15 October 1976

Dr. F. H. C. Crick, F.R.S.,
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Dear Francis,

Continuing my letter of 12 October:-

8. Skeletal models

Half a dozen pairs were sent to you on 13 October by air parcel post. An up-to-date price list will follow in a few days.

9. Bending and kinking

I have now prodded Michael, and indeed he is set up to do some calculations. His quick answer is that bending to a small radius might be quite feasible as judged by the energy of superhelical coiling. From this, one can get an estimate of the energy for bending using a Poissons ratio of 2.

In the lab. here, things go slowly. We are trying to produce "modified" core particles which will crystallise easily. The first experiments have given microcrystals, but clearly a lot more systematic work is called for. We are short of "person power" as I explained.

Len Lutter is redoing the 5' end labelling experiments to see whether he can get more than the 50% labelling he has achieved up to now. He says that this needs to be done in order to avoid the criticism that his results might be consistent with a polar structure which is what Simpson at NIH argued at the Gordon Conference. I don't follow the logic of all this, but Len says while people like you and I might think a dyad absolutely natural, others don't and would like to see more conclusive evidence.

At a chromatin meeting a few weeks ago, Olaf Pongs described what he had been doing on Drosophila. He finds that the repeat is the same in all tissues he tried and is not distinguishable from that in rat liver. He is writing it up but I must say I thought the quality of his gels wasn't as good as some others I have seen. However perhaps they are not worse than Ron Morris'.

Joel Gottesfeld gave me a detailed two hour description of his work on the active particles. He is now writing it up since he will not do any more as John Gurdon is of course pressing him, and moreover the next round of work on this problem is clearly going to be a long and difficult one and needs a special touch. I don't know how much

of it you have already heard, but the paradox is that nuclease S1 digests the active particles, leaving pieces of DNA of about 30 base pairs and less, without a trace of any discrete bands in the gel, so it looks as though, from the point of this enzyme, most of the DNA is single stranded (on occasions he has found more than 60% of the DNA in the acid soluble fraction). On the other hand, the DNAase 1 digests give a perfectly regular pattern, just like ordinary 140 base pair particles (although the rate of digestion is greater than in the non-transcribing particles). So, from the point of view of this enzyme, most of the material appears to have a regular substructure. I can't think what is going on. Perhaps one of the strands of the DNA is complexed to the RNA over a good proportion of its length, while the other strand sits in the grooves in the histone so that it is regular from the point of DNAase 1. I am baffled. I don't know who might take up the problem. Perhaps Barry Honda, whom Ron Laskey has at last got involved in some of the transcriptional aspects of the SV40 work.

I have seen almost nothing of Vaughan Jackson but understand that he is going on with the formaldehyde cross-linking on chromatin and not, so far, with the core particles, as we discussed. Sidney tells me he is rather worried about him in his isolation on the second floor, and I am looking into the possibility of switching people around so that we find him some space nearer Len. However I have my hands so full that I really can't give him the attention that is required.

Yours ever,

A. Klug

P.S. Prints of the new figure 1 enclosed.