

June 11, 1971

Dear Charlie -

I am enclosing the manuscript for the paper. I have written it up & think it ought to go. Sharp, P. Kneller & Staats have worked over it. I made it as short as possible as they could be so much detail which would swamp the meaning for what we want. The paper which goes with it is Harrel's - "A Cytological Correlation of cytological & genetic crossings". I am growing more interested to make the location wider in scope but I believe that this is not necessary for this paper.

The part marked in red across the page is the part which you are to do as you like with. I don't know whether you want to put in a table, make a statement or what. The figures I obtained from a letter you wrote in December. They are more data now. There you will see what ought to be put in here to give up the meaning intended. If you do not like it, anything else let me know. If you do not want to join with me I should feel bad for your data has been a key for me to work with. You may not want to take the responsibility since

You were not in close touch with this latter work when it was done. I hope you won't feel that way though but be frank about it & say what you please. Don't injure yourself to please me.

I am anxious to get Harriet's data in press as I have been advised to push it. Since this paper goes first in the ^{Proceedings of the} National Academy of Sciences I am anxious to get it off as soon as possible. I don't know where best you would be able to do it. I don't know whether you can spare a few hours during the next few days to do it. Next 20. Let me know by a short note what the situation is so that I will know what to expect.

Covering #7. Emerson asked me to be certain to cross #7 with chocolate perhaps. He grew the chocolate perhaps & I supplied the multi plants. Rhoades remarked that he was going to take care of the crosses, etc. He had talked it over with Emerson before I saw Emerson. He probably thought he was doing the easiest thing for all of us. I am sorry if you feel a bit upset about it. I am sure he is doing it as "some thing to get out of the way" about it. I am sure he is doing it as "some thing to get out of the way" about it. Therefore you ought to go ahead with rather than a special problem. Therefore you ought to go ahead with what you have on #7 & chocolate. You will be there before he gets his data. I am sure there will be no hard feelings anywhere.

I am growing the material for you on the 6-th smaller - #5. The ratios, ^{3/4} plants this summer will

Completed. So far I have:

${}^{40}\text{P}_9$ (11) \oplus -

${}^{56}\text{P}_7$: 7 pr : 29 colonies

${}^{40}\text{P}_9$ (13) \times
 ${}^{115}\text{P}_2$: 14 pr : 27 "

${}^{40}\text{P}_9$ (15) \ominus , ${}^{162}\text{P}_7$: 15 pr : 47 "

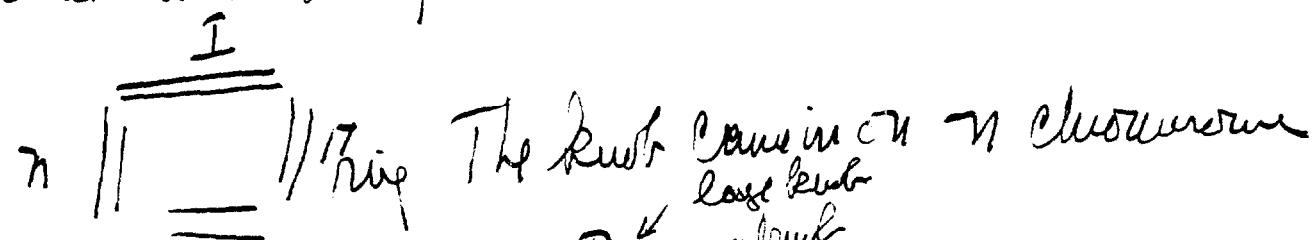
(total) ${}^{36}\text{P}_7$: 3 pr which is OK. for a trisomic \ominus ratio. I shall get 1/2 colonies in the same material. It looks suspicious though, though. I shall be very much surprised if anything goes wrong.

I have no grown Beadle's material for now until I find out about $\# 8$. There is no need of fooling with it when I have $\# 8$. You get $\# 8$ out let us know. I might be needlessly negative. I will get it next spring, or fallow: I am running some close for it & will get it next spring, or fallow:
 ${}^{2n+1} \text{ or } {}^{2n} \times {}^{2n-1}$, ${}^{2n+1} F_1$ will give low sterility-in progeny.
Low sterility will then only ${}^{2n+1} + {}^{2n}$ to ${}^{2n+1}$ probably.

I have been here in Columbia 10 days. It was rather hard getting started as there was almost no equipment. And in

Or all now & have started work. According to my hopes I shall be in Paradise in October. It all depends on how my work goes here when I shall get there.

I believe the knot may be difficult to work in in all cases. There seems to be // a type of dysraphis in some cases because as tho there was no counterpart to the knot (in length, if not in quality). Therefore, severing out is not expected. I have evidence that crossing over occurs before MI - between early pro & anaphase - probably according to Suy. The tendency to form O's at dK is an expression of the force which reads exclusus. My evidence of crossing over is:



When long nephrodes formed I find

in late prophase

I find figures which are clearly crossovers

In these cases the chromatids were clear as were the spindle fibers always. I don't believe we shall see an opening out like this in o-nephrodes when we are heterozygous:

but only

since the knot extends out beyond the line & the knot

chromatins stay together 2 by 2. This seems to be the case in the figures I have viewed.

Have some nice slope & trivalent synapsis in early prophase. I know ~~this~~ now that when a nucleolus part is present it shows double, that is, shows the split. It is now this difficult to tell which is the 2 by 2 & which is the single part. Synapsis is only 2 by 2 for ^{all 3 of} the chromosomes. Then some they consider 2 by 3. Under act. 1 the satellite else. Then some they consider 2 by 2. Not understand. The nucleolus gets together at the region where from the nucleolus the chromatin gets together at the region where the nucleolus is. The chromatin part behaves as the is attached to the nucleolus. The chromatins part behaves as the chromatins do. I am wondering if it is chromosomes or chromatins they trivalent do. I am working on this now. Which synapse 2 by 2. Am working on this now.

People have been walking in & out of my room making such a disturbance I can't keep concentrate. They better will probably be quite uncoordinated. I hope you can make it now, it may

will I see you in the fall? I hope so. This is so well to talk about besides business.

My regards to the "bunch" -

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P. M. S.
Part.