

MRC

Medical Research Council

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Dr. Alexander Rich
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Dear Alex

I have now had the opportunity of speaking with Aaron. There is no doubt in anyone's mind that by the time of the Steenbock meeting Kim had progressed a considerable way towards the model you presented in Science; exactly how far it is difficult to determine. As far as I can see there is little evidence that, for example, you had at that date actually incorporated the correct ternary interactions of A₉ into your model nor that you had revised the structure of the anticodon loop to its present form. However the more general criticism is Kim's interpretation was not sufficiently convincing to you for you to have published it without the stimulus of the work here. The revised model is so superior to the earlier ones that it is difficult to see why if you saw its advantages so clearly as early as April you did not publish it sooner. To an outsider it appears that whatever you may have been considering you only decided on publication when you realised that Kim's interpretation was supported by the English one.

Basically I feel this springs from the fact that English Fourier is significantly superior to yours. I am at the moment unimpressed by the claim that Kim had improved yours much by his bootstrap method, since according to the English model you still have a series of misinterpretations in your Science paper though only time will show if these errors are real. Consequently it is easy for you to guess a structure but rather difficult to have confidence that your guess is correct.

However I think that all these questions about who exactly said what are not very profitable. Max, David and I feel that a note by you in Science in the enclosed form would meet the case and if you do publish it we shall regard the matter as closed and Aaron will be satisfied.

On the larger issue I think you should realise that this unfortunate incident was partly brought about by the reputation you have acquired. I think that Aaron was mistaken in not allowing the complete details of the English model to be disclosed publicly at Madison. Either everything should have been told or a paper should not have been given at all. The main reason for all this regrettable lack of candor between our lab and yours is that there have been other occasions in the past on which your behaviour over priority has upset people. I think you would be wise if in future you took especial care that you respect other people's priority and, what is equally important, are clearly seen to be respecting it. You already have an established scientific reputation and I think you should try to bend over backwards to acknowledge the ideas and influence of others, not only about work in other laboratories but also by junior people in your own. I know from personal experience how very import-

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who can come to feel, rightly or wrongly, that one is stealing their ideas.

About the structure itself, I think that a better Fourier would clearly be an advantage and I hope in future that any group publishing a model will make co-ordinates available, either rough or refined, to other workers in the field. In the long run what matters is not who first glimpsed the structure but that the structure is both known in detail and known to be correct. I feel that this is the target which should be aimed at.

Odile and I are leaving shortly for the South of France for a two-week holiday. After that I plan to go for one week to a meeting on chromatin on the island of Port Cros, and then back to Cambridge at the end of the month.

Yours ever,

Francis

F. H. C. Crick

Enc.