August 13, 1974

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Dear Dr. Adams,

Thank you for your letter of August 5th and for your fascinating chapter on the emergence of molecular biology in the Soviet Union.

You write from a base of scholarship that I can hardly compete with and I certainly do admire the cautions against invoking over-simplified explanations that would put the whole weight of the impediment to advances in modern biology in the Soviet Union on the shoulders of Lysenkoism. To the extent that many others have followed just that line, your strictures deserveeto be stated as vehemently as you have done. I am not sure just how others will view them. For my own part, I wonder if they are perhaps not slightly overdone, or to put it differently that a more synthetic model might be stated more concretely. The difference in our perspectives, if there is any, may stem from your emphasis on the period from about 1958 and thereafter which coincides with the actual liberation of Soviet biological science from the Lysenkoist dogma. My own views were formed and most of my anecdotal evidence would pre-date that. Certainly, during the late 40's and early 50's one could hardly hear any statement from microbiologists interested in problems of variation that did not explicitly echo the Lysenko doctrine.

The particular reference that I have been groping for is, I now recall, to be found in Priroda but unfortunately I still do not have an exact reference or the date. If you could have some way of tracking that down, it would answer my original query and might also be contributary to your present study. I definitely recall that it began with a diatribe against the views of the Mendel-Morganists, among which I was now to be numbered; however, it then included a fairly factual account of my work which left one with the impression that the preamble was to dome extent merely ritualistic.

Some of the other sources that have influenced my own thinking are the contributions of Kossikov and Ierusaleminskii at the Ciba Symposium in 1957 where their contributions can be found in English. This is the one that Imshenetsky reported upon. You can get the original contributions in theiiibbeFoundation Symposium "Drug Resistance in Microorganisms", Churchill, London, 1957. Of course, it was just here that Hinshelwood and

Dean strongly supported the same side of the debate as did the Russian visitors.

You ask a specific question about Hinshelwood and Chargaff. I think perhaps the greatest gaffe that you could make would be to lump Chargaff with the others. Chargaff is known as a rather testy character but who has made enormous contributions to the chemical study of DNA. When he refers to molecular biology, he is not referring to the discipline but rather to a particular school, the phage school, that Mullins has memorialized. It would be a mistake to confuse the two either in the sense that Chargaff has done for polemical reasons or that Mullins did to some extent from his particular vantage point. Stent has made a number of statements about the perception of "biologists" which can only be understood from his own position within the phage group which was not necessarily typical of other members of the discipline. Chargaff was in no way in opposition to the basic doctrines of the biological importance of DNA but rather was opposed to the style of the investigations of Crick and Watson which he felt to be lacking in rigor. The fact is, too, that he was scooped as a result of that difference in style and has obviously resented it ever since.

There was indeed a concerted resistance, if one should call it that, to the integration of bacteriology to other biological disciplines which is precisely the main theme of my current work in this area, with Harriett Zuckerman and Bob Merton. However, it had more the quality of indifference and ignorance rather than active hostility and suppression which characterized the Soviet scene for so long. And it did not last long in the face of new evidence and interest stemming from the early 40's as you have already documented.

The synthetic view that I would encourage would take account of the way in which political Lysenkoism quenched deviant thinking and deviant expression in biology generally and especially where it touched upon genetical-ideological questions. If you reread "the situation in biological science", the 1948 proceedings, you will find that even Alikhanian was obliged to make his kowtow to the new doctrine, and there certainly was a period of at least ten years during which no counter-expression was really possible. It is true that this coincided with the prejudices of traditional, older microbiologists who had very little contact with the doctrines of genetics in any case. So, their hold upon the system and their commitment to traditional ideas was greatly reinforced by the politically motivated suppression of deviant thinking about genetical problems. The same old line establishment might be expected to be much the same people who would fairly acquiescently go along with the doctrines handed down to them, especially if they basically did not understand the issues very well. Under the circumstances, biology was a very unsafe subject to go into and this is indeed one of the most pathetic comments I have heard from some of my Soviet friends, so that bright students would simply elect to go into other areas of science rather than expose themselves to the risks of involvement in this particular controversy. This may help to account for the delay in the further development of molecular biology even after the formal imposition of Lysenkoist doctrine had been lifted.

I wonder if you had an opportunity to consult with Alikhanian or Medvedev or some of the other principals about the issues that you raise in your manuscript.

Besides the other sources that I mentioned before, you might wish to see the monograph "Microbial Variation" edited by V.D. Timakov which in fact appeared in English translation in 1959 under the imprint of Pergamon Press. I think this is rather widely available but I enclose some of the preface to show you the tone of what seemed to me quite familiar language from my Soviet colleagues. You might also be able to find some evidence of the debate between Timakov and myself at the International Congress of Microbiology in Stockholm in 1958, but I do not have the proceedings volume in hand.

If you have English versions of the report by Jerusaleminskii mentioned on page 89 footnote and of Ryzhkov's critique, footnote 31 page 92, I would be most grateful to have a chance to peruse them.

To turn to one of your questions again, about resistance in the west, I think the plain story is that this did not exist to any significant degree at a cognitive level. Even a Dobzhansky quickly accommodated to the credibility and importance of studies on the physicochemical structure of DNA. They might and to some extent justifiably decry the extent to which these reductionists seem to leave little room for other avenues of biological investigation and one has to say that some of my colleagues were rather extreme from that standpoint. Perhaps one has to keep in mind that this entire period was one of very rapid growth of the whole biological establishment, so that even if there were some tendencies which may have existed up to say 1955 to give preference to some of the more traditional lines for new job appointments and so forth, there was not such a stringent competition that this was a serious impediment to further research. In retrospect one can, of course, say that the claims of the nascent field of molecular biology should have been recognized even earlier and the discipline institutionalized in faculty departments and so on and not merely recognized as a promising youngster. My own work faced very little resistance; I would have to say I was more surprised by the apathy that greeted it in some quarters, but within a few years it was even being quoted in the traditional textbooks as they went through their revised editions and within a few years after that even quoted correctly. So, I can really see no evidence of organized resistance on that score. Commener's remarks would have achieved no publicity at all if he were not so totally isolated in his maverick and totally unsupportable views.

I will be very happy to return the chapter to you by separate mail fairly shortly. Thank you very much for the privilege.

Sincerely yours,

Joshua Lederberg
Professor of Genetics

JL/rr

P.S. Have you thought of France as a better contrast/analogy to USSR where the old guard has delayed the advance of molecular biology in the universities (contra the Nobel=prizewinning group at L'Institut Pasteur. Note NY Times ca. Nov.-Dec. 1965 in re. Jacob, Monod, Lwoff on this point).

8/56
cf Sapp on F-resistance to cytoplasm