Moscow 28/I 56

Dear Sir

I have read recently your article in Nature v 173 1954, which was very interesting for me, because I work in comparatively near direction, investigating the questions about synthetic reactions stimulated by influence of mitogenetic radiation.

Mitogenetic radiation (I think, You have heart about it) was discovered by my father, professor A. G. Gurwitsch, and intensively treated by him and his school for a many years.

At July 1954, my father was dead, but we - his pupils - are working in this problem in a laboratory which I am leading.

I send you some articles of my parents (my mother had also worked in mitogenetic problem), which, I think, may be interesting for you.

Last time we pay just much attention to biochemical and chemical questions in relation to mitogenetic radiation.

I should be very glad to know a little more about your investigations.

Excuse me for my bad english language.

Your respectfully A. A. Gurwitsch

Moscow, D-57
Baltyisky poselok 13
Academy of medical Sciences
Inst. of Physiology

Moscow
Baltyisky poselok
13. Acad. of med. Sciences
Inst. Physiology
Dr. A.A. Gurwitsch.

Dear Sir,

I was very glad when I receive your kind letter and reprints, which are very interesting for me.

I also think that there are many common points of view between our work and ideas.

But I am a little anxious that you have not a clear idea who I am. My father, professor A. G. Gurwitsch made in 1923 a discovery of mitogenetic (ultraviolet) radiation radiated in vivo (by living systems) and in vitro during enzymes processes. From this time the problem of mitogenetic radiation was expanded in a ~~great~~ wide biological discipline having relations to Chemistry and Biophysics.
My father and my mother L.D. Gurwitsch (also a biologist) with a group of co-workers had worked in this and other biological problems all their life. I have send you the reprints of my parents.
My father died in 1954, and my mother for three years earlier. I am their daughter, also biologist. Now, in Moscow, I and a little group of co-workers, go on to work on chief directions of the problem of mitogenetic radiation, which were beganed and investigated by my parents, and chemical and biochemical problems are very intersting and important for us, as you perhaps have seen from reprint about formation of enzymoids.

Have you heart something about mitogenetic radiation earlier, or not?

It is very interesting for me to know your opinion about mitogenetic spectral analysis. I send you a little spectral atlas which is doing by spectral analysis of selective scattering method using a biodetectors.

It is possible, for instance, to detect a spectra of a radiation emmited by a very short biochemical reactions in vivo.

Will

or to investigate the intermediare steps of reactions in vitro. Reading with extremely interest your's both reprints, I thought that perpaps you may find in spectral analysis some use for you in this sence.

Investigations of my father shows that the principle of autocatalysis (matrix formation) can be expanded from enzymes to the more simple cyclic compounds. For instance, tyrosine can be multiplicated in a glycine solution. This process is investigated not only by mitogenetic spectral analysis, but also by chromatographic method.

It is not publicated yet, but I think that a little later I could send you an article about this.

I should be <u>very</u> glad if you will write me some about your work.

Now any questions interested me very. How do you explain a catalytical action of ferric chloride? What part of sunlight spectra is more active, it is'nt ultraviolet part?

It seems me that your experiments shows wonderful clear all the variety of processes which can occur in a photoactivated substances.

I misch also ask you in what Comptes rendus iras publicated your article in 1955?

With best wishes to you

Yours sincerly

Anna Gurwitsch

Mai 8th, 1956

Dr. A.A. Gurwitsch
Baltyisky poselok 13
Inst. Physiology
Acad. of Med. Science
Moscow

Dear Sir,

Near two months ago I send you a letter with a drawing of spectra of mitogenetic radiation which, I think, could be interesting for you. But I am not assured that you have become it and should be glad if you will by event to sent word about that

With best wishes to you.

yours sincerly A.A. Gurwitsch.

Inst. of Physiology, Acad. of
Med. Sciences, Baltyisky poselok
13, Moscow D-57.

1.6.56

My dear Dr. Bachadur,

I was very happy to receive your kind letter and to learn a little of your family, it seems to me that I have made an acquaintance with your wife and you and I'm very glad that we actually have a great community of interests.

I send y[illegible] a monograph of prof. Otto Rahn - microbiologist in America - which any years ago worked much on Mitogenetic radiation, to a pity, it was written a long time ago, but it is a single book on english and it is possible to get with help of it a general impression about radiation.
There is briefly described a method, but for a last 15 years it was somewhat changed and perfected and I shall describe you a modern state.

Method

Our chief biodetector are winy yeast cultures (we work with Chablis or Muscat, but there may be also others), growing on solidified nutrient medium (agar boiling on beer wort).

Preparing of yeast's detector

1) Yeast's cultures are each fortnight sowed on sidelong agar (Beer wort is sterilized during three days for a half an hour and ~~kipped~~ keeped in retorts. Agar is boiled in wort in quantity of 1,5% and poured out in test-tubes).

2) Yeasts from theese cultures were sowed each 3-4 days in test-tubes with wort.

Cultures on sidelong agar and in wort are growing for 2-3 days in thermostat on 27-28° C and after there are keeped on 18-20° C (our room temperature).

3) 1,5% agar is boiled on beer wort and poured out in Petri-cups. 10-12 hours after this, a culture of yeasts which was sowed in wort some days ago was poured out on agar in cups. (A test-tube must be before that good (shaken up).
A surface of agar must be covered completely with a thin layer of suspension of yeast's cells. A culture in Petri-cups growed in thermostat also on 27-28° C during two days.
It was formed a soft and dense whitish lot of yeasts.

4) In some Petri-cups was poured a 1,5% agar on wort.

In covers of cupes was putting a filtrire paper which absorbed a humidity of evaporation. After 10-12 hours a surface of agar is ready for sowing. For this some yeast's cultures was skined with helping of glass-stick from a lot of culture on agar of previous Petri-cupe (stamm of yeast culture); a culture was rubbed then in approximately 10 ml of boiled pipe-water. It must be a homogen suspension of yeast's cells with richness as approximately diluted milk in layer of 3-4 cm. A sterility by this operation isn't obliged, as this cultures are keeped not for a long time.

Such suspension is poured out on the surface of agar in Petri-cupes which for a some minutes were shaked for a homogen soaking of agar. Then a superfluity of suspension is poured out in a container (it may to serve with the same suspension many cupes of agar) and a scrap of suspension on the bord of cupe (by it's incline) is absorbed with filtrire-paper and cupes were covered.

The cultures must then grow for a 10-12 hours by 18-20° C. (not by bright light). It is better to place theire a little inclined and to marke a lower bord, as by a horizontal position some suspension is keeped in a insignificant menisces which is formed by a solidifying of agar.

In a 10-12 hours such culture is ready as a detector for experiments.

A making of experiments

There cut out from the even surface of culture two pieces of agar with culture with dimension approximately 20×20 mm. One must be a detector and second - a control.

Detector-piece is placed on side so that a lot of yeasts is turned to a source of radiation (a distance in generally is approximately 5-8 mm). Control is placed by the same illumination and temperature conditions, so that a a mitogenetic source can't irradiate it.

The windows in room must be closed by experiments. (it is better in general to make an experiment not near to window) and it must not be in a room even a week source of ultraviolet radiation.

The exposures from mitogenetic sources (for instance from an enzymatic process in quarz container) are very short - 10-15 sec. But for each source it is necessary to try various exposures and to choose a threshold-one.

Each number is a quantity of little buds on one hundred of grown-up cells. So we counte in sum 2000 cells. You see that different hundreds have very different quantity of buds, but 500 cells are, as a rule, sufficient, from statistical point of view, and 2000 nearly always gives a real impression of intensity of buds production. So to known an absolute quantity of cells by this method is'nt necessary always. There formed generally in a few days a sufficient habit and the received results were completely really and reproducible. We counte, for the most part, the coded preparates, i.e. the investigator counting his experiment don't know which is detector and which control and only after the counting he compared his results with a protocol. A rapidity of counting was getting sufficiently quickly and a counting of two preparates takes of 40-60 minutes. The method is statistical and therefore there must be a the seria of experiments. We repeat each experiment 3-4 times. The error of method is ± 12-15%. The middle effect + 30-40%.

Spectral analysis

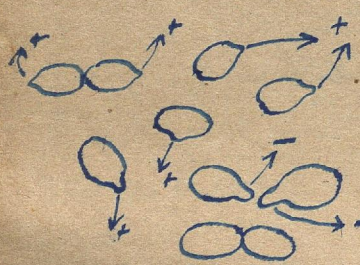
Using this method we make a spectral analysis. A great part of results was obtained on the quarz-spectrograph Fuess (but certainly the work can be made also with other systems). It is convenient because of monochromatic-split moved along the scale of waves-lenghths. The split can be changed relatively to dispersion of districts of spectra. By investigation of spectra detectors were placed, by turns, before that split and so gradually, by threshold exposure, all mitogenetic district (1900-3260 A°) are investigated. Detailed investigation of spectra with a bands of 10-20 A° demands some hundreds experiments which with the counting takes 2-3 months of work. But often it is'nt necessity to analyse in such way all mitogenetic district and we always, by investigation of new question, make at first our experiments in such a manner. A monochromatic slide is raised up by a helical thread and before the large horizontal opening in a focal plane are placed thine metallic plates with a windows each corresponding to 100 A°. All mitogenetic district can be divided on 3 such plates with 3-4 windows on each.

Detectors are placed before each window. So synchronously are investigated 3-4 districts of general spectra of a source which is placed before the collimator-split on a distance of 4-5 mm. We see usually after the counting that in some districts of

After the exposure detector and control were placed in a covered Petri-cupe ~~not longer as~~ 3-4 minutes by 18-20°C, This time is sufficient for formation of first wave of yeast's buds as result of magnetic irradiation. Then both were taken out of cupe and with a bacterial collar or blunt side of scalpel a quantity of a lot of yeast's cells is skimed from theire and transported on two microscopic glaces - slides. Before this ~~on~~ the slides were marked and on the thin layer of a white of an egg with glycerin ~~was~~ placed some fixator - solution (for instance 4-5% formalin). The yeast suspension is carefully stirred on the whole surface of slide, dryed without warming, fixated on the flame of spirit - lamp and stained for a some minutes with water - solution of methylen - blue ~~(0,2%)~~ or krystal - violet or others (approximately 0,2% solution) (for vision it is agreable a blue colour). Then a ~~dye~~ is washed from slides with a stream of water and a blue yeast's cells are rested on a white ground. A preparat can be then dried on a flame.

A counting of cells and buds.

A preparates prepared so are looked with a microscope by an immersion magnifying (by convenient density there are 10-15 yeast's cells in a field of vision). There were counted a quantity of buds relatively to quantity of grown-up cells. The method is statistical, as you see, and so, on different places of preparate it must be counted 2000-2500 cells and registered how many little buds would be on that quantity. The criterion of size of buds must be worked out by each investigator. For your orientation I draw you some types ~~cells with~~ buds. You see that we counte as a buds, which were formed as a result of irradiation, a little swellings which approximately are 1/10 and smaller of a mother-cells (+ we count as a buds, − as a grown-up cells). All that is larger of this size we counte as a grown-up cells. For your orientation I give as exemple a protocol of one experiment.



5		5		1		9	
1		2		3		3	
5	18	6	16	3	10	2	10
3		9		2		1	
4		1		1		2	
2		1		4		2	
0		6		3		0	
5		3		1		4	9
4	15	3	14	5	14	2	
4		1		1		1	
S = 63				S = 43			
On detector				Control			

$$E = \frac{D - C}{C} \cdot 100 = \frac{63 - 43}{43} \cdot 100 = 46\%$$

spectra mitogenetic radiation ~~is~~ is'nt obtained and then detailed analysis must be related only for others. This method significantly accelerated all work.

The dispersion of our spectrograph is approximately 20A°-3mm in district of 1900 A° and that is convenient.

The spectra which I had send you were obtained by a selective scattering spectral method, which is described in one of the papers, as our purpose was to get an atlas of spectra of chemical substances which were not exposed to enzymatic action; and which could be so seing as spectra-etalons.

But enzymatic processes can be analysed without the photoactivation by ultraviolet light, but straightly investigated their chemiluminescenz.

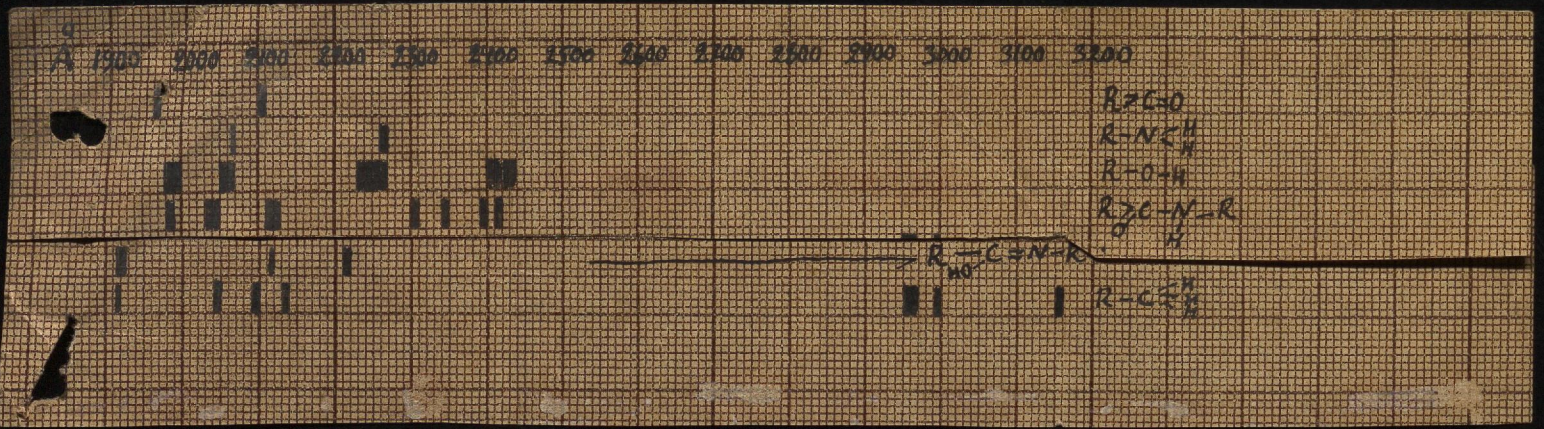
I hope that my short description of method will a little help you and I shall certainly add all that I might if something would'nt be clear.

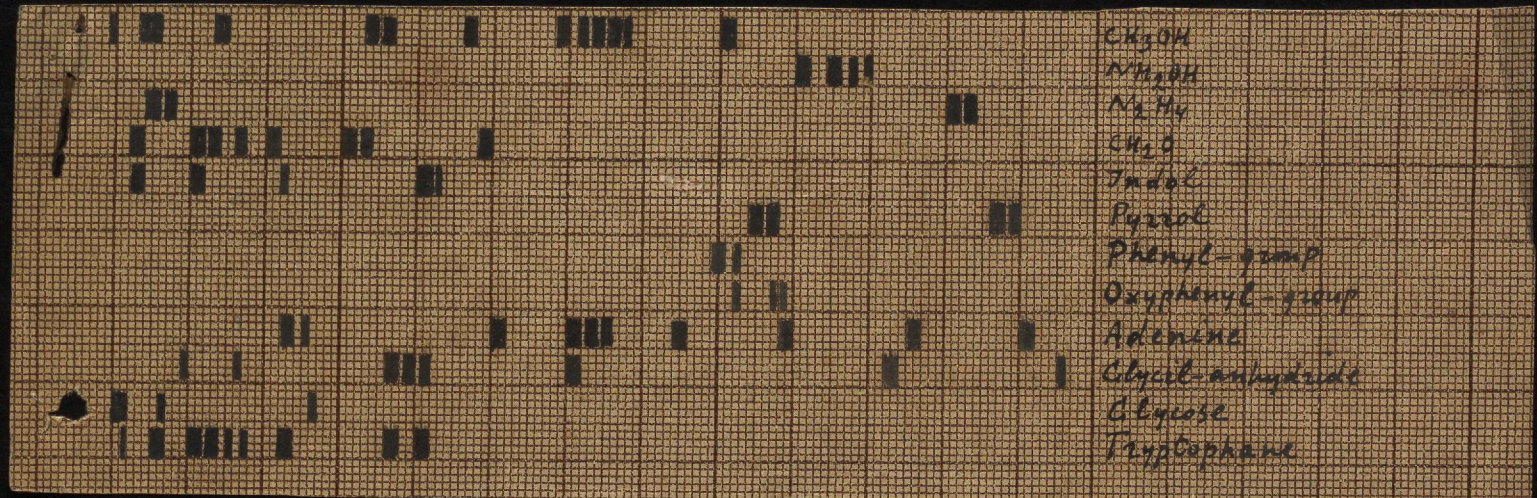
I certainly shall read your paper in Comptes Rendus. Your considerations about free radicals are very interesting for us and I'm very glad that principle of autocatalysis so interested you.

I hope that it would be possible to set a true scientific contact between us.

With my best regards to your wife, your little dauther and son and you.

Your's sincerly Anna Gurwitsch.


A
1900
2000
2100
2200
2300
2400
2500
2600
2700
2800
2900
3000
3100
3200
R₂C=O
R-N<H H
R-O-H
R-C(=O)-N-R H
R-C(=O)-O H


CH_2OH
NH_2OH
N_2H_4
CH_2O
Indole
Pyrrole
Phenyl-ester
Oxyphenyl-ester
Adenine
Glycol-nucleoside
Glycose
Tryptophane

Baltijsky poselok 13,
Acad. of Med. Sciences
Inst. of Physiology
Moscow D-57.

22. 7. 56

My dear Dr. Krishna Bachadur

I was very glad receiving your very interesting letter from 18.6.

Your considerations about the great role of free radicals in synthesis and autosynthesis are very near to our point of view. and for me are especially interesting your dates about the reactions involved in the formation of aminoacids and interconversion of amino acids to one another in the different living tissues of the plants and animals.

Your idea for making a biophysical receiver for the detection of mitogenetic radiation also very interested me.

Recently I received from one plant-physiologist from Hungary the articles in which he describes a new method for detecting mitogenetic radiation based on the measurements of oxidation of yeast cultures. He received very good results.

If it is interesting for you I shall send you a titles of articles and Journal.

I'm very glad that you received an invitation in Moscow and hope that it would be possible for you to come here, then we shall speak in details about many things.

With best wishes to your wife and you

Your's sincerely
Aaron Gurwitch.

Inst. Physiology
Baltyiskiy poselok 13
Moscow D-57.

2/X 56.

My dear Dr. Bahadur

I have received your two letters but the first, from 21 July later than the second.

I'm very distressed that probably it would be impossible for you to come on Symposium in Moscow.

I wait with great interest your paper and think that it can be accepted even in your absentia.

To my regret I can't suggest you ~~some~~ any advise for getting travelling expenses from our country.

Papers of Hungarian plant-physiologist are written on hungarian language with english summary. I send you theese summary and his address, perhaps you will then write him yourself.

Is it succeeded for you to begin something with mitogenetic yeast method?

I hope and want very much that our mutual scientific interest will increase and that sooner or later we shall meet.

It is especially agreeable for me to be in correspondence with you because, apart from our common views which give me a joy, I have a great interest to your country and a great, great respect to Mr. Jawaharlal Nehru the books of whom I read this year. He is a noble man and his ideas attracts me very.

Sometimes it seems me how well it would be to write about my impression to Mrs. Indira Handy, but I d'nt know would it be convenient or not. How do you think about it? Of course I dream to see India but I'm afraid that it's'nt really.

I shall be very glad to read soon your letter.

With kindest regards to your wife and you

yours sincerly

Anna Gurwitsch

2) The application of the quartz chambers also necessitates the use of liquid medium.

The use of the chambers allows the elimination of the separating plug between inductor and detector.

By the continuous agitation, during exposition, of the chambers immersed in the inductor culture and filled with yeast the detector and inductor cells came into very close contact. Accordingly between the two lots of multiplying cells – theoretically – there was only a quartz plate of 0,5 mm thickness. Induced cultures and control, were both agitated by the same shaker, enclosed in the same dark chamber and kept at the same temperature. The control was immersed in sterile water.

Blank tests measured in two quartz chambers gave results of 0 ± 1%. Measurements in the glass chamber alternately against each of the quartz chambers ranged from +3,1 ± 1,0 %. Even this small difference is made apparent by the cell-mass titration method.

Sixteen determinations were made with Escheria coli culture

	Induction	Kontrol	%
1	0,461 ± 0,004	0,400 ± 0,004	+15,2 ± 1,3
2	0,511 ± 0,004	0,495 ± 0,005	+3,2 ± 1,3
3	0,620 ± 0,005	0,663 ± 0,004	
4	0,594 ± 0,004	0,446 ± 0,004	+33,2 ± 1,5
5	0,658 ± 0,003	0,484 ± 0,004	+35,8 ± 1,2
6	0,629 ± 0,004	0,530 ± 0,003	+28,2 ± 1,1
7	0,416 ± 0,004	0,398 ± 0,004	~~0,6,0 ± 1,4~~ +20,0 ± 1,5
~~8~~	~~0,513 ± 0,005~~	~~0,512 ± 0,04~~	
8)	0,423 ± 0,005	0,417 ± 0,004	+13,4 ± 1,6
9)	0,412 ± 0,004	0,398 ± 0,004	+3,6 ± 1,4
10)	0,513 ± 0,005	0,512 ± 0,004	
11)	0,519 ± 0,004	0,460 ± 0,004	+12,8 ± 1,3
12)	0,462 ± 0,005	0,524 ± 0,004	+22,4 ± 1,4
13)	0,415 ± 0,004	0,425 ± 0,004	+40 ± 1,2
14)	0,534 ± 0,004	0,465 ± 0,004	+14,6 ± 1,3
15)	0,474 ± 0,003	0,388 ± 0,003	+22,2 ± 1,2
16)	0,530 ± 0,004	0,533 ± 0,005	0

I Comparative Determination of the Cell-Mass of Yeast Cultures by Oxidimetric Titration

O. Balázs

Department of Plant Physiology, Univ. of Agricultural Science, Budapest.

Summary

....The method of the author consists in adding 1,0 mL. of an 0,001N potassium dichromate solution to the rather concentrated yeast suspension separated from the nutrient solution by centrifuging. The excess of potassium dichromate is determined by iodimetric titration carried out after an oxidation period of 15 minutes. The quantity of titrated yeast is expressed in millilitres of oxidizing solution (0,001N) consumed. In the concentration range investigated, the quantity of oxidizing agent consumed by the cells showed a linear correlation with the concentration of the suspension, i.e. with the number of cells present.

The relative error of the new method is about 1 per cent. Expressed in the percentage of effect, this leads to a variation (absolute error) of only 0,5–2,0 per cent. This fact greatly facilitates systematic investigations on the effect of mitogenetic radiation.

Agrokémia és Talajtan, Tom 3, №1-2, 1954

II New method for the examination of mitogenetic radiation of Escheria coli.

Summary

The mitogenetic radiation of Escheria coli was examined by yeast detector. Radiation was determined in covered quartz chambers by a method hitherto not published in the literature. The application of this method is motivated by the following considerations:

1) The new method used for the determination of cell growth is based on the consumption of the oxidizing agent by the cellmass. By this titration method the error of determination was reduced to ±2% (absolute). This explains the use of liquide mediums and yeast suspensions.

Nos 1-10 show changes in radiation of the same coli culture determined every four hours. Nos 11-16 give results for five-hour periods of the same strain refreshed by large inoculum. Both series plotted gave a typical maximum curve resulting probably from the number of dividing cells in the coli culture. Radiation is not effective through glass according to determinations made in glass chambers.

21/I 1952, Moscow.

My dear Dr. Bahadur,

I'm very sorry that I reply you with so big delay. As by you it was mainly due to the illness of my sister - a strong radiculit - now it is already better.

Certainly I gratulate you very, very with Dr. Sc. degree, I think it is a great event in many relations.

Of course I wait your paper with big impatience and hope that may be it will be possible for you to come to Moscow.

Have you an answer from Dr. Balázs? I have not a letter already for a long time and think that possible now it is'nt time for him for work and writing.

I have n't remember have I write you or not that it is possible to printe in Jena (Germany) a book of my parents about mitogenetic radiation. It would be parallel printed here in Moscow. I don't know if it would be soon but it would be in any case good, as for a long time we could not to printe anything about mitogenetic investigations and I'm sure that a book must evoke a great interest to problem of mitogenetic radiation. In a last ~~time~~ chapter which was written in addition, I try to analyse a cause of decreasing of interest to Mitogenesis and I think that that I wrote is right. Perhaps, if it is interesting for you, I can translate it on english and send to you. Your opinion is very interesting for me.

Have you a success in a cultivation of yeast cultures or have you not tested that?

I shall now wait you answer.

With my best wishes to you wife an you

Sincerly your's Anna Gurwitsch.

(see on 2-d side)

I want very to send you two books: one about Daumier a french painter, pictures of whom I like very much and 2-d about russian painters – Repin

What do you think of painting in general. I like this kind of art very and think that perhaps it would give pleasure your wife and you.

7. 5. 52

My dear Dr. Krishna Bahadur

I received your very interesting letter and paper. But to my regret I saw that we in our laboratory have not provided with some technical possibilities which are so important for your's experiments. I didn't think on that earlier.
For instance, it is difficult for us to make a long experiment in atmosphere without N_2. It is also difficult to saturate a water with carbon dioxide in your's other modification.

It seems me therfore, and I will to know your opinion, that we can only make a short experiment (which you describe in Nature). I mean we can ~~to~~ try to test a mitogenetic radiation from a solution contained paraformaldehyde, nitrate and one from your's catalysers in some hours after the begining of lightening. And then with helping of mitogenetic spectral analysis to see if some free radicals were formed.
Certainly all that must be ~~done~~ made in sterile conditions. But, is it interesting for you?
It may be that latter it would be possible for us to make a spectral analysis of the reactions which you investigated now.

Your experiment with papain is of course very interesting and I shall wait with inpatience your next letter. But it is an other experiment in comparison with our. We get in a little quantity of enzyme or tyrosin in solution of glycine and consider their as „matrix" for formation a new similar molecules from the free radicals, or more large elements, of glycine.
But you know certainly this difference and, if I understand you right, you are interested on the possibility of activation the process

of autosynthesis by mitogenetic radiation of papain.

At any case the result is very interesting for me. I'm glad that the manuscript on autocatalysis of tyrosin is interesting for you and I'm thankful for you want to make some experiments. I took last time some new chromatograms and something is clearer for me. My advise to take a tyrosin in suspension and to get it in a solution of glycine in some quantity is not convenient just as a such procedure is not reproducible. It is better to take a weighted quantity (3-4 mgr on 100 ml³ of destillated water). A filtred solution is poured out in two chemical glaces (40 ml in each). To one is added 1 ml of 20% solution of glycine (molecular relation between tyrosin-glycine is then ≈ 1/300), to other (control) 1 ml. water.

Both portions were standing during 0,5-1 hour on the not bright day-light, without lightening by electric-bulb and without immediate sun-light.

The windows in room must be closed as the ultraviolet of sun, especially of India sun!, can influence this procers. There must not be also at that time in room a sources of ultraviolet light, just such weare, as gas-burners.

I think that such conditions seems you not understable as for the reactions which investigated you, the lightening plays such important role.

But it seems probable, I think, that the photoactivation (a strong also) is very important for such phases of process which are connected with formation of little, energy-reach elements of molecules (free radicals or so on), for transformations-reactions which get you, but can inhibite the phase of self-reproduction of more complicated molecules because in this stage of reaction it is important to keep a less movables of reacting elements. At any case we have some results (now not yet enough) which show that self-reproduction is going better

on the low t°.
What do you think on that?
Now I keep up the t° of solutions on 15-18° C.
After that the chromatogram is made identical as made you, i.e. I mounted a experimental and control solutions in quantity of 0,2 ml. each, dividing these volumes on 20 portions each. If a correspondent quantity of glycine is mounted on the dried control than a drying must be made more swiftly because I'm the impression that the process of autocatalysis gets also on a paper during the tyrosin and glycine are coming in contact.

Now about other. You write that my name isn't clear for you. My father – Alexander (A.G.) Gurwitsch, my mother Lydia (L.D.) Gurwitsh and I'm Anna Alexandrowna (A.A.) Gurwitsh.
It seems me also that we have been knowing each other since long time and I shall widingly write you on my life. I'm much older than you — 48 years of age and though I don't feel the solidity of this ageing, but - - - I'm not married. It was very pleasant to me to hear something about your children. It seems me that your wife I know also a long time. And so I send my best wishes to Mrs. S. Ranganayaki, to little Ranjana ant to more little Chandran.

With kindest regards, yours sincerly
Anna Gurwitsch.

19.6.57

My dear Dr. Bahadur,

I was very glad receiving your cordial letter and wait with great interest the results of your investigations of molecular state of glycine irradiated by Papain.

But at first I want to answer on the question of your syster - Miss S. Rajam. I think that it would be the best if she will write to prof. G.W. Nikolsky, who headed the chair of Ichtiology in the Moscow State University. I have heard that he is an agreeable man and a very good specialist on fishes, and I think that he will try to do something. To a pity I can't offert nothing more. I think that prof. Nikolsky have heard my family name and so Miss Rajam can write that it is my advice.

We shall also soon to close our laboratory for the summer vacation though it is by us much colder than by you, (but the post adress remains the same - on Institute) and I start then (in autumn) with new energy your experiments on the photosyntheris of amino acids.

Some days ago I became from Dr. Roman - editor of Enzymologia a letter and send him our paper on auto-catalysis of tyrosin. But if it would be possible for you to make some this experiments in autumn I should be very, very glad.

Please give to Mrs. Ranganajaki, to already big Ranjana and little master Chandran my best wishes. It seems me actually that I know your family and you for a long time.

I try to read all that I can find about India and mainly I many times read some places in books of J. Nehru, two of which - Autobiography and Discovery of India - were published by us on russian. Besides, I think that I'm perhaps the only possessor in Moscow of the „Conversations Nehru with Tibor Mend" - a french book edited in Paris in 1956, and then I read, to a pity to fast, his book „Glimpses of world history", which

was given me from library.

I seems me that his writings makes on me so strong impression because it is some common between his views and ideas and ideas and opinions my parents, which have come also in us. In any case, I know that a seldom book make such an impression ony my father, when he read Autobiography on english in 1951. He told often to us in jest „my friend Nehru".

Now all my family – my sister, we both are in very intimate, amicable trends, her son – he finished this year a University and is also biologist and husband – all we talk often about his books.

I think I must now finish.

I send you my photograph but it was made 3-4 years ago, I think for some certificate, and so you must mentaly to add these years. More new I have not.

But now I shall wait a photograph of your family and yours, well?

With best wishes, yours sincerly

Anna Gurwitsch.

23/6 57.

My dear Dr. Bahadur.

I'm very glad to know on your arrival in Moscow.
I talk yesterday on thelephone with a secretary of Organizational comotéttee of Conference and he told me that he had abready write you about all the questions which interested you.

He told me that it's the hotel, nourishment all that, is foreseé. We advise you to take a waterproof coat or mackintosh because ~~in August~~ it rains often in August. I think he is right.

He told me also that a Conference will be not in the begining of August but between 19-22 August.

I shall wait your photograph and I have abready send you mine in the last letter.
I think you have abready received it.

With kindest regards to all your family

yours sincerly Anna Gurwitsch.

Dr. A.A. Gurwitsch
Inst. of Physiology
Acad of Med. Science
Baltyiskoy Poselok 13
Moscow D-57

11, 9, 52

My dear Dr. Bahadur,

Of course I'm very, very sorry that this chance of our meeting was lost.

Your idea about the role of mitogenetic radiation in the duplications of certain proteins and other molecules are for me interesting but I feel that I do not understand it. About what radiation exept the mitogenetic you speak?

I shall be very thankful and glad if you will write me more detailed about it and if it would be possible for us to plane some common experiments it would be surely very well.

And what results have you received in your experiments on autosynthesis?

I send your paper to prof. Oparin immediately when you wrote me about it. I think that it would be better if you will to ask him themselves.

With my best wisches to your wife and little miss and master Bahadur

I schall wait your answer

Your sincerly Anna Gurwitsch

23. X, 57

My dear Dr. Bahadur

I have received your litter from 8.10 and read it attently. Your interest for a duplication of protein molecules and to the phenomenon of autoreproduction in general is very clear for me, but I can't agree with the most cardinal in your considerations — with that that the factor which organized quite the same arrangement of the functional groups as by the parents (inductive) molecules, you considered as the radiation.

The absorption of the energy of photon's can give, as you know, as a result the disruption of bond (if the energy of photon is enough for that), the ionization of molecule, i.e. the elementary consequences, which even if it is possible to think about the disruption of quite definite bond by the absorption just the definite photons don't show how it would be possible to connect that with the sterical changes which must be quite definite and specific for the reproduction of the parent molecules. In all this phenomenons and especially in the model experiments in solutions the idea about matrix is necessary and radiation (including the mitogenetic radiation) can be considered only as an energetical factor used as an energy of activation or in a case of mitogenetic radiation as a factor which can disrupt the chemical bonds. So were build our investigations on the autocatalysis of enzymoids and tyrosine and it can't be imagined nothing other, though the mechanism of the action of the matrix, especially one so little as tyrosine, remains until not clear. I want to tell you about the paper of Neugebauer which perhaps remains to you unknown.

— Th. Neugebauer. „Über eine biophysikalische Theorie der autocatalytischer Entstehung der Proteinmoleküle in Protoplasma". Zeitschrift für physikalische Chemie, 200 Bd. H 3/4, August 1952.

I shall write you any phrases from it .....
..... „Nach einer Besprechung der Frage über das Auftreten des entknäuelten Zustandes der Proteinmoleküle in Protoplasma wird eine physikalische Theorie der Entstehung von neuen solchen Molekülen nach einem bereits vorhandenen Muster besprochen. Der Grundgedanke dabei ist, dass die entknäuelten Polypeptidenketten infolge van der Waalschen Kräften solche Aminosäuren oder bereits entstandene kleinere Polypeptidketten binden, die auf dem fraglichen Teil der Polypeptidkette gerade darauf passen. Die dabei freiwerdende Energie wird quantenmechanisch berechnet und aus dem enthaltenen Resultat folgt dass diese Energien dazu ausreichen einige chemische Bindungen zu lösen und in andere zu überführen. Auf diesem Wege werden die bereits durch van der Waalschen Kräfte gebundenen Kettenteile miteinander vereinigt und es entsteht ein vollständiges Analogon des ursprünglichen Eiweissmoleküls."

Do you know that and what do you think about it? Coming from all that out, I can't completely understand the experiment on the autoreproduction of papain, the negatives of which you have send me, for that I'm certainly very thankfull to you. If the enzyme in a small quantity, as a matrix, would be in the solution of the glycine, the meaning of experiment would be for me quite clear, as then there would be the matrices whir are necessary, but how the necessary arrangement of functional groups can be call forth even by the discrete determined lines of spectra of mitogenetic radiation, remains for me as formerly unclear. Besides that chief question I wanted

to ask you if the layer of enzyme which covered the retort was changed? It seems me little probable that it can to radiate for so long a time.
But the clear difference which you received in crystalls (on the negatives the difference is also but no so clear, as you also think) must be explained.
How do you think about that — under the influence of radiation of papain (also if it lasts not a long time) can the polycondensation of glycine arise, till the comparatively high polypeptides. The lactime-form of peptide bonds is typical for such polycondensat and by that its activity as an enzymoids of desaminase can be explained. I write that to you founding on the facts which were received by my parents a long time ago. I send you a paper about it. Such a simple polycondensate can be received by the irradiation of glycine but to think about somewhat more complicated without the matrix, seems for me impossible.
I wrote all that to you so frankly because I know that we are friends and all scientific considerations must be saying openly. It's like me very much when you wrote me somehow that by your, in Indonesia, this is a true friend who say openly his opinion to mitogenetic radiation is very valuable for me and I want very strong that it would be possible for us to work in common on the one of questions which is interesting for you and for me.
It seems me that if the problem of autocatalysis have so large interest for you, so you, as chemist, could work on it certainly more better, than I — and extend this principal having principle to a such biological significance, substances as pyrrol, porphyrin and so on.

I'm convinced only that it must go out by such experiments from the necessity of matrices and the radiation appeared during the process, to consider as an necessary energetical factor in the chain of events which lead to a selfreproduction of the matrix.

You have apparently also more stronger technique as we, for instance we can't to dream about the diffraction spectra.

If you would decide to work in one of such directions so the experiments in which the biodetector of radiation is necessary could nasce ?, I should also very glad if it would be possible for you to use the mitogenetic method but I understand that without seeing it is very difficult to do and that for you to come here or for me to come in India is in a nearest time impossible. But it seems me that if it would be possible to construct in your laboratory the physical methods of detection of mitogenetic radiation than the work would be quite real for you. I don't remember if I have write you that in Germany works now with the counters of Geiger, which were made by a german firm. Do you have enough means in laboratory for the apparatus?

I want very to know your relation to such a common work and shall wait from you the counter-offers and may be also such sharp objections which I have made you.

I want to say you that I'm very grieve that you suffer from the ulcer. It seems me that now are abroad very good medicaments which may be occasionally unknown by you. I try to know that exactly and then write you.

You see that my letter is not shorter that yours.

With my kindest regards to your wife and little Ms. and Mast. Bahadur, singerly yours

A. A. Gurwitsch.

9/I 58

Dear Mrs. Ranganayaki and Dr. Bahadur,

I'm very thankfull for your kind Greeting which was for me especially kind because all that is connected with you country is very interesting for me.

I think, I wrote you already some time ago, how a big impression have made on me the books of Mr. Nehru and I belong to thate large quantity of pupils in the world which connected with his name the best.

I send you some little and not very good reproductions from the pictures of our painters.

Perhaps it would be interesting for you both and you will receive an impression about Leningrad-a town with a big charm.

The other's may be will give a pleasure for both your's little-ones.

I think that you received my long letter - an answer on your's, and I wait now your answer and wont certainly strong for that that our scientific contact will in such or such form to realize. I have read recently your's and Mrs. Ranganayaki paper in C.R. of our Ac. Sc. and was glad that your work go forward.

With kindest regards

Your's sincerly A.A. Gurwitsch

21/II.58

My dear Dr. Ranganajaki and Dr. Bahadur

I received your very kind letter from 21/I and the other's days your parcel. The scarf is wonderful. We all have long looked it and I shall take care it of very (but, of course, also wear), as the symbol of friendship with you and the connection with India.

Your previous letter, which was certainly very important for me, is probably definitively lost, I'm very sorry for that because I wanted to answer you also more detailed. I shall try to do that now.

It seems me that in the main our divergence, if I understand you correctly, is in different meanings which we connect with the notion of autocatalysis. The genuine autocatalysis, i.e. self-reproduction, for instance, an active group of enzyme molecule with inherent to it specificity, is possible only by the presence of <u>matrix</u>, i.e. all the elements which are necessary for the building of molecule and the mitogenetic radiation as energetical source, are not sufficient. That follows from our results and, besides that, it is very probable from the general point of view, do you agree with that?

On the other side, we have the results about the photochemical effect of mitogenetic irradiation which undoubtedly correspond to your ideas.

That is the secondary mitogenetic radiation consisting, as you know, in that that the given substrat must be actually irradiated with the waves-lengths which it can radiate itself, i.e. during the action of specifical splitting enzyme,

I agree that matrix must be there; the question is how its matrix helps.

in this case without any external irradiation.
T.i. only this wave-lenghts, absorbing by the substrat calls off the chain-processes, which in its turn viradiates the waves of the same lenghts.
But, the secondary radiation consists of the chain processes of not synthetical character.
The processes of syntheris can also go under the influence of viradiation, t.i. the viradiation of composition of aminoacids, or of Peptone (we take simply peptone, because we had not the individual peptides), gives the more high molecular combinations.
But such processes, going in the direction of synthesis, does not radiate. For the chain character of synthetic process the energy of the spended in the beginning photons of u-v light, is needed and it seems that such an utilization of energy is more probable in this cases than its radiation.
The polycondensates which appears can not however be considered as enzymoids.
But we have still another modification — if the 0,5% solution of glycene is irradiated during very short time (10-15") from the physical source of u-v rays, (t.i. hydrogen tube) on the distance of 1m or more, so after 30-40' the solution begins to produce mitogenetic radiation which lasts for some hours.
Judging on its spectra (identical with that which produced glycene under the action of desaminase, characterestic for NH_3), on the thermolability of process and on that that active factor does not diffuse through the collodium film, it is very probable that the formed polycondensat have the properties of enzymoids of desaminase.

* to say that polycondensats are enzymoids.

In this case, as by irradiation of peptone, the different wave-lenghts of nitogenetic (u-v) spectra are effective.

I think that I write you that for nothing, because you know that all abready very good.

You see so how complicate this all is. It seems, that it must be formulated so: - the nitogenetic radiation is an necessary energetical factor for the different processes of synthetical character, but for the strong selfreproduction of the molecule - enzymoids, or tyrosine, f. i: it must be besides the radiation, also the matrices which, as it seems, must be of cyclic character and flat from both sides.

Does it corresponds with your views and, as seems me, with near to you views of Kirkwood, the papers of whom I have not yet read, is not quite clear for me.

Certainly I want to ask you to write me once more in details (if it does not very annoyed you), because it is very interesting for me and because I want very to do a near investigation. I'm almost sure that the spectral analysis of autocatalysate, where the matrices was living, as you have proposed, will show the intensification of lines, typical for matrix, because the new molecules are identical to it. I shall try it. You ask me about the experiments with mixture of Paraformald., Nitrates, and Molibd.-Coll. I have not yet do it, but don't think that I have forgotten or don't want to do that. It explains only by that, that

~~the~~ we were only the two and different questions must be disposed in turn.

But I want to see if the spectral analysis can show a free radicals during this process.

So, I end my long, long letter and wait from you. My kindest regards to Mrs. Ranganajaki and little Pampu and Pappoo.

Sincerly yours

A. A. Ovsenitsch

A.A. Gurwitsch
Inst. Physiology
Acad. of Med. Science
Baltyiskiy Pos. 13
Moscow D-57

2.4.58

My dear Dr. Bahadur

I'm not frightened not by the content of your letter, not by its lenght. On the contrary I was very glad receiving it and I feel that more detailed exposition helps me to understand your ideas better.

But I have still any questions and doubts, but I think that must be so?

I want to summarize for myself your pargraphs. I think that I understand rightly that you see the rôle of matrix not in their configuration determining the attraction of similar free excited radicals and in its selfreproduction in this way, but in its specific secondary radiation. Do you connect with that the idea about the localization in definite functional groups or bonds a definite (specific) levels of electrons excitation, i.e. do you consider the secondary radiation as the expression of the „configuration" of the electrons states, is not it? I feel that understand few in this direction and therefore I can't have a definite opinion, (but it seems me that I express now your idea more clearer) You imagine, I think, then that the spectrum of the secondary radiation specifical for the matrix, is absorbed by the similar free radicals and that consequently, through the excitation of the just definite bonds, the probability of their connection and, in such a way, a reproduction of the matrix-molecule, is high.

I think that principally you are right supposing that all the matrix give the secondary radiation. In any case in just such way we

have determined the spectrum of many matrix-molecules, f.i. of tyrosine. You know that the spectral method of selective scattering is based just on that, that the system (solution) is irradiated by the large district of ultraviolet spectra from the any physical source, which is weakened and in some distance from the place of irradiation the feeble spectral lines (our mitogenetic intensity) are detected, which appears as a result of photochemical chain-processes and which are specific for the molecule of system.
That is, this phenomenon is analogous to the secondary mitogenetic radiation.
But I want to remember you the following fact about which we d'ont mentioned in our correspondence. From our experiments with autocatalysis, f.i. with tyrosine, it follows, that the autocatalysis go on only if the matrix have a cyclic ~~compounds~~ construction and that all of cyclic substances, which we have already studied, gives mitogenetic spectrum which is specific for the molecule as a whole, but not for the individual functional groups from which the molecule is builded. That is that in these cases the spectrum of secondary radiation of matrix d'ont be an specific activating factor for the surrounding free radicals.
It seems me therefore that more probable is to imagine 2 types of the process of reduplication, or more general, of the autocatalysis
1) The type of matrix with the „contact" action, it is connected with its cyclic structure. Of course, the rôle of mitogenetic radiation is by that very important, but rather as a

general energetical factor, and

2) The type of chain-syntheses when the element added to the growing chain reproduced (if not absolutely strong), so very near the configuration of the preceeding part.

The secondary radiation of that chain, or more correctly, of their functional groups can have here, as it seems me, for adoling of the following groups, really a decisive rôle. So here I subscribe to your opinion. You have read in the manuscript about autocatalysis that just such 2 types my father had imagined as very probable and the adding of your idea to a second variant can be very fruitfull.

Your experiments with the lighting by sunshine the mixture of glycine and sucrose are very interesting for me. There worked in our laboratory already long time with the near investigations on the possibility to receive the peptides by the mitogenetic irradiation of different aminoacids mixture. The mitogenetical spectral analysis shows very clear the appearance of the peptide-bonds, but their detection by the chromatograms and, of course, by the biuret-reaction is not easy.

And your results are very clear. Don't you ascribe that to the adoling to the radiation energy also a chemical from the oxidation of sucrose? How do you think, what a place that takes in the sequence of processes?

Perhaps it would be interesting to remove an ultraviolet part of sun's spectra, f. i. by a filtering through any gross glasses.

Then the secondary radiation of ingradients could be weaker. And then may be possible to adol the energy from source of any mitogenetic radiation. What do you think about it?

Didn't you think also on the irradiation of solution, f.i. tyrosine and glycine by any ultraviolet source, may be by hydrogen-tube (the light must be weakened on some (2-3) orders, because it is in general better to work with weak artificial ultraviolet light.), and chiefly spectral divided for examining which spectral district is important for the intensification of the process which go already. The spectra of secondary radiation of tyrosine (2200-2800 Å: - 2220-30 Å, 2770-90 Å), or the spectra of functional groups coming in in the molecule of glycine:- R>CO, R-NH_2, R-OH, the common district of which is ~ 1950-2300 Å, or the spectra of the same groups as free radicals:- CO (2020-40 Å), NH_2 (or NH) 2520-40 Å.

Is'nt it possible to use for that a quarz spectrograph which probably is by physics in your University.

You see how a long letter I write you also. I finish it but I wanted only to tell how I value your interest for making more clear the rôle of mitogenetic radiation in chemical processes. I think that many of your idea and results are very interesting.

I hope that soon I shall be more free and shall try then the detection of free radicals in your model.

Now I wait with impatience your answer.

With my best wishes to Mrs. Ranganajaca and both little-ones

Sincerly your

Anna Gurwitsch

21/V 58

My dear Dr. Bahadur

I write you not waiting your answer because I want to tell about the ~~experiments~~ spectral detection of free radicals by the repeating your experiments with the mixture describing in your paper in Nature.

But I could not take the recipt exactly as, at first, I had not paraformaldehyde but only a saturated (with a little precipitation) solution of formaline which I took in a quantity of 10 c.c. to a 500 c.c. of distilleted water and, secondly, because I also had not a ferric chloride and take, therefore, 6 N ferrum chloride ($FeCl_3$). KNO_3 was added, as by you, in quantity of 0,5 gm to this volume.

That mixture was coloured by room t° (20-22°C) yellow-orange, but after the boiling (for sterility) it get a dark-orange colour.

After the cooling it was kept into two glass retorts in. 250 c.c. each, and one was hide in the full darkness, the other lightned by an 100 w. electric bulb. I made the first experiment after the 18-20 hours and 2-nd – after the 40 h. of lightning.

It was taken from both solutions a little volume in a quarz vessel and their ability to give a mitogenetic radiation was tested.

Only lightned solution radiates (by 15-20" of exposure on the yeast-detector the positive effect was received, so the radiation can be evaluated as strong, according to the scale

1. See whether $-CHO$ group (radical) is present. Oxalic

$2H. + H-\overset{O}{C}-OH \leftarrow H_2O + CH_2O.$

2. See the stage in due course.

OH, $-NH_2$, $OH-OH$, NH_2-NH_2, $OH-NH_2$, $OH-CO-OH$, $OH-CO-C{=}O-NH_2$, $NH_2-CO-NH_2$, $OH-CO-NH_2$

$H_2O \nearrow +H.$ $NH_3 \nearrow +H.$

In strong reducing atmosphere, when the reduction of nitric nitrogen takes place the reduction of $COOH \cdot CO \cdot NH_2$ takes place.

$COOH-CH_2-NH_2 \leftarrow$ CH $OH-CO-OH$

& intensities of nitrogenetic sources).
The "dark solution", tested by the different expositions, did not give the effects.
Then, I test the spectral lines which characterizied any free radicals. But I could take only two radicals —NH_2 and —OH, because the spectral line of the third >CO coincides with the one of lines of the formaldehyde spectrum.
NH_2 and OH are characterized only by one line each, but it was shown that the right and left sides of the both lines are were limited very clear and so they were typical in their position for there radicals
If the spectral lines are stronger after the 40 h that after the 18 h, I can't yet to tell with certainty.
What do you thinc about these results and about the reactives I took in?
I was very glad that I could do that at least, though it is certainly not much.
I shall wait your answer, perhaps there would appear some considerations.
Do you receive my previous big letter?
I'm a little anxious for you don't answer on it. Are your family and you quite healthy?
With my kindest regards to Mrs. Ranganathan and you
Your sincerely Anna Gurwitsch

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x) I have received from Dr Roman, the editor of Enzymologia the letter, he write tha the proofs of the paper of autocalysis will soon be.

A. A. Gurwitsch
Inst. of Physiology
Ac. of Med. Science
Baltyiskay Poselok 13
Moscow D-57

28. 7. 58

My dear Dr. Bahadur

I receive your letter from Chidambaram already a long time ago and have not yet answered you, partly because I was very busy in laboratorium (I begin my vacations from the 1st august), partly because the mood in connection with different events was not very good. Moreover agreeable for me was you letter, where you have described the nature of south of India and the temples so vivid and picturesque that I dreamed many days to see all it.

Now, you are surely in Allahabad (when I have read in our newspapers that Mr. Nehru spoke recently in Allahabad University, I thought were you already there or not). How is it now with the summer-heat, is it possible already to work or not. How is now with your pain, it distressed me very much.

Now, about the work. I was very glad that the results with free radicals seems you interesting. Of course it must be made with the same reactants which have you.

It is very agreeable for me that our interests were so near. I have think last time much on the possibility of realization the common investigations of the questions, which are interesting for both of us. You must know that you are one, for the present, between the chemists who had a serious interest to our problems. I understand, of course, that that explains by your own investigations, your ideas and hypothesis are lying in the domain which is near to our biochemical work. But, in any case, it is very important for me to know that there is, even geographically far, a chemist who think many and seriously about our results and views and try to connect it with his

results and ideas.
I think, I wrote you already (perhaps many times) about the big scientific isolation in which we worked earlier, with my father, and works now. It can be explained only by that that the results and the analysis of the facts, made by my father, have go far ahead from the classical modern point of views, and the scientific routine is very strong. I think you feel that also.
It remains only to continue the active and strong work in order to overcome this situation.
It seems me, when I think on the possibility of our common investigations, that, principally, it would be well, in the interesting for us both, questions you will give us the „order". That is, in these problems (or stages of problems) in which the fine and very sensitive method of nitrogenetic spectral analysis may be useful it must be used.
For instance, for the detection of the small quantities short-lived substances (free radicals, or others) which, judging from the general considerations of chemists, are probable. But all, that can be made without nitrogenetic method, must be in your hands, because only then it would be made so as must be made, and that is primary, and because our chemical equipment is very poor and our chemical knowledge not very big. And besides that we are only two: my co-worker and I.
That is why it seems me that it would be better if you will be able to do this experiments with peptide-bond formation, if you find it interesting.
The experiments with free radicals I consider exactly as an instance of work which must be done by us. Certainly they are very preliminary yet. I must repeat them and, I think, it would then be necessary to make it with the same reactants as you. Don't you think also so?
I hope that in autumn I shall made that,

but I'm afraid that it will go rather slowly.

When I shall to send you a good quantity of positive experiments than, perhaps, we could think about the common publishing. I want to tell you that the investigation of that question is very interesting for me apart from the publication and it would very distress me if you are worry thinking in order not to offend me anyhow. That is why, I'm of the opinion that you must not think about me in connection with peptide-bond formation. You will write that the formation was also shown by nitrogenetic spectral analysis, by such a conditions, and that is all that you must done. Of course, I shall write you more detailed if it would be useful for you.

Are you agree with me? I consider you as an true "old" friend and therefore I write you all that I think.

I shall wait your answer with impatience, perhaps you have already some ideas in which spectral analysis may be useful. or more general ideas on our co-operation.

I shall be probably the part of my vacations in Moscow and perhaps on the part we will go somewhere. But write me on the same address as usually.

Of course I shall send you the print of the paper on the autocatalysis as soon as I shall receive them.

My kindest regards to Mrs. Ranganayaka and little Pimpu and Papoo.

Sincerly your's A.A. Gurwitsch.


-NH2
-OH
190 200 210 220 230 240 250 260 270 280 290 300 310 320 330 mμ

A.A. Gurwitsch
Inst. of Physiology
Acad. of Med. Science
Baltyiskoy Poselok 13,
Moscow D-57.

29, 9, 58

My dear Dr. Bahadur

I do not answered for a long time on your very friendly letter because I find it here in Moscow only after the returning from the Crimea where we, with my sister, spend three weeks. I'm very glad that we find more and more common interest.

Certainly I shall do the spectral experiments with free radicals and I think that it would be well, in addition to your plan, to try also a mitogenetic irradiation of the solution. What do you think about that? But I think that the work will go slowly. I'm also very glad that you want to develop the chemical line of investigations. We worked earlier in that direction, as you know, and my co-worker try also already for a long time to receive a chemical discovering of peptide formation by the irradiation of the solution of glycine with mitogenetic radiation, but the results are not constant and we both feel that we can bring more profit to the problem if we shall to investigate the biological problems of Mitogenesis.

Of course I'm very glad that soon, I think, we with Mrs. Ranganajaca, will write to each other long russian letters. I send two little hooks, but I don't know if they will be useful. May be, Mrs. Ranganajaca will write me what she want to have and I shall than send that immediately.

The Crimea was well. In spite of it is little, the nature of its different parts is also different. The south coast has a very reach vegetation — cypresses, magnolians, but, of course, no cocos,

as on south India. On the east coast such a vegetation is not, there are hills, covered with burnt out grass and not big mountains. Probably it look like the Greece and greek settlements, as show the archeological researches, were really there.
We lived in the little settlement named Coctebel on the east coast of Crimea. I send you some photos, but of other places. On the first is the bear-mountain, it is called so because it remember the bear drinking from the sea. Ayu-Dag it is called on the tatar language. Ayu - bear, Dag - mountain.
Near from it we, all the family, lived in summers some years when I was yet a little girl, many years ago. And I like this place very.
I shall wait your letter and send my kindest regards to Mrs. Ranganayaca, little ones and you.
Of course I shall read with great interest your paper.
Sincerly your Anna Gurwitsch.

A. A. Gurwitsch
Inst. of Physiology
Acad. of Med. Science
Baltyiskay Poselok 13
Moscow D-57

19. 4. 59

My dear Dr. Bahadur

I Excuse me that I answer on your letter from 28/II only now. I was affected with cold a little and then there was a great press of different work.

How goes your work now and how are your ideas about the going to foreigh countries? I think that I understand all sides of this question - your care about that, the pure scientific interest to work a little in foreign laboratory, your relation to this group of fate makers and, in the same time, your phylosophical relation to all this questions are very attractively for me. It is wright, perhaps, that such problems make the life more interesting.

I continue, but to my distress, not fast the experiments with radicals. The results are good and, you know, it is interesting, it seems me, that the mixture have an ~~cap~~ ability to give mitogenetic after radiation in the darkness after the long
II (18-20 h) of illumination by electric bulbe. The duration of this after radiation I don't know as yet, but it is probable that the recombination of free radicals can go on for sometime after the lighting, how do you think? As soon as I shall have more results I shall send you all protocols.

III Now we, in our laboratory, think over the possibility for working out the registration of mitogenetic radiation by use the physikal methods - sensitive photomultipliers and so on. You know that it is not new because 15-20 years ago the

detection of rays was made very good with Geiger-Müller counters of photons. By different pupils. But we think that now, when this new very sensitive methods are existing, it is our duty to show once anore the possibility of physical detection and its possible use for the simple experiments. I'm sure that for the biological experiments where the kinetics of the processes is so important and the ~~specific~~ spectral analysis so necessary, the biodetection will remain as the best method, but the working out of physical method must also be done. But it is not easy and it would be possible only with co-operation with physics. Perhaps such a co-operation is possible in your university, then you should have a method in your hands.

IV Do you know such a name Dr. K. Nagaratan from pharmacology laboratory of Indian Inst. of Science in Bangalore. He asked me to send him the reprint about the autocatalysis. I have send it him.

I shall wait your letter and I want very to know about Mrs. Ranganajaky and both little ones and about your life, work and plans.

With kindest regards

your very sincerly A.A. Gurwitsch

A. A. Gurwitsch,
Inst. for Physiol.
Acad. of Med. Science
Baltyiskay Poselok 13
Moscow D-57

8.6.59

My dear Dr. Bahadur

I put off my answer on your letter from 11 May, because I wanted to send you simultaneously the book about Mitogenetic Radiation which, as you know, was published in Germany (Jena) and now is coming out. But I did not yet received the exemplares, which they had send me and therefore I shall send it to you later, as soon as I shall receive it.

In the last time the experiments with photosynthesis of amino acids were a little inhibited because the working out of the physical detection take up much time, but I hope soon to continue them.

Of course you are right that a definite opinion about the free radicals mechanism can be formed only after the special studying.

About the physical detection I can tell you the following. Some materials for the photocathode are very sensitive for ultraviolet radiation, f.i. Cu, Al, Pt, Au and so on. The physicists who are working with Geiger-Müller counters or photomultipliers must know that very well. Many of our mitogenetic sources radiate a large wave-lenght district. F.i., so simple as the reaction of neutralization ($NaOH + H_2SO_4$, or $NaOH + HNO_3$, 30% both), or $KMnO_4$ + glucose, $KMnO_4$ + oxalic acid and so on. From the biological sources, hemolyzed blood of healthy rabbit, or rat, or mouse. So, you don't need to know exact the spectrum of this or this reaction. It is enough if the counter is sensitive for the district between 2000-3000Å, but the sensitivity must be good (100-200 photons for sm^2/sec.). It is very important also to have a large quartz window (5-6 sm^2) and a large surface of the cathode.

We don't have now such sensitive counters, but we hope that we shall have them soon.

I re-read, after your letter, the papers of Ottó Balázs. It seems me that his method is based on the proportionality between the increasing of yeast cells quantity and the increase, therefore, of ~~their~~ the reducing ability of the culture. The number of cells increases because the yeast suspension is irradiated by bacterial culture. It seems that the method is very good.

I'm very glad that your things have such a resonance. There are very interesting and important and I'm sure that sooner or later you will go in foreigh countries for continuing there your researches.

How is it now with hot my little friends? I hope that all is good. Where have you supposed to spend your vacation? I shall be very glad if you will write me about all that.

With my best wishes and kindest regards to Mrs. Ranganajaki and you

Sincerely your A.A. Gurwitsch.

A.A. Gurvitsch
Inst. of Physiology
Ac. of Med. Science
Baltyiskay Poselok 13
Moscow D-57

25. 7. 59

My dear Dr. Bahadur

I received your letter from 26.6 and was very glad that the book on Nitrogen Radiation give you a joy. I also hope that it will make a work large way for Nitrogenesis.

Of course I shall inforne you about all the details of our work with physical detection. Now we cease the laboratory work till the 1st September, but I think that then we shall to order a more sensitive and right type of Geiger-Müller counter for the real work. The preliminary experiments during two last months shows that the use of counters is, as earlier, better and simpler than of photomultipliers.

I feel myself ashamed that the experiments with free radicals in your photochemical reactions go so slowly. These facts and the possibility to study also the reaction with nitrogen fixation interested me very much, but the time is a limiting factor. But I think that in the autumn it will go more fast.

I imagine very clear how hot (I perhaps already was?) in Allahabad now and how wonderful is in autumn months and in Feb. and March. I hear often the radio translations from Delhi in the late evenings (by us) and try to make an idea about the life of different parts and pupils in India and think of them on your family.

The climate in our country is different in different parts. In the middle part to which

Moscow belongs, the summer (June-August) is often hot but not so as by you, 25-30°C in the shade is for us already very hot.
But sometimes there are also a more cold and rainy summer. Autumn (September-November) is, as a rule, rainy and temperature gradually go down till 0°, and then a long winter (November- March) which can be more or less colol, from -10° till -30°C. And then a spring (April - June) - the better season (summer also), when all is green.
Now we have also a summer vacation till September, and we with my sister and two friends made a short auto trip in Leningrad, in Esthonia and then again via Leningrad to Moscow. It was very good and now I shall be in Moscow and try to think over the following work and perhaps to write something.
I send you two postcards with views of Moscow. The ~~monument of~~ our writer and poet Pushkin and the building of municipal council. They are mad rough, but, may be, they can done some impression about the town.
I send to Mrs. Ranganayaki my best wishes and also surely to Rangana and Chandran and hope to hear from you soon.

Yours very sincerely A.A.Gurwitsch.

15. 9. 59

My dear Mrs. Ranganajaki

I was very glad receiving your letter. I'm sure that soon you will write me on russian and I, but no so soon, shall try to write you on Hindi, and I'm sure that that would have brought us closer. Explain me, please, the russian words which are near to Tamil words.

You ask me to write you as a lady from Russia, but I do not feel myself as a lady. I don't have such a pretentiousnes as must have lady, as I think. Here in Russia the womens are very different as in your country also, of course. Most of them work, many, as a mans also, combine the work with a learning, then they learn in evenings schools, evening's sections of Universities, Institutes and so on. Womens have a equal chances in every walk of life with mans, but last years it is not so easy for womens to enter in the University, for instance because many of them, after the ending of the course of study don't work on their speciality because they married and the children and household take much time. So, it was long so that more medical students are womens and now the medical Institutes prefere therefore to accept the mans. But, in the same time, you can see in a big towns and in many, many places of our country that womens work in very different directions and have different professions. The sellers, the conductors in tramways and in buses, the workers in a factories and milles are, in the greater part, womens. I think, it can be said that sometimes the obligations of womens are more difficult because but her professional work, she have also the household and children. But I think it is good and sometimes I very wont to hear the children voices. And so it by you, is'nt it is?

It is very strange that though we - your family and I - have only a letter's aquiantance

it seems me that we are a very good friends and I think that sooner or later we will come across and till that I asked your husband to send me the photos of your all.

I shall be very glad if you will find sometime the time and write to me.

Perhaps you wont to have some russian books?

With all best wishes, Sincerely your's

A.A. Gurwitsch.

A.A. Gurwitsch
Inst. of Physiology
Ac. of Med. Science
Baltyiscy Poschod 13
Moscow D-57

15. 9. 59

My dear Dr. Bahadur

Excuse me that I answer on your's and Mrs. Ranganajaki letters only now, (to Mrs. Ranganajaki I write a separate letter).

Of course I'm very glad to receive your gift and shall wait it impatiently. Let us see how will go my teaching of Hindi? I think that perhaps my interest to your country will help me. I often hear a radio from India and think often about the events which take place now by you. All my sympathy is on the side of your country.

Soon our laboratory-room will be a little enlarged. Now we have one room and many apparatus are under the tables, and soon we shall have two rooms! And I think that then the experiments with radicals will go more fast.

I understand completely the importance of that things. Now we begin again (after our vacations) to work with counter Geiger. It seems me that it will go. I think that in a 2-3 months we shall have two parallel counters and a good shortfocused quartz lens and the effects must be higher. Perhaps it will be also useful for you.

You write that you intend to send me some snaps of waterfalls, it will be very good and it would be also very well if you send me some time the photo's of your family, especially of little Chandram who don't wont to go to school. I understand him very well because I remember how I cried when I was 5 year old and also don't wont to go in children garden. We lived then in Leningrad.

With my best wishes, Sincerely your's A.A. Gurwitsch

A. A. Gurwitsch
Inst. of Physiol.
Ac. of med. Science
Baltyiskyi Porelok 13
Moscow D-57

13. II. 58

My dear Dr. Bahadur

I was very glad receiving your and Mrs. Ranganajaka letters. It was very interesting for me to hear more concrete about the variety of languages in India. I think that Mrs Ranganajaka will write me more and more about her successes in russian.

Of course your last results with papain are not only very interesting for me but also enjoyed me very. I also think that they show the formation of enzymoids on the principle of autocatalysis. Write me please about yours further results.

I don't enough understand about the x-diffraction photo's. Are you supposed the negatives which I have received from you half a year, or so, ago? I can't to my pity, have an opinion about them because I don't know this method. May be you will write me more detailed about that?

You ask me about your paper for prof. Oparin. I send it to the secretary of the Symposium immediately after your letter in Summer 1957.

Of course I think that you are free and can to send it in another journal; it is quite clear for me.

But, as you ask my opinion, I must write, that, from ~~it will not be~~ my point of view, you know it, it will better if the ~~using~~ of nitrogenetic radiation for the ideas about the origin of life. ~~will not be.~~

Is it coincided with your present point of view, or is not?

I wait the eleven number of Iswestia of Ac. de Science, where your paper must be published and I shall immediately write you.

With my kindest regards to Mrs. Ranganajaka, little-ones and you.

Sincerly yours A. A. Gurwitsch

25. 11. 59.

My dear Mrs. Ranganayaki

Thank you very much for your so kind letter. It seems me that after it I know you better. Of course, we are friends. I understand very well your trepidations before the trip to England. And I'm sure that if I shall be going to go there I should be excited no less. You know, I did not be also in abroad and I don't know if it will be possible later, so I'm a little envy to you.

Your intention to keep the eyes and ears open pleased me very much also as your feeling of pride for your country, which, as more as I read about it and about your pupils and leaders, become for me nearer and nearer.

I think, I'm sure, that your husband and you will find in England only a good treatment and will receive many friends. It seems me so impossible and monster that the question of different skin can hinder that.

I want you to say already now that I shall wait from you the short descriptions of yours impressions from the first days of your voyage. I shall be also very glad if you will answer me.

With my best wishes to you and both littles-ones,

Sincerely yours Anna Gurnitsch.

A.A. Gurwitsch
Inst. of Physiol.
Baltyjsky Poselok 13
Moscow D-57

25. II. 59

My dear Dr. Bahadur

I was very glad receiving your letter. Really it is wonderfull that you will go in England, and all together. This Fellowship shows that your investigations call a great interest and I think that a development of your work will give many new interesting results. Who is Dr. Jacob, did he work with enzymes, especially with urease? I think that you will receive there also the possibility to have a large exchange of opinions and in general such a form of scientific contact which is so usefull and so necessary for all of us.

My different experiments go not fast, but I hope that nearer to spring I shall have more results on the question which interest us both. Besides that I want also very much to put the detection of the radiation by physical method. I think, and that is, as you know, very disagreable for me, that the large scientific opinion will be reconciliate with Mitogenetic Radiation only after the possibility to work with physicall apparatus. Thank you very much for your snaps. I imagine very clear how beautifull and interesting is the landscape near Allahabad and it was very agreable for me to see you and your friends if only on the photo.

I send you back your letter to prof. Löhnis which probably you send me by mistake.

With my best wishes, sincerely yours
A.A. Gurwitsch.