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Dr. J. Lederberg,
University of Wisconsin.

Dear Josh,

I was very glad to see the distillate of our talk in Stockholm. My chief reaction was that it will at least make biologists look at antibody production as something that has to be fitted into a rather important niche in the general structure.

I feel that I shall have to hold firmly on to a very considerable persistence of preadaptation to account for two things -

- (1) The necessity of using perinatal organisms to obtain tolerance - lethally irradiated mice show a distinctly different sort of tolerance to the cells that save them,
- (2) Medawar's experiment by which the tolerance of mouse A induced in utero to skin B is broken down by the implantation of unconditioned A lymph node cells.

The main difficulty is still the very rapid production of primary antibody by rats or rabbits given typical Gm-bacterial antigens. It is probably crucial that these are just the antigens that nobody can obtain tolerance with. This makes it very difficult to devise any experiments to sort out the impasse.

I have a prejudice against the concept of a universal variability of part of the genome which can be stabilized by contact with an antigenic determinant that it meets at the right moment. As far as I can see it is very little more than a restatement of Pauling's theory and, except for making immunological memory easier to interpret, it is no more attractive.

I am amused to find how enthusiastic my colleagues here are in their efforts to find ways by which the clonal selection idea can be proved wrong! Undoubtedly they will eventually succeed but in the process they are going to have a lot of fun and I hope when the story comes clear I shall still be regarded as one of its distant progenitors.

I shall keep on steadily exploring ways to allow Gus to have his year or two with you at Stanford and feel confident we shall manage. I hope you also will drop a word in any promising quarter.

Kindest regards to you both,

Yours sincerely,

F. M. Burnet

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