He said—

I'm just returning from a quick trip to St. Louis where I had my first good chance to talk at some length with W. Cohn. He has a well designed and well executed system for studying anti- body and I have to say at the outset that he is certainly finding double yielder. Don't think the discrepancy is due so much to the presence of his test (which I, to be sure) which is laborious as to the character of his antigen and technique of comminution.

There is some circumstantial evidence that double yielder arise mainly under maximum intensity stimulation cues, as to speak to tue young the responding system. Furthermore he favours strongly first with my antigen and rely then with the second. On the whole this might get more easily with a hypermotility hypothesis than a pre-fixed clones idea, but of course the experiment is not decisive in itself. With any, the results of my trials: anti T-1, anti T-5: double yielder were about 250:15:7:3 which I suspect is statistically significantly different from your summary, though not.

He and Heard recently are in concordance about your mo. He has a number of criticisms, many of which are reasonable, to some degree, and some of which have voiced myself, but none seem to me crucial. Hope you will get the criticism and will reply in due course in the same constructive spirit in which this question. We are both looking forward to your coming to Stanford. He is independently thinking of writing you to come as his guest, and that, unless we have a happier rearrangement.
time-natural, rectifying the some of the
induced disequilibriums, while can only lead to a
deepen menyart. He has similar aspirations as local
analysis and, although our approaches differ enough this
should be mutually helpful. Avier

John