

April the 24th, 57

Dear Professor Lederberg,

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

I don't exaggerate in saying, that I became quite excited in reading them. You probably understood this state of mind: to meet again - after a five months compulsory pause in research - the most familiar ideas (partly accepted and emphasised 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 establishing of identity between the L-forms and protoplasts, highly probable not yet proven, however, before.

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

1) In connection with your remark 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 Hedrick and O'Leary, the protoplasmic transformation was irregular, ~~too~~.

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

3) On the contrary, we could very often observe the active motility of regular plasma globules. We had in mind

To make a short microcinematographic record of these moving globules. In several of our electronmicrographs these appeared, as if flagella were attached to the large round bodies, which could be held responsible for their motility. Sometimes the explanation of motility quite obviously lies in the fact, that a "bacterial protuberance persisted (or repeated)". - Similarly to you we also found very often the complete loss, in some cases however, merely the marked decrease of agglutinability of plasma globules in the homologous anti-flagella serum.

4) Our experiments have shown, too, that T.T.C. is reduced by the plasma globules.

5) Your observations on the marked capsule formation membranes are somehow to some preliminary observations of mine in 1953, when I registered a striking contrast in capsule formation between the atypical long forms and chains of bacilli: arisen under the influence of penicillin and the typical bacilli, on the other hand (inoculation method)

6) One of our future plans was to look after the phage synthesis in several phases of the L-cycle as to indicate the biosynthetic ability of cells, resp. living matter in the course of their development.

7) The idea to a "hybridine" bacterial plasmonic elements of different types arose from the observations, that

a) fusion of plasmonic elements can take place in some instances as showed by the electron microscope and more easily

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by the light microscope (Statelin's and our own observations)

b) The usual proportion of bacterial DNA/RNA is changed in the L-forms on behalf of the DNA (as demonstrated by Tuleman and Vanderschuer in case of *D. proteus* L-forms)

finally there is the presumably easier uptake of "alien DNA" by organisms lacking the cell walls.

8) Our views on the possible mechanical role of agar (and other instances) in the viability and especially, regeneration of plasmas globules is in full agreement. In our assumption the filtrability of L-cultures etc. can be partly traced back to the plasticity, semifluidity of the elements they consist of (see Acta Biol. Hung 171, 6, 1955), partly, however, is due to those viable granules, that measure about 200 μ m-s (or rather above: 300-350 μ m as demonstrated in new filtration experiments of Kheineboer-Nobel and Sinkovics, the ^{size} being also given without filtration by Dienes) We don't know yet, what are the reasons of the very often failure (i.e. the very rare success) in obtaining filtrable forms some of them we mentioned in the above article. But I must tell you, we do not attach much importance to the filtrability recently (since it is of a very relative value and cannot be considered as a biological term), the thing that's biologically important is, that there exist such "subcellular structures", which are viable and the function of them considerably smaller, than the bacterial cell (about 500 μ m).

9) From this later it follows naturally, that accepting ^{may occur} though, that "an essential organizational point

corresponding to the content of an intact cell is still required for the persistent viability of the protoplasmic elements"; I nevertheless consider them as they lack at least one essential component of the cell, as acellular living matter. (It is quite obvious, that a certain amount of RNA, & of 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 has the promise to get the possibility to carry out basic research in the next future. We, however, do not give up the hope to work together in the later future on the biochemistry and genetics of plasma globules. I got a definitely negative answer from Rockefeller Foundation to my application, so I don't know whether it had sense to apply again. The more I have time, since I agree with you, that first I ought to have had some position ~~before coming~~ ^{before coming} for some time to you to study the methods you adopted in genetics and which are of biggest interest for my future work.

Sincerely yours,

J. P. J. J.

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 matriculation, 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 does not want to take part in the class-struggle along the line the party wishes and determines, follows - on the contrary - his own ideas."

In 1949 I married my wife, medical student at this 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 postgraduate 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, abacillary forms of bacteria: plasma globules (granules), filtrable forms. Due to three successful filtration experiments in 1953 I could follow up some phases in the life-cycle of *Salmonella enter. var. Danga* partly light-, mainly, however, electronmicrographically (I. Juhász: *Acta Physiol. Hung.* 26, 5 (1954), I. Juhász, B. Lovas, M. D. Egressy: *Acta Physiol. Hung.* 27, 8 (1955), I. Juhász, B. Lovas, M. D. Egressy: *Biol. Kézl.* and I. Juhász, M. Rosenberg: Unpublished data (1956). — The recuperation of filtrable forms was found more effective by the aid of the submicroscopic network of fibrin, when chicken plasma was added to the protein media used before (I. Juhász, J. Vadá: *Nature* 208, 176 (1955), I. Juhász, J. Vadá: *Adad. Kort. Kézl.* 151, 6 (1955)). Later on, however, in consequence of ^{the} new filtration experiments yielding moreover, even unsuccessful results, we ceased to carry out further filtrations, we merely tried to follow up the aoid of plasma globules of *Salmonella* under the effect of penicillin and the reversion of the so gained plasma globules into the original bacillar forms. We have succeeded in making a microcinematographical record of the above processes (particularly of several morphological types of reversion: J. Vadá, I. Juhász: *Nature* 168, 176 (1955) and *Biol. Kézl.* preliminary reports, *Acta Biol. Hung.* 17, 6 (1955) detailed description) — Recently our main problem (A. Juhász, I. Juhász) 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 (A. Juhász) was able to develop a method for producing a pure material of plasmatric elements lacking completely the usual bacterial 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 plasmatric elements (A. Juhász, T. Juhász: unpublished data, 1956)

I have delivered several papers on the above subject in the Society of Hungarian Microbiologists (1953) ~~published~~ Hungarian Biological Society (1954), I have read the main paper: a review on microbial dissociation (published in Biol. Köt. 51, 2 (1954)) at a session of the Microbial 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 Juhász