

5th December, 1974

Dr. Alexander Rich
Department of Biology
Massachusetts Institute of Technology
Cambridge
Mass. 02139, U.S.A.

Dear Alex

Shortly after your last letter arrived Max came to speak to me to discuss whether he should write a letter to New Scientist along the lines you had suggested. I had to tell him that I was strongly against such a step. Any such letter would have to be agreed by both parties, otherwise it would lead to further public exchanges. I told Max that I thought it most unlikely that any such agreement could be reached and that in any case preparing the draft would certainly lead to further acrimonious private exchanges. Finally, I am against any further publication of any sort as it only draws people's attention again to a matter they would otherwise forget. I should point out that both sides feel that the article maligned them - see, for example, the final paragraph of the New Scientist article. Sydney, who happened to be present, independently advised Max as I did. Max has agreed to write to you in this sense.

I also think it would be better if you stopped trying to convince yourself that you are the only injured party. Everybody now agrees that Kim had independently gone a long way towards the present structure. There is a difference of opinion as to whether he had gone all the way by the time of the Madison meeting, but this is a fairly small matter and could be cleared up if I exerted myself by cross-examining all the people concerned. I am reluctant to do this unless it is considered essential.

The major accusation against you personally is not that you gave an incomplete account of your new structure at Madison but that you described your old structure in public while, in private, selected people there were told about Kim's new structure. I have listened to the tapes and there is little doubt that you described a structure with the G15-C48 base-pair as W-C and not reversed W-C. Moreover you have admitted in your considered letter to me dated 9th August (Page Six: "I feel I made.....competitive spirit) that your action "was the root cause of all the misunderstanding". People like Brian Clark, who are familiar with the details of tRNA, could see that in your talk you were describing, albeit rather vaguely, your old structure. Yet within a few weeks you had submitted your new one for publication. No wonder my colleagues were upset. Even though subsequent knowledge has shown that they misjudged you, the fault for the misunderstanding was largely your own, though they were also somewhat to blame for not giving full details of their structure at Madison.

If any public statement is to be made I shall feel bound to insist that the above facts, plus your admission of blame, be put on public record. Since I consider this to be

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entirely against your own interests I would strongly advise you to agree, as soon as possible, to the proposal in my letter of 22nd October. Although you may feel that your reputation has suffered, my information is that there is now considerable sympathy for you in scientific circles because it is felt by many that the original changes against you are unfair. As this is in part true, I suggest you leave it that way rather than risk antagonising people again as might well happen if a detailed description of your actions at Madison was spelt out in public.

Perhaps, after reflection, you would let me know whether you and your collaborators are now prepared to agree to the fifth paragraph in my letter to you of 22nd October. We could then go on to consider matters like the exchange of co-ordinates, etc.

F.H.C. Crick