MS. DIV.
AC. NO. 72-197

October 20, 1948

Dear Professor,

If only I had finished a letter to you one of the innumerable times I have thought of you, I should be more content with myself. I have only one reason for not having done so — and that is not a reason, really, just a feeling. So long as our legal situation continues to be irregular over here, I am troubled and haunted when I try to write to friends in America. From here America seems sometimes a haven, where one can still lead an orderly life. I am here over a year now, and our case is still buried in a file somewhere at the tribunal. There is nothing we can do with the legal system, which worked well enough for Napoleon, but which, in an apathetic 20th century state, is now grinding slowly to its ruin. It really makes very little difference to me over here, where people are so used to having things proceed in an irregular fashion, because our every day life and relations with friends is just what it would be if the legal difficulties didn't exist. But, as I have already told you, I find it difficult to explain to people in the States.

Rollin's visit here — and in England — was in every way a success. Reports have come from the English laboratories that indicate he captivated the British Isles. Here in Paris his usual difficulties with language were augmented by the fact that he had a French audience. Nonetheless, the beautiful work which he reported came through the strain triumphant. In personal contact he won many friends. Indeed, there is afoot a plan to bring Rollin over here for a year (after our new genetics Institute will be finished) with the aid of the Centre National.

Only very recently my appointment has been made officially. Through your generous help I was made a Chargé de Recherche, which is equivalent to an assistant professorship. I am very content with

this, for it is a good beginning. These are yearly appointments, but are renewed if there is even moderate reason to do so. Also, it is an appointment which permits me to retain my American citizenships, and that means a great deal to me. A number of the people on the committee making appointments, since, have inquired about you, and told me effectively, how great an honor it is to have been supported by you in my application. I really know this already, very well, and shall try very hard to justify it.

From the transformation point of view, the year has started very well. It makes a big difference to start up work after vacation, with a year's efforts behind one. I have outlined very concretely a program for this year, and have begun a vigorous execution. I am interested first of all in analyzing the apparent interaction between a T.P. in the cell, and one in the environment. This program will go slowly because there is a large amount of preparative work involved (which I shall do entirely myself). I now have a technician for ½ a day, to work on non-routine things (media glassware etc. are all taken care of). She is going to carry on the work on improving & developing a defined medium for transformation. This is also a slow, long range project. There are a number of small, less ambitious experiments I hope to work in, during the year, more or less to fill out the experiments I did in New York. I rather expect Rollin will revolutionize the techniques of sonicating, as soon as he can, and so I shall leave it all to him. For the present, I use our old techniques, with albumin & anti-R where needed. Last year I found I could separate T.P. from Type III polysaccharide, in my new technique of preparation — which resembles Mac Leod's very much. I hope, then, to accumulate, during this year, polysaccharide from the mutant Type III forms, so that by direct study of the polysaccharide it can be seen how these forms differ — if they do differ in a qualitative sense.

I am invited to England in April, to the Society of Microbiology meeting, at which there is to be a panel discussion of the nature of the bacterial surface. Our pneumococcus story touches at so many points upon the question that I hardly know where to begin. Already I am sorting things out in my mind, because I want to do justice to the material. I am thrilled with the idea of visiting England, which I have always longed to do. And it will be fun to meet the English scientists.

at the same time Boris is invited to give a series of lectures in England and Scotland — at London, Edinburgh & Glasgow, where the main genetics laboratories are. It should be a grand tour, with Boris doing most of the talking. He will have never two lectures to prepare; and he lectures so well, I shall enjoy even the repeats!

Perhaps you have seen Medawar's longish paper in the new British journal, Heredity. We both read it with great interest, feel his data are suggestive, but not quite adequate yet to prove his "transformation" theory. It is an extremely difficult thing to prove, because one cannot work with "pure cultures" of melanoblasts.

Perhaps also, you have read of the tremendous "trial" of Mendelian genetics in Russia. Being somewhat closer to that state, Paris has been shaken by it. Also, since communists are a recognised & powerful minority in France, it is natural that such an event be of first importance in the Paris press. One of the leading liberal papers ran a series of articles by French scientists on the pros & cons of the "trial". Boris was asked to write an article, and for three or four days struggled with the idea of putting something down on paper. Each day he got unhappier because it was clear that journalism and scientific writing are such different things. To be a journalist one has to toss an article off in a few hours' time and not have too much conscience about the precision of what was written. The upshot was that he did not write an article, but instead, obtained the complete report of the "trials", in the Pravda. (He reads Russian, it being his native language.) The trials — and it is just to call them that only because they result in sweeping condemnations — were on an enormous scale, and it took days to read the reports. I am tempted to write just a little about them, because the issues at stake are so great, and few enough people will take the trouble or have a chance to read the reports. For us, reading these records put an absolute end to any hopes that science played any good role in the Soviet Union — or that science could flourish there. The most important point is this: that the merits of Lysenko's work (if they exist) are of no consequence in the question. The decisions in Moscow are not concerned with the scientific merits of the supposed "Two schools of genetics". These trials are concerned with establishing once & for all that dialectical materialism is the sole true doctrine, and any scientific facts which are irreconcilable with this doctrine must be discarded as false. Lysenko has developed the thesis that modern genetics is "undialectic", a position which incidentally is totally without reason, and has twisted modern genetic theory to make it imply things it was never intended to

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simply. By arousing the purely demagogic elements of the communist party on this basis, he has convinced the highly nationalistic people that modern genetics is nothing but capitalist propaganda. The most significant aspect of the trial, around the thesis of Lysenko, is the state of mind of the defendants, whose speeches will probably never be translated for the English speaking world. (Unfortunately everyone seems to feel it is more important to read Lysenko's speeches, to gain an impression of what went on.) Three or four of the best geneticists in Russia defended themselves, but against cat-calls, boos, and interruptions from the floor of the Academy (the Agriculture academy). But on the whole, it is clear that everyone is in terror of the consequences of not being on Lysenko's side. Several, after a defense of modern genetics, had to completely turn around and run into the Lysenko fold — amid boos and hisses from the assembled academy members. All effort to resist were vanquished when Lysenko read that the Central Committee of the Communist Party supports his views. Nothing quite equal has happened to scientific thought since the middle Ages — and because science is now so developed, one can say never has such a large scale extermination of the scientific approach to problems occurred before in the history of man. It is a crushing story. The perversion of science by the Soviet Union is equal to that in Hitler Germany at its best, though its practical consequences are not so brutal and so immediate.

Equally discouraging are the failures of communist-scientists in England & France to defend what they know to be scientific fact against destruction by party doctrine. I have never felt so strongly before the importance of socialist & liberal organizations, and the kind of thinking they do. It seems to offer the only hope for a saner economic management of material resources, which at the same time permits free thought. But it is David against a Goliath — goliath being a clever, dogmatic and absolute propaganda, spreading throughout the world.

These things are very much with us here in Paris. The love of our work, the enthusiasm of our students, and the joy of being together are the bright sides. And to the bright side one can add the feeling that our friends in the U. S. are prospering, and free to work and think, in spite of contortions of the communists & the contrary! What an important, if fallible, country the U. S. A. is, and will be in the years to come.

By now you are in Tennessee. New York will never be the same again for me — nor the Institute. In a way I am glad I wasn't there when you decided to leave, because I would have been "dead again" it, and very unhappy about it. As it is, I don't quite believe it yet.

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and wont until I see it with my own eyes.

I saw Dr. Heidelberger, briefly, here in Paris the other day. We had lunch together, with Dr. Wurmser, a physical chemist who is interested in antigen-antibody reactions. He (Heidelberger), certainly swims around well in French. We scarcely used any English for the two hours of the luncheon - He looked very well, and had many interesting adventures on behalf of the U.N.

Boris' work is going very well. Effectively, it is the study of a mutation in yeast, induced by acriflavine. The acriflavine acts upon yeast cells in some fashion which causes the cell to lose a group of enzymes. Cytochrome oxidase, succinic dehydrogenase are two which are lost. This change is hereditary — but not Mendelian. That is, thus far, the chromosomes do not appear to directly control the presence or absence of these enzymes. This is a rather surprising thing, in view of the numerous cases in which a single gene appears to cause the formation of a given enzyme. At present, he suspects that the acriflavine acts by inhibiting the reproduction of certain cytoplasmic particles, the particles containing the enzymes, and also being autoreproducing. He has accumulated a tremendous amount of genetic & biochemical information which is most easily interpreted by such an hypothesis, but, of course, at present it is only a working hypothesis. He and his group of workers are in the process of writing it all up, to appear before so very long in the Ann. of the Pasteur Inst. Tute. It makes a very nice story, and I have to admit that as experimental material it is just as pretty as the pneumococcus.

I am going to go back now to today's experiment — and put some T. P. — that mythical genetic "protein" — into some tubes of containing unexcited R cells.

With great affection as always,

Harriet.

P.S. My congratulations to you, on your birthday, which is tomorrow, I believe.