

November 20, 1958

Sir Macfarlane Burnet
The Walter & Eliza Hall Institute of Medical Research
Royal Melbourne Hospital Post Office
Melbourne, Australia

Dear Mac:

Thank you for your cables and for your very nice note of November 13. Undoubtedly the best feature of the Nobel affair has been the pleasure it has given my friends.

I particularly appreciate the prompt attention you gave to my suggestion concerning Lois Markin when you must have been pre-occupied with preparations for your travel. I can certainly appreciate your reliance on her for the continuity of your laboratory work and can only hope that the excellent help she will be able to give to Gus can in some way compensate. My own plans concerning Lois are predicated on a visit of just one year's duration and she will presumably be under some explicit limitation in the terms of her visa. Of course, it would only be realistic to recognize that she is of an age where she must be quite vulnerable to more domestic attractions, no less in Australia than she would be in the United States.

I hope my remarks about the Eastern Division of the Hall Institute were not so jocular as to conceal my growing appreciation of the full impact of my brief visit last year. I also hope you will have some occasion to accept the invitation I indicated.

I understand that there is some possibility of seeing you at some of the meetings in Europe next spring and look forward to the occasion if it materializes. We have explicit plans in any case to be away in Italy, in Cavalli's laboratory, from the beginning of April till the end of June.

I had a delightful visit with Talmage yesterday, who was kind enough to drive up from Chicago primarily for a casual conversation. As I see his theoretical development, he is simply being somewhat less foolhardy by covering the diversification process under the general heading of differentiation without suggesting an explicit genetic mechanism for it. Partly to complement his own synthesis I am writing up a somewhat more intelligible version of my own conception of the problem for a review paper perhaps in Science, perhaps to be printed in company with his. My only fear in giving adequate credit to your own formulation is that you may be less willing than I am to recognize the paternity of the offspring. I rather suspect that most of the differences that have crept in are going to be terminological or else will concern issues that have to be postulated on a reasonably arbitrary basis at the present time. I am looking forward very eagerly to the publication of your Flexner lectures for a more complete exposition of your ideas on clonal selection. Meanwhile, I am relying on the assumption that you would place the main burden of randomization of cellular potentialities on development in prenatal life whereas my own feeling is that this process can and probably does continue throughout the life of the animal.

I will, of course, take pains to send you copies of any relevant manuscripts.

It doesn't look as if there will be much opportunity for any more lab work on my part until we get settled at Stanford. As I have said many times before, I'm looking forward very eagerly to Gus's joining us.

As ever,