not sent

October 23, 1952

Dr. Andre Lwoff
L'Institut Pasteur
28 rue du Dr. Roux
Paris 15, France

Dear Dr. Lwoff:

Dr. Hershey has forwarded the request you had sent to him for Het stocks
of E. coli K-12. I am fully prepared to discuss this with you myself, keeping
in mind that we have some interest in these studies still, ourselves.

In fact, I cannot refrain from indicating that I was most offended that
you should resort to another channel, as it were, behind my back. This is
especially grievous to me because of the high regard that I have had since
student days, and continue to have, for your person as well as your scientific
achievements. I recall another incident that perplexed me greatly at the time.
While we werealready working on lysogenicity in K-12, coincidentally with
your own magnificent studies on B. megaterium, your discovery of induction of
lysis was being applied to K-12, in consequence of your visit to Caltech. We
finally learned of this by vague rumor, rather than any direct account, not-
withstanding the fact that we had provided the essential materials in the
first instance: in particular, the indicator strain. I do not mean that we
have any patent on anything that is distributed, but this pattern of beha-
vior is hardly likely to evoke the most friendly and cooperative spirit. It
is very easy to forget that another laboratory may have as deep an interest
in a problem as oneself— perhaps I have been guilty of such forgetfulness
myself. But a friendly and constructive solution to such difficulties can
only be remedied by amicable communication, and discussion of the apportion-
ment of programs and mutual confidence. I regret that this has not ensued
for the K-12 studies. I ask your advice on the measures that should be taken
to remedy it. We have the right, I think, to ask for a certain freedom of
movement to exploit lines of work that have opened up from the investment
of a great deal of effort in the development of stocks. We would prefer that
this freedom flow easily from a spirit of mutual confidence, not roughly
from an antagonistic monopoly of experimental materials. Until such confidence
has been affirmed in practice, the most reasonable course would seem to be
that we discuss the lines of work on which some overlap may be expected. I
do not believe that I have refused any reasonable request for stocks, and I
think you will agree that the flow in the past has been greater from Madison
to Paris than the converse. With respect to materials that are the subject
of our immediate investigations, I believe I have the right —and the obliga-
tion to students and colleagues who are also involved— to enquire on the ef-
fect that their distribution will have on our long-term program.

May I illustrate this with an example

May I illustrate this with a message that I would like to ask you to communicate to your colleagues, Jacques and Mel. I hope you will believe my assertion that I had decided on this several days ago, and was planning to write to them soon. Jacques and I have shared an interest in gene-enzyme relationships that dates, on my part, since about 1946 (and much longer for you, of course). At a time when the one-to-one theory seemed unassailable (and which I initially accepted myself) experimental results with Lac- mutations in K-12 pointed towards considerable complexity. My resources of time, facilities, and collaboration were, perhaps too, limited but I had hoped we could continue a fairly elaborate program. A good deal of time was spent in producing the mutants, their genetic analysis, and the characterization of the lactase. Monod and Cohn have worked much faster and better on certain aspects, especially the immunochemistry and one can conclude from their physiological, no less than our ~~my~~ own predominantly genetic, studies that the gene-enzyme relationships are not so simple as most people had thought original. On the whole, I think our relationships have been fairly amicable (to disregard a few ruffled tempers), although each of us has naturally exaggerated our own studies in our own writings. I had a postdoctoral fellow here who is doing immuno~~chemical~~genetic studies on E. coli, and I had hoped he could extend his work to the coli lactase. The idea of such an approach was not unique to Paris, although the brilliance of its execution has been. Still, I thought we could build on the methods and information accumulated by Cohn and Monod, and apply them to our system, to correlate with genetic studies. For this reason, I felt that we should hold on to our collection of Lac- mutations at different loci, although several of them were sent to Paris. It is now apparent that we were unduly optimistic about being able to do this in the near future. New developments such as the genetic transduction in Salmonella, and the compatibility-factors in K-12 "sexuality" have drained most of my own time. If the problem is not superannuated, I would like to ask whether your colleagues would still care to execute an immunochemical analysis of the Lac mutants— if so, I will be glad to send them.

If I may conclude so wordy a statement, we will be very happy if we can create a situation where we can freely exchange ideas, information, and materials on a foundation of mutual interest and confidence. It is not easy to expose so confessional a tone, but I hoped it might be the best way to ameliorate resentments that may have built up on both sides of the Atlantic. One of the elements of this foundation should be that each of us has an interest in the development of one's own experimental material, albeit this interest is not exclusive. What is it that you think should be done with K-12 lysogenicity that we had not thought to do, or that represents a fixed conclusion that requires independent confirmation? There is more to complicated stocks than the cultures themselves— there is an accumulation of information on their history and behavior that is very difficult to convey in a letter. Certain aspects are rather fluid—e.g. the relation of linkage studies to the compatibility factor, and a full appreciation of them requires, if I may say so, considerable indoctrination. I do not think that anyone would have the slightest difficulty in reproducing any of our experiments, as they are published, but extensions of them may entail a very heavy investment in the development of appropriate stocks.

Yours sincerely,

Joshua Lederberg

P.S. May I make a faint rebuttal to your review of Werkman & Wilson's "Bacterial Physiology". Certainly one may argue that phage ought to be taken up in more detail, but this is usually done in Virology rather than bacterial physiology. My own chapter was one of the most peripheral in the book—and in it, phage the least central item. I could not give a full historical summary— if I quoted the earlier French writings (which very few of the students at this level would be able to read)

I should have to go into the moot question of phage as a "hereditary viciation" as against a parasite. I did spend too much space on phage as it was, and based this en an erroneous impression of transformations as possibly resulting (in any significant instance) from phage infection per se. I submit that you were overzealous in reporting that Burnet's early work was ignored. I should have liked to see a proper treatment of phage physiology, but this was beyond the scope of the book. A more appropriate criticism would be the way in which enzymatic adaptation was ignored. Unfortunately, this subject fell between the many stools of the several writers and editors, and by the time this was realized, it was too late to pick it up. This does not, of course, remedy the defect.

J. L.