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GARAGE

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Dear Dr Heidelberger:

I certainly seem to be learning things very rapidly on my trip — especially about the limitations of the centrifuge methods. Unfortunately there was very little discussion about assumptions etc at Upsala, so I was not at all aware of any uncertainty in giving the values found as the true molecular weight. Bernal, MacFarlane and Pirie, however, have pointed out the fact very definitely, that S and D can only be used to calculate M for a rigid spherical particle, and in these cases f/f_0 should be 1.0. In all other cases of asymmetric or non-compact molecules one cannot assume $M = \frac{RTs}{D(1-\bar{\nu}\rho)}$ to hold since one gets an oriented sedimentation and hence viscosity effects due to the sliding of one molecule along another — whether this is eliminated by running the s concentration curve and extrapolating to ~~zero~~ zero protein concentration, I have not as yet considered thoroughly. I am convinced, however, that with the horse, cow, + pig having f/f_0 of 2.0 a molecule 25 X as long as it is wide and rabbit, monkey and human $f/f_0 = 1.5$ a molecule ca 10 X as long as wide, that the actual molecular weights are meaningless and I think I shall have to be much more cautious in the statements about molecular weight. The bulk of the data, is of course just as important without a true molecular weight figure.

I don't think I mentioned but in the <u>two</u> Ea antisera studied

in the inhibition zone, I found the same ratio of $\frac{Ab^{mg}}{Ea^{mg}}$ in the compound independent of the amount of excess Ea with 160,000 ± 40,000 for the mol. wt., this gives $Ea_3 Ab_2$. The sedimentation constants of the ~~so~~ two compounds formed are ca 8.7, 11.3 and one much too low for such an empirical formula. In view of my talks with the English workers, I am not sure whether it would not be very rash to assume 160,000 for M and that perhaps the ratio might be 1:1 which would be more reasonable.

In any case, apparently Bernal is the only person able to calculate a true molecular weight for an asymmetric molecule from X Ray patterns. I have ~~followed~~ watched his technic for making dried films to orient the molecules and it would be very easy for us to send over films of antibody and polysaccharide for him to study. He would be very much interested in doing so.

From my conversations with Adair, I am very much surprised that the osmotic pressure method is not in routine use in every lab for molecular weight determinations. I think that it would be of very great interest to measure the mol. wt of the two types of antibody in this manner for comparison with the centrifuge. It requires so little time and attention, that perhaps we could do something about it when I get back. A control run in the centrifuge to see if we had homogeneous products would be necessary.

I have been trying to convince Pirie that quantitative precipitins would be of use in his work, but he says that all the junk in the tobacco juice comes down with the precipitate.

I read Shaffer and Digel's paper but am not convinced that our degraded antibody explains their results.

I am not going to send any of the three papers off, until you have gone over them. I haven't got all the figures yet, but if I get them soon, I'll send you the first two.

I hope you are having a very pleasant summer. Best wishes to Mrs Heidelberger and Charlie.

Sincerely Elvin.