural choice would have been x = 38, z = o. As you do not list your observed equatorial spacings, it is difficult to see how things would fit (Incidentally 22 x 3 = 38).. Is the effect perhaps due to the negative contributions from the peaks near x = 44, z = o.?

Over.

p.15 As you know, we have never believed the anti-helical implications of the last paragraph, because of polybenzyl glutamate. Would you call an 11-fold axis a high degree of symmetry?

ADDITIONAL NOTES

Calculation of Intensities

Jim tells me that you claimed that "the water had only a general lowering effect, and that the Na+ was negligible". This is not strictly correct. For calculating structure factors for the longer spacings it is convenient to allow for the water by taking the average electron density of water as the "zero" of electron density. Thus any group which has the same electron density as water makes no contribution. For each group its average electron density must be calculated, and that of water subtracted, before its contribution can be given its proper weight. Thus, if the electron density of a base were, say, 1.2 times that of water, only a fraction ($\frac{0.2}{1.2}$) of its electrons would be counted. On the other hand the Na+ ion, due to strong electrostrictive, probably has a very small (or negative?) partial specific volume, and thus almost all of its electrons will count. Thus the effect of one Na+ may be about the same as that of a base. It is this necessity for allowing for the water which makes structure factor calculations difficult.

Riley and Oster

There seems little doubt that at least some of R. and O's long spacings are genuine, and it is a pity that in studying Structure B the backstop in your experiment was placed so that it would hide any such spacings, though one can see you wanted short exposures.

It is interesting to note that the spacings in your Paper 1, plate 6, are almost exactly in the ratio of $3\bar{1}40$ and $4\bar{0}40$ suggested by R. and O. If it were not for this complication of a longer spacing, one could use their results to obtain the number of

chains in Structure B. Moreover the changes of the intensities of the equatorial reflexions as the spacings increase would be very informative. I don't feel much progress can be made with Structure B until the long-spacing position is cleared up. I surmise the micelle structure is caused by the 10-fold screw axis of the fibre trying to give a hexagonal pack and not quite making it.

SALT

I notice that your plate 5, paper 1, shows "spots". Are these due to salt in the specimen, or to some other cause? Oster and Riley had a NaCl ring in one of their speciments, and also got a sharp 3.25 A reflexion which looks fishy to me. The reason I ask is as follows. The equatorial spacings for Structure B appear to show one main spot (neglecting doubling for the moment). If the structure is pseudo-hexagonal this is likely to be either $m+$ or $\bar{m}$. If $+$, it means a great lump at the origin of the C projection. This could only be phosphates near the centre. If $\bar{m}$, it means a lot of material between the helices (unless your suggestion of helices in the trigonal position is correct; even then I am not clear if it would give the right answer). Part of this interhelical material could be the Na+, but this seems hardly enough. However, if some extra salt were there this might be sufficient. Moreover, it would explain a lot of the puzzling density discrepencies which are difficult to account for in the model. Of course, you add distilled water when making the fibre, but is it quite certain that, in the specimens which give X-ray photos, you have only the same amount of Na+ (or other ions) as $PO_4^{\equiv}$? The dilemma is a very real one because in effect one is deducing one "radius" from the general reflexions and another from the equatorials

This suggests that there is something non-helical in the structure. This is likely to be where adjacent helices interact. A structure which is cylindrical contributes nothing to the general reflexions but alters the equatorials, and the most likely thing to do this is the ions in the water.