Dear Barbara:

I imagine you’ve heard, via the grapevine, or otherwise, that since my retirement from administration three years ago, I’ve gone back to work on the origin of corn. I never believed the evidence was any good for Paul Mungelsdorf’s tripod theory that teosinte is a recent (or remote) product of hybridization or that there was ever a wild corn of the kind he postulates from archeological evidence.

I believed years ago that teosinte was a logical wild ancestor and still do.

For three years now I’ve been accumulating evidence, mostly from F₂ and BC hybrids of various teosinte and corns. The recovery of good corns and good teosinte says we have more than 4 or 5 independently segregating units. I agree that there are in some instances clusters of linked genes, but that is to be expected from the repeated transfer of genes from teosinte to corn over the last 7000+ years in the areas where they coexist.

Last November (8 of us from 7 universities were down in the teosinte country of Mexico. When we spent 120 man (one woman) days looking at and
collecting seeds from 75,000 plants. Randolph was along—remarkably spry for his age—and so were anthropologists, ethnohistorians, taxonomists, etc. Kato was a very effective person in charge of one group of Q. I think we all learned a good deal.

Knowing the cohabitation characteristics of corn and teosinte in the areas where they grow together as extensively documented by Darlwin Wilkes, who was along as leader of the second group, it seems to me that teosinte that grow in association with local corn types, and have for long periods, are bound to be alike in many genes of the kind that do not differentiate them. Many keep selecting out the good corn types that acquire hybrid vigor from the successive backcrosses of hybrids to corn and natural selection does the counterpart for successive backcrosses to teosinte. Thus to identify the essential genetic and cytological differences between the two, these are the logical populations to compare—those that coexist I have for long. Wilkes documents many vegetative characteristics,
that do this shuffling back and forth but of course these are not the ones essential for survival for the two extremes.

If I am correct, there is some published evidence that this shuffling helps to resist for blight. I know you have much evidence on this but I'm told lots that is not published. As a result of a good deal of recent work of Walt Bckin's (we've been keeping in close touch), Paul is changing his views fairly rapidly. I believe he has given up the corn x flint corn origin of teosinte but not his wild corn. I believe the similar data are very significant, so I hope you are planning to summarize and publish before too long. In the meantime I'd be grateful to know how many comparisons of pairs of corn x teosintes, known to have the same areas for long times — i.e. non-local lines of corn grown for long periods in an area where teosinte is uncommon.

If this is not available I think it would be very important to get it. You of course are the ideal person to do it but I realize...
You may not have the time and energy to do more than you are already doing. I've been in close contact with Kato and he might be persuaded to do more. I knew him as a super fine man, but I have no idea how good he is as a cytologist. If these comparisons I suggest have not been made rather systematically - i.e., the data not collected, and you cannot be persuaded to do them, do you think Kato could? If so, it would be good for both science and Kato. I rather get the impression he has not made a good niche for himself at CIMMYT and I'd like to see him do it.

In the genetic work in which the grown populations of ca. 40,000 plants in Mexico, Mario Gutiérrez of CIMMYT has been a super collaborator. Whenever he is in charge, I know things will be right.

Mario and I are now in Davis, Calif. where we are doing a 2-week stand as co-lecturers on such subjects as Urban Renewal (both), Pre-school Years (M).
Harpers Court (a not-for-profit artisans center in Chicago in which Samuel has played a key role),
History of Broodhen-Bettys (2) and The
Origin of Corn (2). One week is gone and
we found it very stimulating. We saw a
lot of Dobie, Hedward Sheldon, Charlie
Rick and some of all and who's head
of the Dept. We'll be back in Chicago
shortly.

I have an part time appointment
at the U of Chi but have given up a 1/4-1/2
time for as Pres of the Chi Hort Soc which
I took pending a permanent full time one.
Now we have Louis Montin from Brooklyn
Botanical coming so he have pretty much
full time for corn work.

I hope all goes well with you. I
know whatever you are doing, you'll
be doing it with the devotion energy
and effectiveness you've always worked.
Now that I'm on corn again, I think often
more often for I always did about these
good old days at Cornell. Regards Bests