12 October 1976

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Dear Francis,

Thank you for all the letters on the various topics. I am writing in haste so that you will at least have minimum answers.

1. Our paper on the origins of protein synthesis.

I agree with most of the comments, and Annette is doing a new drawing for figure 1, but wouldn't it be a lot simpler if you revised the manuscript and sent it in to the editor? I don't have any of the formal referees' reports (except for a copy of Woese's letter) and you will have all the information necessary when I send a print of Annette's new drawing.

2. Kinking and related matters

I have not yet seen Michael Levitt, but will raise the matter. The difficulty is that if it is the water or the histone that is going to do the determining, then calculations aren't of much use. One can already see that the Sobell kink is rather crowded.

3. Bisulphite

I did know that for the chemical modification studies it is used at about 3M, so I don't think the quantity we add (20-50 mM at most) could affect the preparation. Moreover, I don't see how it can be used for studying nucleosomes since at this ionic strength presumably the histones will come off. I will answer the question about the detailed attack on tRNA later.

I like your idea of the 5MeC being there to prevent some kind of attack.

4. Size of the core particle

Len hasn't done anything more on this but, while he was away, he let a long autoradiograph stand which gives better resolution than fluorography. A quick inspection of this suggests that his results will stand, but he is going to rerun the markers and the nucleosome DNA on the same track to make sure that nothing funny has gone wrong.

5. Packing of the crystals

I am sorry I omitted to give the space group. In fact the PS of your letter of 8 October (which arrived today) shows that you have got our idea right. I enclose a neat drawing by John Finch.

Attempts to measure the density have failed. The crystals are too small.

I don't like the pitch of 65 Å much either but could you point to the fault in the logic of my argument. If there is such a thing as a screw down position for 140 or 150 base pairs, then surely there must be an expansion of length going from 1 6/9 turns to 2 complete turns.

6. Bak's work

Haven't had time to digest this but, at the Jerusalem meeting, Hans Ris described mitotic chromosomes of 1000-2000 Å diameter. There is a good deal of literature about this and I would like to find out what is so distinctive about Bak's work, of which I agree I have not seen as impressive a picture as his 2n.

7. The Laemmli material -

John has continued taking X-ray pictures of this. What one sees are '55' and '38' Å bands with the shoulder at 100, but the '27' Å is not visible. The latest pictures show some orientation of the '38' Å band on the equator! We are puzzled by this because if the 300 Å fibre is oriented parallel to the microscopic fibre, then one would expect this spacing to correspond to some repeat along the nucleofilament. 38 Å is surely too small for the pitch of a Sobell-type helix on the nucleosome since it couldn't span a distance of 113 Å in 1 2/3 turns. Perhaps it is a fluctuation in the histone arrangement. Alternatively, the material may be trying to coil up into the next order so we may have our directions mixed up relative to the solenoid axis.

Yours ever,

A. Klug

Enc.