Dear Barbara:

Thanks for your good letter — so full of exciting ideas!

Shortly after writing you I had a long letter from Wellhausen telling of your conference and your plans to publish with Kato and Blumenstein. That's great and I thoroughly agree that all of you should stick to getting the material organized in form for publication.

I have made a table comparing the longley-Kato 20 knob positions with Ting's six Mexican teosintes. This does not include size differences, just positions. See last one of the 20 corn positions and found in the teosintes, but there seem to be 45 in the teosintes not found in the L-K corns. (Incidentally Schmick Ting's get his chromosome 5...
and for and a time or two) There unpublished some new data from Sikkim corns by Jain and coworkers at New Delhi. (Murray and I were in Pakistan—India Ann Ford found a couple of years ago) Three of the four kernels in Mex teosinte not found in Amer corns are present in Sikkim primitive-type corn. That leaves only one of the 14-16 kernels of corn not found in the 6 teosinte (limited populations and I'm not sure how good they were with his material) plus only one in the Mex teosinte not found in the LHK corns.

Because of the sterility of Guatemala teosinte with corn (Emerson and I noticed this) and with Mexican teosinte, I'm much inclined to think that Guatemala Honduras teosinte are an offshoot line that had better or nothing to do with the origin of corn. Your indicated center of origin in the Rio Balsas where the...
largest populations (really wild too)
of Mexican teosinte supports this. Thus if Guatemala and
Mexican teosinte were separated long before corn evolved, then
corn later was taken to Guatemala where
because of sterility, introgression from teosinte would be less likely—
both for sterility and probable shorter time of association—one would
expect just what you find—(i.e., poor correspondence of chromosomes
in kernels etc. between Guatemala corn &
"teosinte.

Getting back to the Sikkim corn, this is the stuff Edgar Anderson believed was pre-Columbian corn in Asia,
which Paul be pooch-pooched (in this case I think justly) in any
case it has undoubtedly been
separated from American corn since
good after Columbus. It probably came
from South America—Argentina-Brazil?—
Paul's paper would indicate this. I
think the Jain et al work should be
checked. I brought some seed which George Sprague was going to grow under guarantee conditions at Beltonville. I didn’t follow up and it’s now lost. But I’ve written him for more seed which Sprague will grow in the US and, if free of disease, send me. I hope you’ll be willing to have a look.

Do you want to see the Jairat reports? I’ve mimeographed them and I can send it if you like. I saw the plants in New Delhi and they are indeed rather strange.

I agree with you about Walter Galina. He has lots of energy and enthusiasm and is a very hard worker. I’ve wondered about the cytology but am not such a good judge on that. He has too many faults in the nice and is not too critical.

Have you seen his Bon Rev? I spent a great deal of time with him on it. His logic is not always good and he can’t write for a damn.
Wilkes and I went over to his a couple of times and made large numbers of changes. Without our encouragement, he would not have dared challenge Paul, but now I think he and Garrison both see the light. Garrison is good — very smart, very good on detail. But he has so many interests I'm afraid he won't ever do much research.

Good old Fritz Randolph was on our collecting trip. He now has a long list depending on generic separation of teosinte which Walk and I have tried getting in not to publish — without much effect, I'm afraid.

I have beautiful reconstructions of early archaeological corn — from corn teosinte hybrids — obviously much closer to teosinte than is modern corn.

Paul has now been pushed back by the fossil pollen evidence which I'm convinced is no good. —over—
A year and a half or so ago
Fitz, Walt and I went over the
archaeological material at Harvard
with Paul. It has more distinctive
characters than reported — ie 2-ranked
coils and some single spindles. While
we were there Panajee, a grad student
with Banghoorn showed up scanning
electron micrographs of Tripsacum,
texture and corn pollen exine
patterns. Corn and teosinte are
quite indistinguishable but
Tripsacum is very different.
Panajee said he didn't want to
publish until he got his degree!!
Paul has a powerful hold on
Banghoorn it seems. I don't
believe one word of the "fossil" corn
jokes* and Walt doesn't either.
It's something, isn't it?
Keep pushing on the publication.

* For this reason too.

Regards

Beets