UNIVERSITY OF ILLINOIS
DEPARTMENT OF BOTANY
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1/7/52

Dear Joshua:.

I have been trying to find the English copy from which my Paris paper was translated into French but no luck. I recall having given it to some one but can't remember to whom.

Your letter raises some pertinent questions about plastid mutations. As you will see in my 1946 C.S.H. symposium paper I state that it is difficult to differentiate between irreversible plastid mutation and a permanently changed cytoplasmic condition in which normal plastid growth is impossible. This could be done in plants where normal plastid primordia enter with the sperm cytoplasm. If it were known that this occurs regularly the fate of these plastids would give the answer. Unfortunately in maize we are not yet sure that any cytoplasm accompanies the sperm nucleus altho I hope to settle this matter with material now available. The "cytoplasmic conditioning" mentioned in your letter seems to me to favor the idea of a cytoplasmic condition which inhibits normal plastid development but one can also argue that plastid mutation involves several steps and that the plastids in the F2 of ij plants had been somewhat modified by the action of the ij gene in the first cross of ij x green so that they were made more susceptible to a second exposure to this gene. I do not favor this but can't rule it out.

We have carried mutant plastids through three back cross generations. However it should be recognized that in a single white seedling a good many cell generations occur in which plastid

recessive could take place. No doubt it would have been desirable to carry the mutant plastids through more generations (several) but I ran out of time.

If I haven't answered your questions please let me know and I will try again.

Cordially yours

Marcus