33 June 1943

Dear

Many thanks for your letter of June 21st. I will try to do what you ask in respect of the points arising from Dr. Korenchevsky’s proposed research at Tooting Bec.

(a) On comparatively anecdotal evidence I do at the moment believe that the vitamin requirements of ageing persons are very well worth studying and likely to lead to improvements in public policy and in the general health of the population. Had I the choice, I would rather be able to give results with the arrest of mild or premonitory symptoms of senility than with severe or advanced symptoms. I imagine, however, that what we used to call a "workhouse-infirmary population" is the only one likely to be available for experiment. What degree of ability or disability can be found in selected groups at Tooting Bec I have, I'm afraid, not the faintest idea, though the sorting of the material available according to its capacity to respond to physical and mental tests seems the necessary basis of a good experiment.

(b) This remark has some bearing on the question of the number of cases: "No experimenter objects to the material measured giving variable measurements; it is just because they are variable that they are worth measuring. What he cannot bear is that some of
the material should be incapable of giving a valid measurement at all in some respect or other. I think a reasonable experimenter would be satisfied if he could get complete results from even 200 persons in all, that is to say that 200 subjects should survive the treatment and be available and competent to respond to all the tests applied. Such material capable of supplying information of value might, however, well have to be sorted out from a much larger human mass. Unless a sufficiency of really suitable material is already in view, it could I suppose always be improved if a larger sample were available for preliminary research.

I put two hundred as a reasonable minimum because I think that any responses of immediate practical importance to our ageing national population should be demonstrable using comparisons between groups of about 50. There is a law, I believe never yet formulated, that the effect that the cost of experimentation in any one field is inversely proportional to the square of the public value of the results obtained. I do not mean that that two hundred persons would be sufficient to detect the least response of sufficient public importance to justify the expenditure, but that if they do not supply coherent immediate guidance for the general social and medical treatment of many persons, they should be sufficient to point the way to better-directed experimentation within this field.

(c) I find myself a good deal dissatisfied with the form of experimentation proposed and discussed so far. Whatever the number of persons available, this number can be more or less efficiently subdivided into experimental groups, and such subdivisions do not appreciably increase the time, labour, or expense required for the
experiment. I cannot see why less than four groups should be used from the start, namely With and Without Vitamins, With and Without Hormones. I do not yet understand, though I am open to conviction, what advantage there is to be gained by separating two periods of experimentation by a rest-period which must increase the number of losses by death or dispersion. I should expect the second period of treatment to be most informative if all the initial groups were again subdivided into four each in respect of their treatment during the second period. Even though final subdivisions of only 10 or 12 persons would result, the combined comparisons would lose no precision by such subdivision.

Finally, I am aware that Dr. Koenchevsky, and perhaps others, find considerable difficulties in some of the suggestions I have made, and I should not like my own views, formed as they are without particular knowledge of the material, in any ease to prevent other more practicable plans being out.

Yours sincerely,