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February 8, 1980

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Raritan, N.J. 08869

Dear Phil:

Your letter with enclosed reprints and MS (Historical Perspectives...) just received and greatly appreciated. The same, of course, applies to your earlier phone call. And all this after so many years.

The week following your phone call Elvin Kabat was our guest. In the course of driving him around on the campus we were discussing the good old days (e.g., 1938-1948) and I just happened to say, "Guess who phoned me last week?" and he replied, without any hesitation whatsoever, "Phil Levine". I asked "How the Hell did you know that?" He said "That's easy, Phil's been phoning everyone!"

And then I told Elvin about the time we first met in Madison when you came back to talk to Irwin's group (Ferguson, Irwin, Black, Cumley, myself and a few "outsiders") about your ideas on the etiology of HDN. When I heard your case histories, and you had only a few at that time (1939), I was convinced that you were on the right track and didn't hesitate to give you my opinion. That was the kind of assurance you needed and it wasn't long after that that the data began to appear in the literature.

Reminiscing is an interesting pass time, and I am certain the stories get better and better as the years roll by. But there is one I always like to tell and it needs no magnification whatsoever. One of my former students asked me when discussing the merits of the linkage theory (CDE) and the theory of multiple alleles (Rh-Hr) if I know R.A. Fisher. I said "Yes, of course, I even danced with him". The story is true and the scene was at the final banquet during the 9th International Congress of Genetics held in Bellagio, Italy, August 1953. Fisher (Sir Ronald) and I, who had been engaged in a discussion with J.B.S. Haldane about chimeric cattle twins, entered the ballroom late to find that the ladies by then were all engaged with dancing partners. R.A. looked at me in dismay and said "Stormont, shall we dance?" I said "Why not?" Of course, we both found better partners right after that dance and that was the last time I ever saw Sir Ronald.

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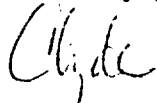
Unfortunately, I never did meet Karl Landsteiner but I can always say that I did know some of his most outstanding coworkers (often referred to as "students"), Phil Levine and Al Wiener. You know, Phil, I still have on display the two books which I refer to as bibles, namely, the 1936 and 1945 editions of Landsteiner's Specificity of Serological Reactions, and you will see (p.3 of the enclosed reprint on The Early History of Cattle Blood Groups) that I referred to the bible once again. I highlighted that reference for your benefit and also one other on the same page. Phil, in your Historical Perspectives Before 1945 - Updated you wrote, "In my 56 years devoted to active research in the laboratory, I invariably discovered surprisingly 'new' gems in reviewing the literature of the past".

(1972) // And, Phil, now that the serology of the MNSsUMi<sup>a</sup> system can all be explained on the basis of substitutions involving ~~and~~ sequence of 8 amino acids (re your Perspectives), do you still subscribe to the Fisher-Race-Sanger theory of closely linked genes to explain that system? You know, people like myself and Al Wiener took an awful lot of punishment over the years for proposing and adhering to the theory of multiple alleles to explain such systems. Indeed, I have never had any problems whatsoever in explaining any of the numerous complex blood group systems, which I and my students have discovered in cattle, sheep, horses, etc., on the basis of multiple alleles.

I'm off on Sunday for two weeks in Brazil. Will contact you when I return in regards to immunizing some horses with antigens of the MNSsU system. In the meantime, the word is go. So get the antigen preps ready to mail to me.

Best personal regards.

Sincerely yours,



Clyde Stormont  
Professor of Immunogenetics

CS:lc  
Enclosures