

July 27 1961.

My dear Francis,

I have just sent off another letter to you about the Alice paper and must say here, off the record, that I have been extremely angry about it; however, I have not said anything about it. Lab news:

(i) Suppressors

I have just had a session with Leslie and am not too happy about the mapping of suppressors. The order seems to jump about quite a bit. In order to try and sort this out a little more effectively, I have got her to make double mutants of suppressors of the same generation. The first one that looks as though this is successful is FC1 FC7. This should be like a deletion and help to locate the others. She is also busy testing what I call the "spin" of suppressors that map at the same site. This should be known by the end of next weekend and is an important experiment; to test whether suppressors of opposite spin can nevertheless occupy the same site. Richard is busy with minute suppressors, which should generate another set of mutants. I have been helping him with how to do the experiments. I am very impressed by his initiative and I think he is bright.

## (ii) Mutagenesis

Muriel has isolated 150 A7 mutants. We confirm that there are very few  $r_{III}$  mutants: the main aim is not to throw away the high reverters which may be very hot for suppressors. We are looking for one in the B1-B4 region since this is so well worked out. Also whatever  $r_{III}$  we have found I shall try and get mapped without K selection: they may well be at the ends of cistrons. This should be quite exciting. I think we should also look at Acridine mutants at the other end of the cistron B to see whether there is a similar pattern of suppressors. This will come in good time.

We have a student working here and I have got him to isolate 20+ mutants from each mottled plaque induced from  $r^+$  with EMS to see if they are all the same. He can do this for photodynamic and hydrazine mutagenesis as well. Everything will be steadily pushed on.

## (iii) My work:

At the moment I am concentrating on  $\lambda$ , preparing the  $\lambda$ dg. I have induced clear mutants with acridines in  $\lambda$  which, incidentally is not inhibited by acridine dyes.

(iv) Proteins:

(i) the ribonuclease producing strain arrived from Japan but I haven't done anything with it yet. Am waiting for subtilis strains from Josh: the strain I have is very poorly transformable. Also somebody has a transducing phage for subtilis - I have written for this as well.

(ii) We propose doing the  $r^+$ / $r_{II}$  protein experiment next week having got the Oxford electrophoresis to work. I am most hopeful.

(v) Jim's work has not got much further. I do not believe that they have any evidence for specific peptides but they are still hopeful.

The boys ~~we~~ came back with the following story from Strasbourg. Paul Berg has purified the RNA synthetase and finds that if he takes the coli incorporating system adds DNA and the enzyme, protein synthesis goes on for hours. He gets the same effect if ~~if~~ he makes the RNA first, then adds particles. But if he tries to isolate the RNA and add it, he gets no effect at all. I think our approach should be in trying to make the messenger in situ

with the engine. Perhaps we can start this when  
more people come.

You may care to know that Heldegard Sampson has  
now delayed her coming here until January which  
should ease space considerably.

Lots of love to you all; we are inundated  
by American visitors

Ever yours

Erasmus