THE UNIVERSITY OF WISCONSIN
COLLEGE OF AGRICULTURE

Madison, 6

DEPARTMENT OF GENETICS

November 18, 1950.

Dr. T. M. Sonneborn,
Department of Zoology,
Indiana University,
Bloomington, Indiana.

Dear Tracy:

This letter concerns the project for republication of papers in microbial genetics, which the University of Wisconsin Press has agreed to undertake, and which we discussed briefly at various occasions. I have waited this long to send you the list of titles because I hoped to have your judgment at the most difficult stage, i.e., the transition from a tentative to a definite list of titles. Such a list is enclosed with this letter. I would appreciate it very much if you could study it and make suggestions that might improve it, or make it a more useful adjunct to your forthcoming (?) text.

Unfortunately, I can't resist a few words of explanation or apology, probably along the lines of the introduction to the collection. The UW Press Committee, on the basis of a questionnaire (which was much too fussy and was sent to too many people) has agreed to a volume of only about 300 pp., so as to keep the cost at a level which will attract a moderate sale. The title list is already of this length, so that (except for a very short paper) any amendations will have to displace one of the titles already down. I have put a o next the titles I thought the most dispensable, but would appreciate it if you would include the titles to be displaced in any recommendations for change.

In making these choices, I have had to keep in mind various elements of availability with respect to length, copyrights, and original place of publication. I could not make any suitable inclusions for yeast genetics, for example: Winge's publications are inextricably secured by copyright; Lindegren's contributions are buried in the preponderant content of his papers, and I couldn't justify Spiegelman's work as the sole representative of yeast genetics, aside from the problem of choosing a suitable paper. Yeast is a lacuna I shall simply have to apologize for. I would have liked very much to put in Ephrussi's acriflavine story - but will await your recommendation as to precisely which paper to include.

If I may make some judgment on my own efforts, the sections on bacteria, phage, and paramecium are probably reasonably well covered, within the limitations of the project. I am not especially happy about fungi or about the enforced exclusion of yeast or algae, but don't know quite what to do about it. It might have been a better idea to have restricted the area to bacterial genetics, which because of the wide diversity of lines of research lends itself best to this treatment. However, this restriction would not best serve the immediate teaching needs which motivated the volume, and in any event, unless there is very strong sentiment to the contrary, I am already committed with the Press to a more general essay.

There may be a brighter side to this, however. This project is very much an experiment. A number of people have already indicated that, if it is successful, it should be extended to other fields (and I have heard that Raper may be doing something similar on myco-physiology at Chicago). If so, it may turn out to be for the better that fields such as fungi and protozoa are not developed here to an extent which might preempt a more detailed and satisfactory treatment. Along this line, Luria suggested putting the volume out in some loose-leaf or similar form which would facilitate additions, and the idea seems sound, as well as reducing the binding costs which are multiplied threefold in the retail price.

Finally (or almost finally), I have tried to avoid considerations of personalities or priorities in selecting the titles, hoping to make up for it in the explanatory introduction. Tatum's name is, for example, grossly under-represented. I hope that my other colleagues and seniors are going to take comparable omissions in as good a spirit.

May I suggest the following fields from which substitutions might especially be considered and for which specific recommendations would be very helpful:

bacterial cytology ; mutagenesis
fungi: ascus segregations ; non-ascomycetes ; Horowitz' paper on biosynthesis
yeast: good cytoplasmic inheritance; mating types
protozoa --
algae- are any of Moewus' papers particularly suitable

You know that your comments will be considered very closely; unfortunately, there are controlling factors which may make it impossible to follow all of them even if I wished to.

Sincerely,

Joshua Lederberg

P.S. If you could possible spare a reprint (or better, two) of your 1943 and 1948 papers, it would help considerably in setting up copy. They can be returned to you in reconstructed condition (as well, of course, as in a complimetary copy of the book).

1 1/17/50

Bacteria

21	Luria and Delbruck	1943	Genetics	(Bacterial mutation - variance analysis)
322	Lea and Coulson	1949	Jour. Genetics	(Statistical distribution of mutants and measurement of mutation rates)
30	Newcombe	1948	Genetics	(Mutation rates and phenotypic lag in bacteria)
2	Newcombe	1948	Nature	(origin of bacterial variants)
21	Lederberg	1947	Genetics	(recombination)
10	Davis	1950	Experientia	(biochemical mutants)
12	Demerec	1948	J. Bact.	(drug-resistance: step mutations)
22	Avery, et al	1943	J. Exp. Med.	(pneumococcus transformation)
12	Burnet and Lush	1936	Austr. J. Exp. Biol. Med.	(lysogenicity)

Phage

28	Hershey and Rotman	Genetics	(recombination)
30	Luria and Dulbecco	Genetics	(multiplicity reactivation)

Paramecium

15	Sonneborn	1943	PNAS	Genes and Cytoplasm I & II
6	Sonneborn	1948	PNAS	(hereditary differences between genically identical cells)
7	Preer	1946	PNAS	(attenuation of kappa)

Fungi

8	Beadle and Tatum	1941	PNAS	(1st Neurospora paper)
18	Beadle & Coonradt	1944	Genetics	(Neurospora heterokaryons)
??	Lindegren	1933?	Torrey Bot. Club	(Ascus segregations)
16	Keitt and Langford	1947	Amer. J. Botany	(Venturia: inher. of pathogenicity)

28 T + ?? + introduction + bibliography

Total should not appreciably exceed 300