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THE UNIVERSITY OF WISCONSIN
Laboratory of Genetics
Genetics Building
MADISON, WISCONSIN 53706
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Department of Genetics
College of Agricultural and Life Sciences

Department of Medical Genetics
School of Medicine

Professor J. Lederberg
Department of Genetics
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Stanford, CA 94305

Dear Josh:

Thanks for your kind letter of September 10.

Your special interest is noted in documenting the attitudes of biologists at the time of your appointment here toward the striking new genetic findings that were bringing you to the fore. A few excerpts from the letter of recommendation, a copy of which I could not send you because of reservations on the author's part are significant in this relation. You as a person and your scientific work are confounded with each other in the replies he obtained. I shall quote, of course, without identifying sources.

The writer of the letter referred to took the trouble to ascertain the views of a few fellow biologists with whom he was able to talk personally concerning you and your pioneer work.

One of the persons interviewed was somewhat distrustful of you "on the suspicion that his ambition and enthusiasm lead him to advocate the most spectacular conclusions from experimental data that would only set more stable workers on a long series of cautious checks."

Another person took a quite different attitude, stating that "Josh recognizes his deficiencies in technique, is the first to distrust results of his own that are out of line and repeats them until he is satisfied that they are repeatable and welcomes criticism and repetition of his work by others he trusts."

Still another associate is quoted as referring to you as "one of the most brilliant minds in biology today, and he trusts his work implicitly."

The writer of the letter also stated that "although two other investigators have failed to confirm the 'sex in bacteria' work...

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it now seems clear to me that to regard his work as untrustworthy is unjust."

As mentioned earlier, certain of my Wisconsin colleagues, previously opposed, switched position and voted to invite you to Wisconsin on the basis of the letter being withheld.

L. J. Cole was retired and in ill health at the time you were under consideration for appointment here, and took no part in the discussions. Cole and W. H. Wright (Agric., Bact. Dept.) in 1916 published an article entitled "Application of the pure-line concept to bacteria" in *J. Infect. Diseases* 19: 209-22. I do not recall Cole expressing any particular interest in the genetics of bacteria later on, that is, during my association with him (after 1922).

A brief comment will be made concerning the reference in the last paragraph of your September 10 letter to improving the nutritional value of cereal proteins. This is now one of the most active areas in plant breeding here and abroad. After various false starts by others over the years, my present colleague, Oliver Nelson (then at Purdue) and associates, about 10 years ago, opened up the field in an exceedingly promising way with the discovery that the opaque-2 gene greatly increased the content of lysine and tryptophan in corn endosperm. (Corn has the lowest quality protein among the major cereals.) Mutants with somewhat similar effects have since been found by others in barley and sorghum. Intensive work to the same end is also in progress with rice, particularly under the sponsorship of the Rockefeller and Ford Foundations in the Phillipines.

The opaque-2 gene (and another maize mutant, opaque-7, with a comparable effect on protein quality) reduces yield of grain, ten percent or so, delays seed maturity, reduces germinability and also significantly increases susceptibility to certain fungus and insect pests. The extent to which the several effects of opaque-7 vary independently in response to different mutations at the locus is a question in which I have recently become interested. Am currently attempting to generate a spectrum of o7 variants by "transposition mutagenesis", using a scheme patterned on McClintock's Ac-Ds system. This particular approach is novel, so far as I know, and it is too early to say whether it will be effective.

We are much interested, of course, in Peter Carlson's work on the hybridization of vegetative cells in flowering plants.

He was definitely interested in the position on our staff that we recently offered him following a visit to Madison. He has decided, however, to remain at Brookhaven for the time being so that Mrs. Carlson can continue graduate work in psychology in New York City of a sort said to be unavailable elsewhere. Our offer probably will be renewed later, and we hope Carlson's seeming interest in Wisconsin will stay alive.

Sincerely yours,



R. A. Brink
Emeritus Professor of Genetics