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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 the Longley-Kato 20 knob positions with Ting's six Mexican teosintes. This does not include size differences, just positions. All 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 (1)

P.S. Kato helped me harvest corn - teosinte hybrids at El Batán and he was the leader of our 1/2 of the big collecting trip — 9 people a week in the field. I'm all for him and I hope he will be able to get that degree.

end - for end a time or two) I have
some unpublished data from Sikkim corns
(2) 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 L-K corns are
present in Sikkim primitive-type
corn.* That leaves only one of the
K-L knobs of corn not found in the
6 teosintes (limited populations and
I'm not sure how good Jing was with
his material) plus one only one in
the Mex teosintes not found in the
L-K corns.

Because of the sterility of Guatemala
(3) 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. Ann Mo Bot Gard 36: 355-396 1949

largest populations (really wild two) 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 ~~—~~ ie., poor correspondence of chromosomes in knobs etc. between Guatemala corn & " teosinte.

(4)

Getting back to the Sikkim corn, this is the stuff Stonor & Edgar Anderson believed was pre-Columbian corn in Asia, which Paul has pooh-pooh'd (in this case I think justly) ~~I don't know~~ 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

(5)

(6)

checked. I brought some seed back which George Sprague was going to grow under guaranteed conditions at Beltsville. I didn't follow up and it's now lost. But Doc written Jain for more seed which Sprague will grow in the 4th 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? 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 Galinat. He has lots of energy and enthusiasm and is a very hard worker. Doc 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 Bot Rev Evolution piece on the origin? I spent a great deal of time with him on it. (9) His logic is not always good & and he can't write for a damn. Garrison

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 (12) 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 MS defending the (11) 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 (12) corn teosinte hybrids — obviously much closer to teosinte than is modern corn.

Paul has now been pushed back to the fossil pollen evidence (13) which I'm convinced is no good.

— over —

A year and a half or so ago Fritz, Walt and I went over the archeological material at Harvard with Paul. It has more teosinte characters than reported — i.e. 2-ranked cobs and some single spikelets. While we were there Banerjee, a grad student with Barghoorn showed up scanning electron micrographs of Tripsacum, teosinte and corn pollen exine patterns. Corn and teosinte are quite indistinguishable but Tripsacum is very different. Banerjee said he didn't want to publish until he got this degree!! Paul has a powerful hold on Barghoorn it seems. I don't believe one word of the "fossil" corn pollen* and Walt does not either.

It's something, as it is!

Keep pushing on the publication.

Regards

Beets

* For other reasons too.