Dear Professor Lubarsky,

my best thanks for your kind letter and the most interesting papers, which you sent me and were unknown both to me and my wife so far.

I don't exaggerate in paying, that I became quite excited in reading them. You probably understand this state of mind: to meet again—after a five months compulsory pause in research—the most familiar ideas (partly accepted and emphasized in our previous publications, partly disputed at length between ourselves) some of which we had before eyes in planning our following experiments.

The most we appreciate in your recent research, is the experimental establishment of identity between the S-forms and protoplasts, highly probable not yet proven, however, before.

Allow me now to return to some point of your report, which are of particular interest for us.

1) In connection with your research on the formation of protoplasts in minimal medium I want to note, that in our experiments too, when using the semi-synthetic medium proposed by Heddi and O'Kane, the protoplastic transformation was irregular too.

2) Neither did we obtain, so far, direct microscopic evidence of the multiplication of spherical elements.

3) In the contrary we could very often observe the active motility of regular pleurome globules. We had in mind...
to make a short microradiographic record of these moving globules. In several of our electron micrographs these appeared, as if globules were attached to the large vacuolar bodies, which could be held responsible for their mobility. Sometimes the explanation of mobility quite obviously lies in the fact, that a "vascular protrusion" persisted (or represented) — similarly to you, we also found very often the complete loss in some cases, however, merely the marked decrease of expulsiveness of the vacuolar globules.

1) Our experiments have shown, so, that T.T.C. is reduced by the vacuolar globules.

5) Our observations on the normal capsule formation have also provided us some preliminary observations of mine in 1933, when I registered a striking contrast in capsule formation between the typical long form, and chains of bacilli, arisen under the influence of penicillin and the typical bacillus, in the other brand (India-rose method).

6) The if our future plans was to look after the phage synthesis in several planes of the Y-cycle, as to indicate the biosynthetic ability of cells, these living macro in the course of this development.

7) The idea to a "hybrid" bacterial process of elements of different types arose from the observations then. Some fraction of phagocytic elements can take place in these instances as observed by the electron microscope and more long
by the light microscope (staining) and our own observation.

b) The usual property that bacterial DNA/RNA is changed in
the L-form is in behalf of the DNA (as demonstrated by Tularen
and Vinderi in case of Diplococci, L-form). Finally there is the presumably entire uptake of alien
DNA by organisms loosing the cell walls.

8) Our views on the possible mechanical role of apo. (and
other instances) in the viability and especially, regeneration of
plasma globules is in full agreement. In our assumption
the filtration of L-culture etc. can be partly traced back to
the filtration sensibility of the elements they consist of (one
note April 127, 5, 1953), partly however is due to some
visible granules that measure about 200 mµ-5 (or rather above
300-500 mµ as demonstrated in new filtration experiments of
Kleinberger, Nolbel and Sirkovics, the value being also given
without filtration by Those). We don't know yet, what are
the reasons of the very often failure (i.e. the very rare success) in
obtaining filtrable forms since 6 times we mentioned in the above
article. But I must tell you we do not attach much
importance to the filtration recently (since this is a very
relative value and cannot be considered as a biological form),
the thing that's biologically important is that there exist
such subcellular structures, which are visible and the twist
of them considerably smaller than the bacterial cell (envelope).

3) From this, it follows practically, that accepting
though, that an essential (organizational) unit
corresponding to the content of an intact cell is still required for the persistent viability of the postplasmatic elements. I, nevertheless, consider them as they were at least the essential component of the cell, as acellular living matter. (It is quite obvious, that a certain amount of RNA, i.e. DNA and some type of organization is inescapably needed to life.)

Excuse me for this long letter, but for the first time since many a month I felt the need to discuss these problems.

Finally some personal news: I myself still have no job here, my wife got a fairly good position at a pharmaceutical firm. she now has the possibility to carry out basic research in the next future. We, however, do not give up the hope to work together in the future in the biochemical and genetics of pleasure glands. I got a definite negative answer from Rockefeller foundation to any application, so I don't know whether it will be easy to apply again. The more I have since, since I agree with you, that first I ought to have that junior position before going once more to you to study the methods you adopted in genetics and which are of biggest interest for my future work.

Sincerely yours,

[Signature]
Curriculum vitae.

I have been born in 1923, in Kecskemét (Hungary). I have finished my public and high schools in my native town. After the occupation, in 1941, I was not allowed to continue my studies at the University Medical School due to the German (Nazi) occupation of the country and the war. In 1944 I was deported to the German concentration camp Bergen-Belsen. After my liberation I have spent 6 months in Switzerland, in a refugee camp just until my return to Hungary in August, 1946.

From September, 1945 - to August, 1951, when I graduated as M.D. I attended to the courses of the University Medical School, Budapest.

As undergraduate I joined to the Communist Party 1945, was however, excluded in 1949 as "class alien" whose father is a big house owner and who doesn't want to take part in the class struggle along the line the party wishes and determines, fellow in the contrary - his own ideas.

In 1949 I married my wife, medical student at the time, too.

After my graduation, from the 1st of September, 1951 I have been working in the Institute of Microbiology, University Medical School, Budapest: first as post-doctorate fellow (1951-53), then as lecturer (1953-55), ultimately as assistant professor until my recent leave from Hungary. I have taken part both in the training of medical and science students, and in research work on the other hand.
My research work was concerned with the atypical, bacillary forms of bacteria: plasma globules (granules), filtrable forms. Due to three successful filtration experiments in 1935, I could follow up some phases in the life-cycle of Salmonella enterica var. Dancy, partly light-, mainly, however, electron-microscopically. (J. Julién: Acta Physiol. Hung. 26,5 (1935), J. Julién, B. Lovas, I. D. Egry: Acta Physiol. Hung. 47,8 (1935), J. Julién, B. Lovas, I. D. Egry: Biol. Közl., and J. Julién, H. Rosenbery: Unpublished data (1936). — The observation of filtrable forms was found more effective by the aid of the public microscopic network of fibrin, when chicken plasma was added to the protein media used before (J. Julién, J. Vádai: Nature 138, 176 (1935), J. Julién, J. Vádai: Acta Biol. Közl., 151, 6 (1935)). Later on, however, in consequence of these filtration experiments yielding promise, even unsuccessful results, we ceased to carry out further filtrations. We merely tried to follow up the order of plasma globules of Salmonella under the effect of penicillin and the reversion of the no gained plasma globules into the original bacillary forms. We have succeeded in making a microscopic topographical record of the above processes (particularly of several morphological Types of reversion: J. Vádai, J. Julién: Nature 138, 176 (1935) and Biol. Közl. preliminary report, Acta Biol. Hung. 111, 6 (1935) detailed description) — Recently our main problem (Dr. Julién, J. Julién) was to gain a standard material of plasma globules of Salmonella, suitable to be examined.
both biochemically (from the point of view of protein and nucleic acid synthesis) and genetically (recombination of different genotypes). The investigations themselves, remained unfinished, preliminary experiments because of the Hungarian Revolution, last year. In spite of this one of us (J. Julán) was able to develop a method for producing a pure material of plasmatic element, lacking completely the usual heating forms. This method enabled us to use the material, quantitatively obtained, for immediate biochemical purposes, eliminating at the same time all harmful effects on the fragile plasmatic element (J. Julán, J. Julán: unpublished data, 1956).

I have delivered several papers on the above subject in the Society of Hungarian Microbiologists (1953) and in the Hungarian Biological Society (1954). I have read the main paper: a review on microbial association (published in Biol. Koll. 57, 1 (1958)) at a session of the Biological Section of the Hungarian Academy of Sciences (1953).

After the suppression of the Hungarian Revolution, we decided to flee from Hungary. Between the 3rd of January and the 19th of February we have been staying in Vienna, on the 5th of March we arrived to St John (Canada). Since then, it was in vain I looked for job here.

Dr. Stephen Julán.