Professor R.A. Fisher,
Galton Laboratory,
University College,

Dear Professor Fisher:

I was very glad to learn that you were going up to the Galton Laboratory. I understand that Egon Pearson stays on also. The two of you should make a fine team. We ought from now on to have from the Galton Laboratory not only good contributions in mathematical statistics but more critical contributions in the way of applications of statistics to social and biological phenomena.

I sent you recently a paper of about 100 pages on the logistic theory of population. I hope this cleans the subject up. The stuff that has been printed on it in this country is a disgracefully careless contribution to applied mathematics. Sometime I am going to look up the matter of who was editor of the Royal Statistical Society when they printed that particularly incorrect paper by Reed and Pearl in 1927. There aren't many journals in England and America which will print an obviously incorrect attack by notoriously inaccurate people upon a paper written by persons who have a reasonable reputation for accuracy without at least referring the polemic parts of the paper to the persons attacked to see what their reaction is. Moreover, when I sent to the editor a little three page contribution pointing out that the work of Reed and Pearl was in fact incorrect and that their criticisms on my work and Luyten's were not valid, the editor returned the little manuscript without any comment whatsoever although I had sent it to him with a statement that I thought something was due me because I had as a matter of fact sent my manuscript to Pearl who had kept it for a good number of weeks before I ever sent it to press. I thought this might impress him with the fact that I was trying to avoid putting into the literature of the logistic theory articles which Pearl had not had a chance to see.

I am afraid it comes back to the comment Pearl made to me in 1924 when Yule had read a paper at Toronto on Pearl's theory and Bowley had made some general but entirely sound comments on the situation which I had relayed to Pearl. The reply from him was that Bowley was a stupid man and that if he ventured to say anything against Pearl's logistic theory Pearl's gang in England would fix Bowley. I had known, as you doubtless have, that there is in certain groups of statisticians a gang spirit but I had
it put so bluntly before. Moreover, although one may have to appeal to the gang spirit in dealing with a hostile gang it seems to me quite unnecessary to appeal to it in dealing with so gentle a soul as Bowley. However, I believe that the American gang has been pretty well beaten up in the last 4 or 5 years and that we shan't have so much trouble with them. Patterson's frontal attack on Pearl's contribution to the Eugenics Congress at Berlin showed real courage for a young fellow and I have never known that any answer to that attack that was even feeble has appeared. The paper was printed in the Am. Naturalist by J.M. Cattell who knows and likes Pearl personally but who was just as convinced as could be that Pearl's contribution was unsound and Patterson's sound. The contributions of Carlson and Bell and of Fortune in the way of gutting the ground from under Pearl in his long paper on Cancer and Tuberculosis pretty well cleaned that paper up and possibly I put the finishing touches on the matter with my theoretical treatment of this whole sort of question which I printed in the Proceedings of the National Academy of Sciences and with my statistical discussion of the matter in the American Journal of Cancer. Although Bowley urged me when I saw him in Toronto in 1924 to clean up the logistic theory of population I was very loth to do it. From the early days of Pearl's contributions I had always put my own notions of the theory before him and I was hopeful that he would adapt his writings to what was mathematically true. Moreover, what little things I had written on the subject had been in his hands for criticism before they were printed and had not only been written with the aim of avoiding direct conflict with him and in the hope that he would incorporate the ideas into his own work but had actually been revised to remove any point of conflict to which he seemed to be sensitive. It was therefore a little surprising to me to find him printing in the Journal of the Royal Statistical Society an article which he had not shown to me and it was this situation which finally led me unwillingly to feel that perhaps I had better undertake to do what Bowley had earlier suggested that I do.

Yours very sincerely,

[Signature]