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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
Blumenschein. 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 ①
the Longley-Kato 20 knob positions
with Ting's six Mexican teosintes.
This does not include size differences,
just positions. See but one of
the 20 corn positions are found in
the teosintes but there seem to
be 4 in the teosintes not found
in the L-K corns. (Incidentally
I think Ting got his chromosome 5

P.S. Kato helped me harvest corn at - two-site hybrids
at Col. Patán and he was the leader of our 1/2 of the trip collecting
trip - 9 people a week in the field. Jim are for him and I
hope he will be able to get that degree.

(2)

end-for end a time or two) I have some ^{unpublished} knob data from Sikkim corns by Jain and coworkers at New Delhi. (Muriel and I were in Pakistan-India for Ford found a couple of years ago) Three of the four knobs in Mex teosintes not found in ~~the~~ ^{L-K} Amer corns are present in Sikkim primitive-type corn.* That leaves only one of the ~~the~~ K-L ~~the~~ knobs of corn not found in the 6 teosintes (limited populations and I'm not sure how good Ting was with his material) plus ~~one~~ only one in the Mex teosintes not found in the L-K corns.

(3)

Because of the sterility of Guatemala teosintes with corn (Emerson and I noted this) and with Mexican teosintes, I'm much inclined to think that ~~the~~ Guatemala-Honduras teosintes are an offshoot line that had little or nothing to do with the origin of corn. Your indicated center of origin in the Rio Balsas where the

* ~~Stoner & Edgar Anderson~~ Stoner & Edgar Anderson. Ann Mo Bot Gard 36: 355-396 1949

largest populations (really wild too) of Mexican teosintes supports this. Thus if Guatemala and Mexican teosintes were separated long before corn evolved, then later ^{corn} 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 ~~is~~ i.e., poor correspondence of chromosomes in knobs etc. between Guatemala corn & "teosinte".

Getting back to the Sikkim ^{stone} corn, this is the stuff ^{Edgar Anderson} believed was pre-Columbian corn in Asia, which Paul he pooch-pooched. (in this case I think justly.) ~~Identical~~ In any case it has undoubtedly been separated from American corn since soon after Columbus. It probably came from South America — Argentine-Brazil? — Paul's paper would indicate this. I think the Jain et al work should be

checked. I brought some seed heads which George Sprague was going to grow under guaranteed conditions at Beltonville. I didn't follow up and it's now lost. But I've written Jain for more seed which Sprague will grow in the GH and, if free of disease, send me.

I hope you'll be willing to have a look.

(7) Do you want to see the Jain et al ^{reports?} ~~stuff~~? It's in mimeographed form and I can send it if you like. I saw the plants in New Delhi and # they are indeed rather strange.

(8) I agree with you about Walt Malimat. 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 and that. He has too many irons in the fire and is not too critical. Have you seen his Ann Rev Genetics piece on the origin? I spent a great deal of time with him on it. His logic is not always good ~~to~~ and he can't write for a darn. Garrison

(9)

Wilkes and I went over the MS 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 Fitz Randolph was on our collecting trip. He now has a long MS depending the generic separation of teosinte which Walsh and I have tried getting him not to publish - without much effect, I'm afraid.

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

Paul has now been pushed back to the fossil pollen evidence which I'm convinced is no good.

6.

A year and a half or so ago Fitz, Walt and I went over the archaeological material at Harvard with Paul. It has more tectate characters than reported — i.e. 2-ranked cobs and some single spikellets. While we were there Banerjee, a grad student with Barghoorn showed up scanning electron micrographs of *Tripsacum*, tectate and corn pollen exine patterns. Corn and tectate are quite indistinguishable but

Tripsacum is very different.

Banerjee said he didn't want to publish until he got his degree!! Paul has a powerful hold on Barghoorn it seems. I don't believe one word of the "fossil" corn pollen* and Walt doesn't either.

It's something, isn't it?

Keep pushing on the publication.

Regards

Beets

* For other reasons too.