Twenty Years of Mitogenetic Radiation: Emergence, Development, And Perspectives

A translation of the great Russian biologists' 1943 review of the discovery and development of mitogenetic radiation.

Courtesy of V. Voeikov

by Alexander G. Gurwitsch
and Lydia D. Gurwitsch

Introduction

Twenty years' existence of a new discipline gives a substantial basis for critical review of its successes and failures. This is particularly true for mitogenesis, because such a review may throw light on a very peculiar, and, in our opinion, unprecedented chapter in the history of science.

The fate of mitogenesis is indeed very peculiar: A review by several authors, published in commemoration of the decennium of the discovery of mitogenetic radiation, appeared at the culmination point of the apparent success and acknowledgement of the new discipline.[1] Since then, the opposite trend has become more and

EDITOR'S NOTE

This 1943 article was translated from Russian by Dr. Vladimir Voeikov and Dr. Lev Beloussov, a grandson of the Gurwitsches. It first appeared in English as an appendix to the proceedings of the International A.G. Gurwitsch Conference, Sept. 28-Oct. 2, 1994, held in Moscow, titled Biophotonics: Non-Equilibrium and Coherent Systems in Biology, Biophysics, Biotechnology *(Moscow: BioInform, 1995). It was originally published in the Russian journal* Uspekhi Sovremennoi Biologii *(Advances in Contemporary Biology), 1943, Vol. 16, No. 3, pages 305-334. This translation has been edited in collaboration with Dr. Voeikov.*

A report on the second International A.G. Gurwitsch Conference, held in Moscow in September 1999, will appear in the next issue of 21st Century.

Alexander and Lydia Gurwitsch, from a 1940s photograph.

Editors' Notes from the 1995 Publication in *Biophotonics*

The editors of this volume found it appropriate to present, along with the conference papers, this review by Alexander Gurwitsch and his spouse and devoted assistant, Lydia Gurwitsch. It was written in the full swing of World War II, soon after the authors escaped from besieged Leningrad, where all the laboratory documents had to be left. Therefore, the bibliography is incomplete.

This review is of great scientific value, and still relevant. It is an exciting human document of a "little scientific tragedy," as Alexander Gurwitsch modestly noted in one of his papers. It is written with his whole heart. Gurwitsch demonstrates here self-criticism, pointing to his previous mistakes and delusions—a feature not frequently met among scientists. However, as regards the essence of his main discoveries, Gurwitsch strictly insisted on their objectiveness. He was disappointed by an inadequate and preconceived attitude of the majority of the scientific community.

Obviously, a scientist is free to accept or reject a certain concept based on the direct experimental facts, but the history of the problem, including the history of the emergence and development of theoretical ideas also means a lot. It seems that any reader, either completely unfamiliar with the problem, or even prejudiced against it, will be impressed by the sincerity of the authors' tone and by such an immense amount of a highly qualified and promising work made in this field within the first 20 years. Probably, he will become more open to the perspectives in this scientific field that are arising today.

The editors omitted about 10 percent of the original text; they hope to have succeeded in retaining and adequately reproducing the content and the style of the authors. Editors' notes appear in square brackets.

more evident: Interest and confidence in the new discipline have been gradually fading. Things have come to such a point that now the publication of the next, even modest collection of papers seems to be impossible. And the gravity of the current moment [the authors are referring to the wartime conditions of 1943] cannot be the only explanation for such a situation.

Meanwhile, we, being devoted to mitogenesis, are convinced that the real development of this discipline during the second decade of its existence was very fruitful. Its achievements have even exceeded the expectations of the broadest scientific circles. How could it happen that—under complete isolation and while seeming to fade on a worldwide scale—mitogenesis was undergoing a rapid and successful self-development? This question undoubtedly sounds like a challenge for anyone who seeks to reveal regularities in the general roots of evolution of scientific thought.

We are attempting to review here the overall relations between our laboratory and the scientific community. However, we would like to start from a general formulation of the reasons which led to the present situation.

The main reason for the hostility or, at least, the skepticism of scientific circles towards mitogenesis has been the absence of any connection of the discovered phenomenon to the already known facts, or rather, to their interpretation within the limits of the conventional concepts. Such a discovery can function psychologically like a bomb blast. One can hardly find in the history of biology similar cases of the unwillingness of scientific circles to accept the new fact. The second reason for skepticism, and the neglect that manifested itself later, but became dominating, was the non-classical character of the phenomenon discovered by mitogenetic methods. This conclusion can be illustrated by the words of the well-known physiologist Hill: "The new era would come for neural physiology, if the claims of the Russian authors were correct. . . ."

From the psychological point of view, such an attitude is easily understandable, although it is quite unworthy of science. Unfortunately, most of the researchers educated within the frames of a given set of theoretical concepts are unwilling to re-evaluate these habitual concepts, because giving them up is always an unpleasant task. These two motifs are, so to speak, natural. Conservatism is inherent in science and unavoidable.

However, along with the above-mentioned reasons, the attitude to mitogenesis was fatally influenced by some other circumstances, seemingly attendant and occasional. We mean the attempts to test our main assertions in other laboratories, both in our country and abroad. With the remarkable exception of the studies of a few authors who really contributed to the new discipline (among them we may mention Magrou and Magrou, Ziebert, Blacher, Wolf, and Zirpolo), all the other numerous tests—with either positive or negative conclusions—led the authors to express doubt. Some of them expressed severe criticism.

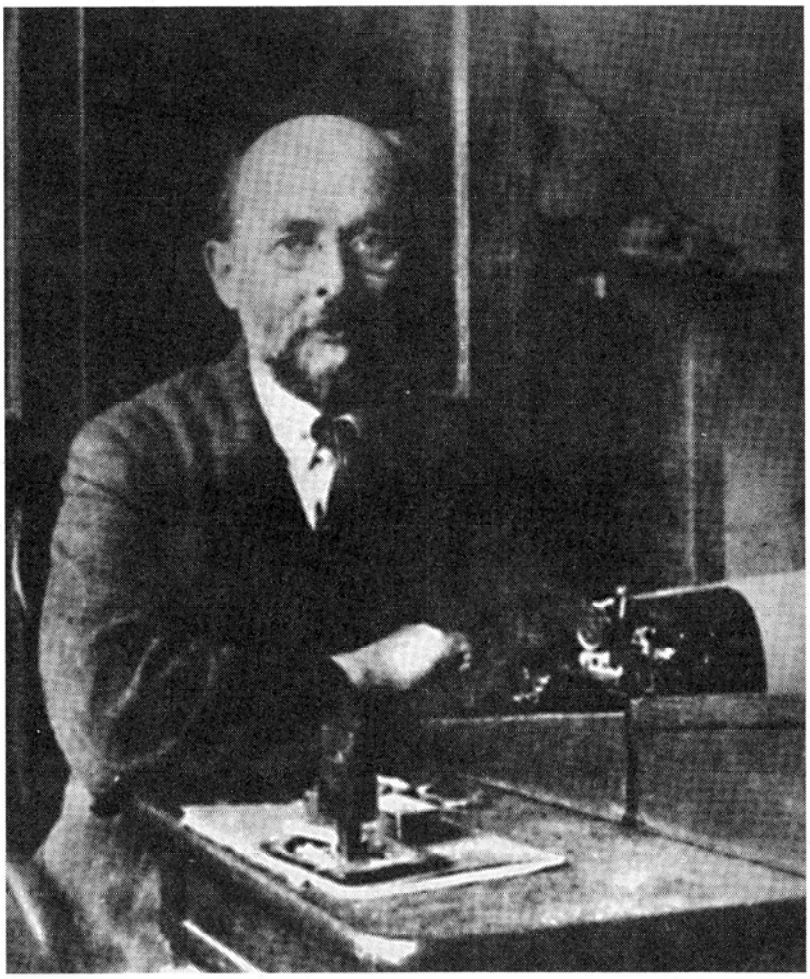
The latter came primarily from those authors whose aim is to test the existence of the phenomenon, but who do not precisely follow our recommendations with respect to methods and, in some cases, even deliberately violate them.[2]

Another reason for distrust of mitogenesis seems to be rather strange and forces us to suspect a certain unfairness of some representatives of the scientific community. Numerous reviewers from various countries, who never sought to carry out their own experiments and were often absolutely ignorant of the literature on the problem, are claiming that from year to

1. A.G. and L.D. Gurwitsch, 1934. *Mitogenetic Radiation.* Leningrad: The All-Union Institute of Experimental Medicine Publishing House. In Russian.

2. What we mean here is that our recommendation and restrictions are based on a firm empirical foundation. For example, our recommendation that the incubation period of a yeast culture should not exceed 2 hours, because the mitogenetic effects can not be detected after a longer period, was not taken into consideration by Schreiber and Nakaidzumi (1932). They tried to observe the effects after a 4-hour incubation period and certainly failed. In spite of the substantiated recommendation by Baron to use diluted cultures (no more than 200,000 cells/cm^2), Bateman and Kruechen (1934) used cultures 10 to 15 times more dense.

year the number of the negative results is increasing, while the number of corroborations of mitogenesis is decreasing. The discrepancy between such statements and the reality is so shocking that similar claims, if made on the matters of everyday life, rather than on scientific questions, would not go without punishment. Suffice it to say that, according to the last and obviously most complete review by Maxia (1940), several hundreds of confirming results coming from different countries could be opposed by barely a couple of dozen reports of negative results.



From archives of L. Beloussov

Alexander Gavrilovich Gurwitsch (1874-1954)

We abstain from elucidating logical and psychological grounds for such an unhealthy atmosphere around mitogenesis. In any case, the reference to the inherent conservatism of scientific thought would be wrong here. But it is important to say that we have never met in the scientific literature any motivated arguments revealing errors in our central statements or pointing to their physical unattainability. However, as shown below in more detail, our long labor has forced us to conclude that many of our initial suggestions and interpretations appear to be completely wrong. Therefore, it is not surprising that our theoretical interpretations of the experimental results have been changing during these 20 years. But we may declare with full confidence, that in all cases of recent tests of our initial experiments (even by the newcomers in this field), the results have always been confirmed.

Emergence of Mitogenesis And the General Trend of Reasoning

Many cases are known in the history of science in which erroneous assumptions have led to valuable results and even to discoveries. However, it is difficult to find any analogy to the emergence and development of mitogenesis. A long chain of theoretical considerations and conclusions, which finally led to the discovery of mitogenetic radiation, turned out to be a particular combination of successful and correct ideas, on the one hand, and quite erroneous speculations, on the other.

It would be aimless and tiresome to follow step by step our entire course of reasoning. We shall limit ourselves to the main stage of the path taken. However, we shall strictly preserve the reality, without reformulating the initial, immature or poorly substantiated reasoning according to our current logical consideration of the problem.

An inexhaustible interest in the miraculous phenomenon of karyokinesis was the starting point of the whole story. Having in mind purely cytological aims (shifting yolk platelets out of the animal parts of triton eggs), we employed an intense centrifugation of eggs. We were amazed by the resulting, chaotic picture of cleavage that disobeyed all rules and regularities and looked like an "occasional" event. Was it really so?

By a strange coincidence, just at that time, we became acquainted with a book on biometrics, containing an elementary account of probability theory. Using the elementary notions of normal, supernormal, and subnormal distribution, we could easily demonstrate on a number of subjects (and on onion roots in particular) that spatial distribution of mitoses obeyed purely random distribution. This result brought about the following chain of reasoning that turned out to be crucial for the further discovery of mitogenetic radiation.

Cell division is an "occasional" episode in a cell's life. Following the division of a parent cell, two sister cells may have quite different fates, even under completely identical life conditions. One of them may remain intact, while the other one may divide. From this, the following conclusion could be derived: An occasional event is the result of at least two mutually independent factors. To create a theory of the emergence of mitoses, we had to start with this dualistic concept. We know that such a formally correct conclusion from a largely unconvincing and biologically improbable statement led to the discovery of mitogenetic radiation. Only much later did it become clear that there are alternative explanations of the statistical distribution of mitoses, that are almost identical to the "occasional" one. It might be the result of a long succession of cell divisions, each having certain fluctuations of interkinesis duration. So it was biologically wrong to suggest the existence side-by-side of sterile and repeatedly dividing cells. However, if the initial idea was correct, that at least two independent factors were necessary for a cell division to occur, it would be

logical to assume that one of the factors is endogenous, that is, it coincides with the whole complex of the internal "ripening" processes (a factor of possibility), while another is exogenous, even if it originates in the same organism to which a dividing cell belongs (a factor of realization). Such a distinction of the two factors seems to be merely a theoretical construction; nevertheless, it found substantial support and had been introduced into science.

We point therefore to an example of a correct conclusion from the incorrect premise used in our argumentation. We shall see below that such a peculiar mixture of successful and erroneous conclusions occurs rather often.

After we had found experimentally that the impulses for cell divisions come from onion basement membranes, and after a long series of measurements and calculations, a very simple linear dependence between the surface area of meristem cells and the probability of cell division was revealed. The new, quite arbitrary and thoroughly inconceivable conclusions were derived from these facts. In spite of the physical naiveté of this reasoning, it provided the impulse for decisive experiments, instead of leading us down a blind alley.

Because, in those days, we were utterly seized by an unfortunate, preconceived notion of the complete "fortuity" of mitoses, and rejected any possibility of regular cycles of cell division, we suggested the following, quite artificial construction.[3]

Earlier we had demonstrated that the meristem cells grow exponentially. From this fact we drew the correct conclusion that cell surface growth has a strictly assimilative character. This conclusion is identical to an assumption that there exist two kinds of substances at the cell surface. The quantity of the first one (K) does not change with growth, and K occupies the same surface area during the whole growth period. The area occupied by the second component of the surface (A) is constantly increasing, because A is growing in an assimilative way. This suggestion would imply that during cell growth, the cell surface retains all of its properties. Thus, surfaces of small or large cells should differ only in continuous changing of the ratio of the variable parameter A to the constant parameter K. Now, what is the explanation of the simple dependence between the A/K ratio and the probability of mitosis?

Assuming that we are dealing only with an action of external factors upon the cell surface, we suggested first that those factors are similar to Haberlandt's hormones. In this case, the only plausible notion would be the following: The cell surface can be considered as a kind of mosaic in which K is a dispersed, and A is a continuous, component. It is clear that as A increases, the dispersion of K will increase as well. Hence, if K is permeable to a hormone but A is impermeable, the permeability of the cell surface to the hormone will gradually decrease. Under these circumstances, however, the relation between A and the division probability will be expressed by a fraction in which the A-variable is the denominator—that is, by a hyperbolic function. However, this dependence is in fact linear.

This discrepancy impelled us to reject the idea that division was induced by a hormone, and we moved further in the direction of risky and far-reaching speculation. The linear dependence can be expressed by a formula:

$$P = aK - A,$$

where P is the probability of a mitosis, and *a* is a coefficient.

It may follow from such a dependence that K is favorable but A is unfavorable for the action of an external factor. As A increases, more regions occupied by K are inactivated. but a certain residue of K retains its properties. One may suggest that K creates a regular mosaic which is gradually destroyed by continuously growing A. The fragments of the mosaic would lose their function under these conditions.

These considerations were followed by a very risky leap of thought. If the configuration of the K mosaic plays a decisive role, one may suggest that the perception of an impulse by the cell surface is based upon something which can be defined as a resonance. This suggestion leads to the following: The "factor of realization" which determines cell division is of an oscillatory nature, that is, it may have something to do with a radiation process.

It is difficult to understand now, how such a chain of arbitrary and physically rather naive reasoning could have led us to the valid result—the discovery of radiation. However, the first suggestion, that the cell surface is a decisive factor for perceiving mitogenetic radiation, seems to be true. This was quite clear at the very beginning, when the nature of the division factor was completely unknown. Such a conclusion could be directly deduced from the synchronism of cell divisions in various syncytia and polynuclear cells, as opposed to the high degree of randomness in the distribution of mitoses in most cell populations. On the other hand, the very idea of a resonance-like principle also contained a grain of truth, which could not be completely realized at that time.

Therefore, as now appears quite obvious, a reaction of the cell to one or several photons is possible only if the photon absorption triggers a chain reaction largely dependent upon the spatial arrangement of the molecules involved, that is, in general terms, upon the supramolecular order.

We shall now resume the review of our ideas, delusions, and researches that led us to the discovery of mitogenetic radiation. It is trivial to claim that knowledge is gained only after many errors and delusions. What may be instructive in our case is that blunders frequently intervened in the chain of our deductions, sometimes in its most crucial links. This happened repeatedly after the discovery of the phenomenon and in the course of its further investigation.

The First Experimental Results

After we had offered a risky suggestion that some form of radiant energy constitutes an exogenous factor, we ran into a number of problems and found ourselves in a rather miserable situation. Because the visible and infrared parts of the spectrum could be rejected, only two possibilities remained: either ultraviolet or some new, unknown kind of radiation. It was natural that we tried to investigate the first case.

As followed from our previous experiments, only an onion

3. It is clear now that the most simple and biologically plausible explanation of the linear dependence of division probability on cell surface area is that the duration of interkinesis increases in parallel with cell growth.

basement membrane could be a source of radiation. It seemed reasonable to suggest, in addition, that rays could travel along the whole axis of the root (not less than 10 to 12 cm long) towards the meristem. The latter could be reached only by a beam of parallel rays.

Strange as it may seem, the chain of our initial reasoning ended here, and we did not immediately reach the natural conclusion that at least part of this beam could be emitted from the root. Such a simple idea came, as a kind of a revelation, only a few weeks later, while I was walking; this may be qualified as one of an inconceivable number of cases of inconsistency and a lack of logical of thinking.

What came directly from the initial hypothesis was a rather doubtful idea to trace the spreading of rays in a bent system, in the hope that in this case some regions of the meristem zone would be "illuminated," while others would be "shadowed." A large series of experiments gave the expected results, that is, those calculated as a function of a root distortion. But we regret that we published them. Both the constructions and the assumptions are too complicated, and the results could be explained on the basis of quite different hypotheses. Experiments with frog cornea wounding performed at the same time had similar drawbacks. In the latter case, one extensive rounded wound was made by a heated needle and another wound, having a linear shape, was made at some distance from the first one. We suggested that the impulse to mitoses coming from the first wound is able to spread throughout the whole cornea, and that it will be at least partly screened by the linear wound. These experiments also gave some results which could be interpreted as positive. It is interesting to note that these doubtful considerations and results were accepted with wider sympathy (for example, by Wasserman) than the later data which were reliable and unambiguous.

Courtesy of Vladimir Voeikov

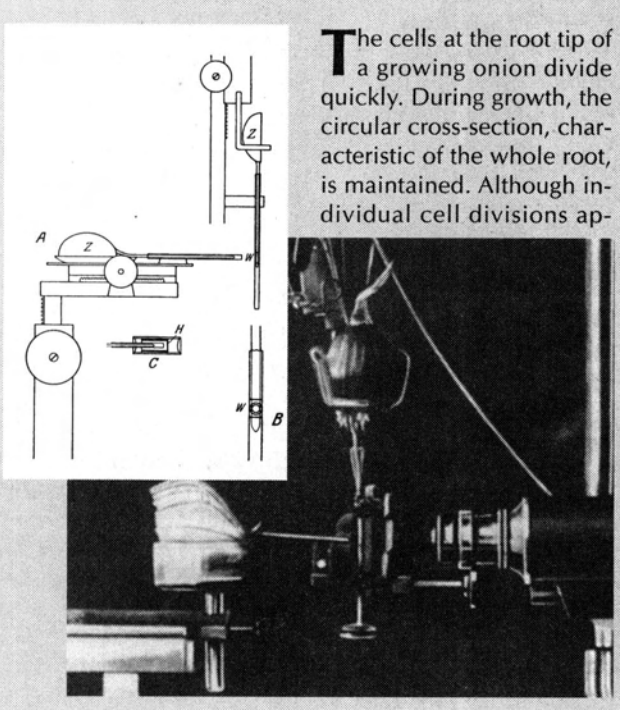
Some of the participants at the First International Alexander Gurwitsch Conference on Non-equilibrium and Coherent Systems in Biology, Biophysics, and Biotechnology, held at M.V. Lomonosov Moscow State University in Moscow, September 1994. In the first row are Prof. F.-A. Popp (third from left) and Prof. Lev Beloussov (fourth from left), co-chairmen of the conference. In the background is a statue of M. Lomonosov and the main building of the university, which he founded.

The Main Experiment

Even before we reached, at last, the conclusion that if the main hypothesis were correct, emission should come from the root's tip, we started to look for an appropriate detector. And the first successful idea, after so many unsuccessful ones, finally came. Only a set of cells which is capable of dividing normally but, at the same time, is sensitive to the action of an exogenous "realization factor" increasing the division probability, could be used as a detector. The result of such an action—that is, according to our hypothesis, irradiation—could be detected only in a comparison with a control set of identical cells not affected by the same factor. The well-known onion root could serve again as the most suitable subject for such a study.

Straight roots are completely symmetrical. Thus, they can be divided into two equal parts by any medial plane. The

Gurwitsch's Famous 'Onion Experiment'



The cells at the root tip of a growing onion divide quickly. During growth, the circular cross-section, characteristic of the whole root, is maintained. Although individual cell divisions appear to occur in an unordered, even random distribution, the number of divisions in all directions from the axis must nevertheless be approximately equal. The root would otherwise not have a cylindrical form.

Gurwitsch supposed that at least some of the cells must be emitting light that regulated the rate of division of the other cells; he proved it by means of the experimental setup shown here. The roots (W) of two onions (Z) were positioned perpendicularly, so that the tip of one root pointed to one side of the other root. He then examined under the microscope the second root, at the site facing the tip of the first root. He was able to establish a statistically significant increase in cell divisions there, compared to the opposite, "unirradiated" side. This effect disappeared when he placed a thin piece of window glass between the two roots, and reappeared when he replaced it with quartz glass! That meant that ordinary glass is opaque for mitogenetic radiation, while quartz glass is translucent. Hence electromagnetic radiation must be operative, and ultraviolet light in particular, since it passes through quartz, but is stopped by window glass.

Source: A.G. Gurwitsch, *Das Problem der Zellteilung* (The Problem of Cell Division), 1926

number of mitoses in both parts is almost equal. Although in different roots the total number of mitoses is in the range of 1,000 to 2,000, the difference between the two halves of the dicotyledonous plant is no more than 3+ to 5 percent. Even in two halves of the same section, the difference is only a bit greater. Therefore, the difference between an irradiated and a "shadowed" half caused by a unilateral, strictly localized irradiation should be quite distinct.

The first experiments immediately gave brilliant, decisive results. Further experiments confirmed the initial data. This requires discussion in more detail because the persuasiveness of even a single experiment appears to be much greater than one might initially suggest.

As radiation is detected from a root tip at a distance of several millimeters from the surface of a detector root, the rays, for purely physical reasons, should be directed at the normal. Thus, they should act only on that part of the detector surface that intersects the extension of the axis of the radiating root. Therefore, if the detector root is dissected into a series of longitudinal sections coinciding with this direction, the effect—that is, the excess of mitoses on one side compared to the other—should be expected to take place only within the medial and paramedial sections, and in any case should decrease on both sides of the medial plane.

At that time, we already knew that there are fluctuations in the number of mitoses, equally probable for both halves of the section. If the probability of an excess within one section is 0.5 , the probability for x successive sections should be $0.5\ e^x$. That means that if $x = 5$, this probability should be as small as 1/64. The following also confirms this reasoning: The probability of successive unilateral excesses of the number of mitoses in the absence of irradiation would be the same for both exactly medial and much more laterally prolonged sections. When, under medial irradiation, the excess is detectable just within the medial, rather than the lateral regions, the probability that the observed phenomenon will be of a purely occasional nature becomes very low.

Finally, the last and most convincing argument is that the excess can be observed within several medial sections of the induced side; this amount was several times greater than the level of statistical fluctuations in other sections. We see therefore that the result of even one experiment can be convincing.

After it was observed that the effect is not inhibited by inserting between the roots a thin glass plate (several dozen micrometers thick), but the effect disappears when a cover-glass was used, all doubts about the radiant character of the impulse were gone. It became clear that we were probably dealing with ultraviolet irradiation. Strict evidence of this was obtained much later.

Critical analysis of our interpretation of this undoubtedly real phenomenon should reveal the following point, which escaped us at that time, and escaped our critics, who doubted the experimental fact itself. If the assumptions that the irradiation is coming from an onion basement membrane and that we are dealing with ultraviolet are correct, it seems impossible that radiation emerging from the onion basement membrane could pass a distance of 10 to 15 cm,

through a meristem layer, down to a root tip in a medium rich in proteins.

We realized this problem much later, when we had already observed the phenomenon of "secondary" radiation under the influence of irradiation. The idea of secondary radiation came to us when we recognized that the effect of irradiation, that is, the excess in the number of the mitoses on the irradiated side, is spread along the meristem up to 1.5 to 2 mm from the area that was briefly irradiated with a point source. The diameter of this area did not exceed fractions of a millimeter. If an irradiated cell transmits the same impulse further, by way of secondary radiation, it is obvious that the photons reaching the meristem are generated not in the onion basement membrane but in the vicinity of this very cell which started to divide after absorbing the photon.

In a short time after our first studies had been published, various laboratories reported their own results, both positive and negative. Very soon the regrettable situation already mentioned in the introduction became obvious. With the exception of a quite reliable investigation of Magrou and Magrou (1927), which was a corroboration of the discovered phenomenon, both the positive results by Wagner (1927) and the refutations by Rossman (1928) and Moiseewa (1931), as well as many later studies, were of no significance.

We shall discuss these papers from the general point of view. Our opponents, of which Rossman is an example, usually claim that in several experiments that they performed, the radiation effect could not be reproduced. They opposed their results to at least several dozens and, later, more than 130 sets of experimental data which we have published. However, we pointed out that we published all of our results, which means that there was no lack of the effects observed. In spite of that, our opponents insisted that the described phenomenon does not exist at all. We consider it reasonable to ask the following direct question: How can the authors explain our numerous positive results—by a systematic error made by all of the authors, or by their dishonesty? This question has hardly any answer worthy of the attention of the scientific community. We consider that negative results may be used as a refutation of the positive ones only in some exceptional cases.

On the other hand, it is not difficult to explain the sources of the negative results. Usually, they came from methodological errors. For example, Rossman used roots of *Leguminosae* as a detector. This dicotyledonous plant obviously has a certain bilateral symmetry, even in a hidden form (instead of the required radial symmetry). He centered the roots with the naked eye, instead of using the horizontal microscope, and so on.

We pay such attention to these incidents because of their general importance, because they are equally applicable to further "refutations" of our data obtained with the use of biological detectors and also to some investigations in which physical methods have been used. At this point we finish what can hardly be qualified as "scientific polemics." We had to mention these sorrowful facts in our retrospective review only because we were often saddled with the reproach that we ignore weighty objections.

Corroboration of the Ultraviolet Nature Of Mitogenetic Radiation

The hypothesis that mitogenetic radiation belongs to the ultraviolet range was experimentally corroborated rather soon. First, it was found that a crystalline quartz plate is completely transparent to radiation, while even the thinnest gelatin plate is nontransparent. Second, Gleb Frank made the first spectral analysis of radiation from a biological source, which was muscle tissue. Finally, it was established, in collaboration with Frank, that the positive effect upon roots can be obtained from the spectrally dispersed UV from physical sources, if the intensity of emission is considerably reduced and the time of exposure is very short. Incidentally, the latter finding disproved an established view that the biological action of UV can only be inhibitory. It became evident to us (but, unfortunately, not to biologists at large) that the mitogenetic phenomena imply very special microevents, which can be neither corroborated nor disproved by employing UV of commonly used intensities and doses.

The second way to prove the identity of mitogenetic radiation with UV is to use purely physical methods which overstep the limits of our competence. Therefore, they will be mentioned only briefly here. Rajevsky's data, published previously, were not quite convincing from the standpoint of quantitative criteria. Later, he obtained additional quantitative data and thus removed any doubts.

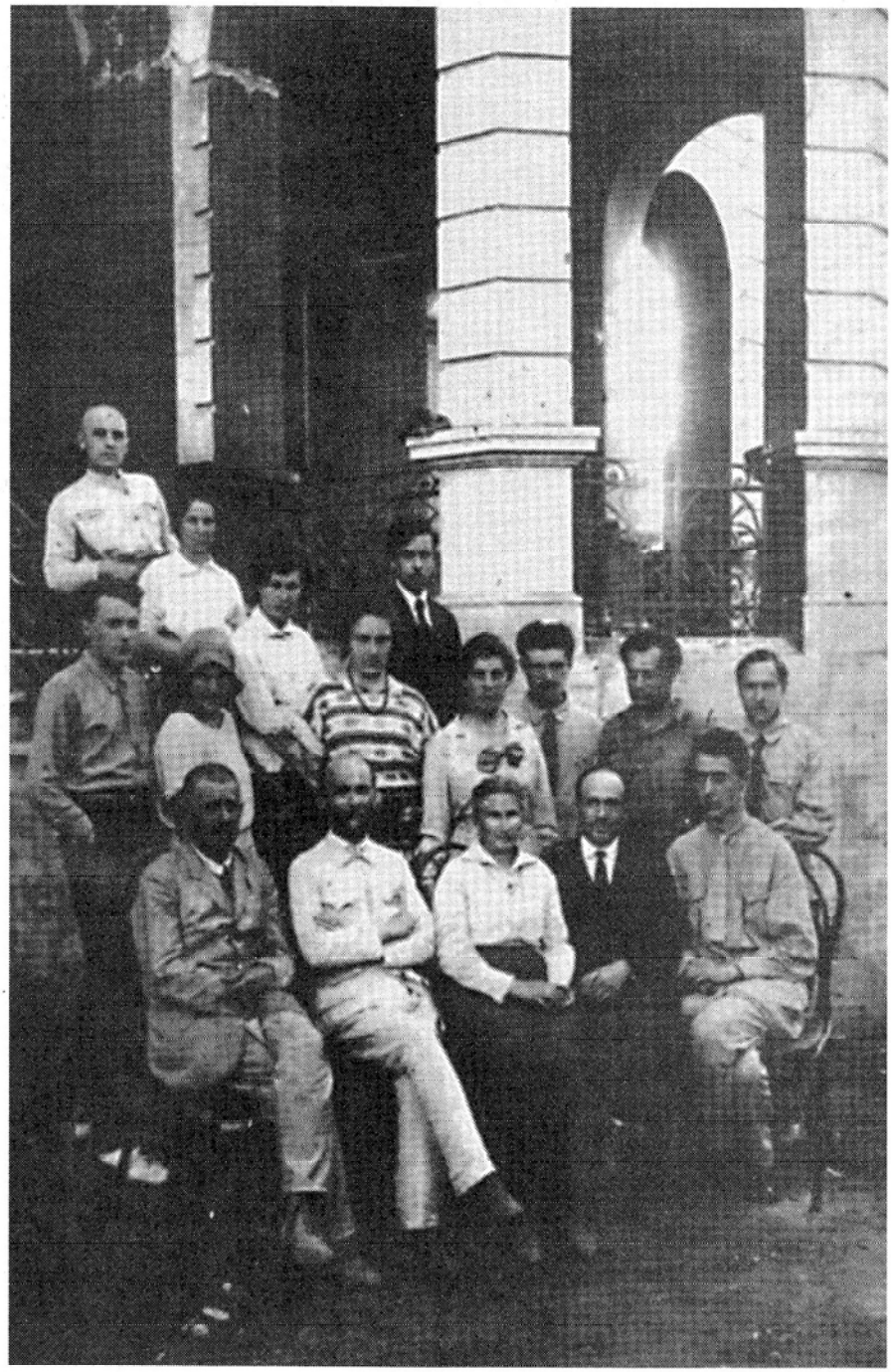
Numerous attempts were made to use physical methods to detect mitogenetic radiation that were capable of reproducing the data obtained with biological detectors. Some of the favorable studies, however, could not be considered technically perfect. As well, several reliable, positive studies (for example, Frank and Rodionow 1932; Barth 1937) were criticized by authors who could not detect radiation. And again it was convincingly demonstrated (Barth 1937) that the negative results may appear because of the deficiency of the devices, but mostly because the authors neglected our recommendations and used unsuitable sources of radiation.

However, a substantial study by Krost and Peuchert, and the extensive, and largely acknowledged studies by Audubert, completely clarified the situation. Mean-spirited attempts of the latter author to conceal the truth and neglect the relationship of his data to our own are a manifestation of the grievous symptoms of modern scientific ethics, but they cannot change the essence of the matter.[4]

Further Development of Mitogenesis

The above-mentioned experimental findings were obtained while still in Simferopol [up to 1924] and during the first period of our work in Moscow [1924-1929]. Later, particularly because of the introduction of a new detector—

4. It is interesting that in some reviews of the discovery of mitogenetic radiation, even in favorable ones such as Huxley's, it is stated that I (A.G.) initially claimed to have discovered some specific "rays of life," and this notion is still widely used in low-pitched popular literature. It is scarcely necessary to say that this is pure fantasy on the part of the authors. A reasonable initial cautiousness in our first report, with some doubts as to whether the new phenomenon could be related to a poorly studied range of Lyman wavelengths (shortwave UV), was probably not adequately estimated. But one should be astonished that it is possible to ascribe such a childish idea as "rays of life" to me.



From archives of L. Beloussov

The Gurwitsch laboratory at Simferopol (1923-1924). Gurwitsch is first row, second from left; his wife is third from left.

yeast cultures (M.A. Baron)—the scope of our work increased tremendously. At the same time, the main lines of investigation began to branch off rather chaotically. Such a situation seriously complicates the main task of this review, namely, to attempt to present a systematic account of the development of the general idea. The rest of this account, therefore, will have a rather fragmentary character. Moreover, we have lost the opportunity to use our notebooks [those left behind in besieged Leningrad] and thus to recover the exact sequence of our thoughts and the motivations of the new trends in our work. However, we can say that we were guided by the following major considerations.

First of all, it was but natural to see whether mitogenetic radiation is a general biological phenomenon, and we still remember our excitement when we succeeded, while still in Simferopol, in detecting emission from certain animal tissues (amphibian tadpoles). But later, when it turned out that all the tissues were emission sources, the problem certainly became much more complicated and required new, extensive studies.

We would like to mention one incident which put forward the problem of emission from blood. When studying photon emission from early chicken embryos cultivated in physiological solutions outside the egg, we ascribed the initial negative results to the lack of blood circulation. We came to the idea of studying blood as a possible general source of radiation. However, it was established later that the negative results appeared to be caused by the presence of Bunsen burners for heating embryos in the near vicinity of the sample. The burners were found to emit much more intense UV than mitogenetic radiation.

The addition of blood to the range of experimental materials initiated a new trend in our studies that persisted for a long time. What was done later in this field was mainly the accumulation of occasional facts and the results of scientific curiosity, rather than of a well-defined and substantiated plan.

This applies mostly to the studies of blood radiation under the influence of various diseases. Many experiments of this kind had already been performed by L.D. Gurwitsch at the very beginning of the blood studies. In any case, one of the most important chapters in mitogenesis—the study of the "cancer extinguisher"—did not at all emerge as a link in a logically developing chain of events.

An extensive study of various plant and animal subjects yielded results that could not be adequately interpreted in the early period of our studies, and still cannot: Radiation could not be observed in all tissues. For example, it was not detected in parenchymous tissues characterized by intense metabolism (liver and kidney). So long as this fact remains enigmatic, one cannot interpret the mechanisms of radiation arising in living tissues. Up to now, therefore, there are no satisfactory solutions to the questions related to the universality of radiation and the conditions of its emergence in living systems.

Moreover, the main problem of mitogenesis, namely, the analysis of the basic mitogenetic phenomenon—of stimulation of cell division by UV photons—was for a long time con-

sidered ambiguous. It is obvious now that consideration of mitogenesis using the concepts of the usual classical optics was wrong, because the latter can be applied only to high light intensities and completely ignores quanta of light. We tried to analyze the results of our experiments with intermittent irradiation, as well as creeping effects and the mechanism of a continuous increase of intensity, and so on, assuming the concept of continuity of light. For example, in experiments in which the detector was irradiated with intermittent light, this treatment was considered a strictly rhythmic process. However, it now became clear that, with the rotation frequencies and angular values of sectoral slits used in the experiments, some of the slits did not let even a single photon through. [The routine method involved the insertion of a rotating disk with one or several windows between the radiation source and the detector.] It is obvious that any attempts to analyze the process of mitosis induction with inadequate equipment were doomed to failure.

It was very important to realize that the process of stimulating mitosis with mitogenetic radiation is based on the chain reactions that could be triggered by a single photon. A study of the chain reactions became possible only because of the discovery of the secondary radiation, and because of the results of studying processes developing in nonorganized systems (homogeneous solutions) after their irradiation. A study of these phenomena appeared to be so complicated that we were diverted for a long time from the main task—the elucidation of mechanisms of the mitogenetic effect.

But there was another reason for our excessively slow movement toward the solution of the main problem. For many years we could not establish whether mitogenetic radiation was really a specific factor that triggers cell division. To prove its specificity, one should arrest cell proliferation completely by the action of a certain factor *X* and then restore it completely by irradiating the detector from the outside.

Such a crucial experiment was first performed by Zalkind. It yielded the results that permitted us to move forward. The study was made on yeast cultures in a liquid medium. By adding a negligible amount of the so-called "cancer extinguisher" from the blood of a cancer patient, a complete, temporary arrest of cell proliferation can be achieved. After irradiation of the culture from the outside, proliferation was renewed and brought to its initial level. It was also demonstrated that the addition of the extinguisher, besides suppressing proliferation, inhibits mitogenetic emission from the culture itself. The extinguisher did not disturb any other conditions necessary for cell division, except for self-irradiation of the culture. Thus, it was found that external irradiation completely substitutes for self-irradiation of the culture in the process of initiating mitogenesis. These experiments have proven the specificity of mitogenetic radiation as a factor inducing mitosis in cultured cells which are ready for this event.

Only later did we establish that not only the impulse for a premature division, but also the development of the mitotic process, is controlled by mitogenetic radiation. A series of investigations of photochemical processes opened the possibility for rational analysis of the crucial role of UV photons.

We turn now to the recent investigations that clarify the mitogenetic action of UV photons. Irradiation of peptone solutions or of a mixture of amino acids (which should include at least one dicarbonic amino acid, that is, glutamate or aspartate) induces their polycondensation into peptide molecules. This conclusion is based on the susceptibility of these peptides to cleavage by pepsin that was revealed by the detection of a typical mitogenetic "peptide" spectrum after the action of a pepsin. To initiate the process in an amino acid solution, the photon energy should exceed 105 kcal/mol. This energy may be supplied either by a single photon with a wavelength not exceeding 270 nm, or by two photons. The energy of the first should be not less than 87.4 kcal/mol (326 nm), while the second can belong to the visible or infrared range with an energy limit of 18 kcal/mol, that is, around 1,500 nm. Such sharply limited energy requirements may be explained as follows.

Amino acid polycondensation requires cleavage of one hydrogen atom from the amino group of one amino acid and of the hydroxyl residue from another. The first event requires 87 kcal/mol, while the second requires 66 kcal/mol. Total energy expenditure for both processes is completely compensated by two exothermic processes: the formation of a water molecule and a peptide bond, CO-NH. The additional 18 kcal/mol, which is necessary for the process, represents probably the energy of activation. We established that from 326 nm, up to the short wavelength limit of a quartz spectrograph, effectiveness of the UV radiation depends exclusively upon the degree of UV absorption by the peptone or amino acids, rather than upon the photon wavelength.

Polycondensation may also take place under other experimental conditions. After addition of a very dilute liver extract to a mixture of amino acids or to a peptone, polycondensation occurs without UV irradiation by bright visible light, but not in the dark. It was found that diluted liver extracts irradiated with visible monochromatic light, emit UV with a wavelength roughly corresponding to the energy of two photons of the monochromatic light. In this case, the process of polycondensation is triggered only when the energy of the resulting UV photon exceeds the value mentioned above.

A detailed investigation of the energetic parameters of the mitogenetic action of UV has shown that they coincide precisely with those required for polycondensation. All of the above-mentioned facts remove every doubt that the mechanism of mitogenetic action of UV photons consists in, and is limited by, the stimulation of the processes of peptide synthesis.

In relation to the main mitogenetic effect, the cancer problem first attracted our attention long ago. An extremely low level of mitogenetic radiation in cancer patients' blood, because of the presence of the extinguisher, led us to look for the place of its formation. The mitogenetic analysis provided a definite answer: The extinguisher is a product of cancer cells and is obviously located in the superficial monofilm covering the cell, together with the specific enzymes: glycolytic, proteolytic, phosphatases, and probably some others. Their specific feature is that they bear negative charge, while the corresponding blood enzymes are charged positively. Both the extinguisher and the enzymes can be obtained by washing intact cancer tumors. Using spectral

analysis we obtained evidence that enzymatic activities of proteases and peptidases in cancer cells are located mostly epicellularly. That means that their substrates are derived from nutrient medium as well as from tissue elements located in close vicinity to the cancer cell. On the other hand, the intercellular splitting of proteins in the cancer cell is obviously minimal.

Such a peculiar localization of peptidase activity can determine an extremely parasitic character in cancer cells. Along with some other discoveries, the following fundamental fact that reveals the origin of cancer was discovered: It appeared that a number of tested carcinogenic substances emit mitogenetic radiation, while some similar chemical substances, including noncarcinogenic resins, lack such a capability (Kannegiser).

Taking into consideration the peculiarity of the mitogenetic state of cells exposed to a source of radiation for a long time, as well as the versatility of the action of UV photons upon cells, we may suggest that UV photon emission by carcinogenic substances plays an important role in their action.

The problem of the basic mitogenetic effect was studied for many years with ups and downs. It was typical of all of the important problems that we had been studying during the preceding two decades. We would not be surprised, if someone said that our school gives the impression of having no principal idea, or that it uses the trial-and-error method in research work. One should never forget, however, that mitogenesis did not have any relationships with the allied sciences, and that all of our results had a "non-classical" character. That is, they did not fit within the usual cytological, chemical, or physical concepts. We therefore had to propose new concepts for approaching problems, which seemed, at first glance, to have no connections among them. Later it turned out that these problems are interrelated. Sometimes it becomes possible to make a step forward in solving a certain problem only with the help of new data obtained in the neighboring field of science, which depends, in its turn, upon some other, often remote, field. After successful achievements in one direction, further progress may be delayed or even blocked for a long time, until new steps in some other direction open the way.

The present status of mitogenesis is, to a considerable extent, determined by the method itself, which appeared to be really miraculous because of its sensitivity. Mitogenesis implies microprocesses in both the organized and non-organized systems. These microprocesses are "non-classical" in the sense that, on one hand, they do not allow easy extrapolation into the realm of macroscopic events and, on the other hand, they seem to proceed independently of the latter. However, they are usually shadowed by macroscopic processes that can be easily studied by the classical methods.

Each section of mitogenesis is supposed to be a narrow gateway into an immense new field. Let us analyze some of them.

One of the main achievements was the discovery of so-called "degradational irradiation," described in the following brief history. Studying the mitogenetic process of corneal epithelium, Yu. N. Ponomareva came across the fact that irradiation of the enucleated frog eye results in an increase of the number of mitoses only 20 to 25 minutes after enucleation. It was found, at the same time, that the enucleated frog eye did not itself emit during the whole refractory period, and that restoration of its own radiation occurred just at the time when its epithelial cells recovered their ability to react to external irradiation. At first, she [Ponomareva] could not find the explanation for this finding.

Investigations of the effect of cooling the cornea revealed a phenomenon that seemed to be quite paradoxical: a very strong mitogenetic effect was observed when a freshly enucleated cornea, being cooled down to 2° to 5°C, was subjected to external irradiation. Trying to explain such a strange finding—that cooling is a factor that favors the effect of the external irradiating source—we came to the conclusion that under the condition of rapid and intense cooling, the corneal tissues start to irradiate themselves, though this suggestion seemed to be rather unlikely.

This somewhat improbable suggestion was confirmed in the very first experiment: A burst of radiation under such a treatment lasted for about 5 minutes. This fact had innumerable consequences in our further investigations. Here again the work was of the same irregular and scattered character as in the other fields of mitogenesis. The research bifurcated immediately into two lines: First, much attention was directed to purely phenomenological aspects of the newly discovered phenomenon; its universality and the specificity of the emission spectra had nothing in common with the spectra of the same organs and tissues under physiological conditions. Second, attempts to provide a theoretical interpretation for these unexpected events were undertaken, and led us to some new, and again non-classical concepts, such as "non-equilibrium molecular constellations" (NMCs). The latter was, in fact. a logical jump, because it was based on intuition, which gave us a feeling that NMCs constitute the main apparatus for performing quite varied processes that can be considered the basic manifestations of life.

These findings were followed by a series of experiments which, at first glance, had nothing to do with the preceding ones, but whose results exceeded all of our initial expectations, nevertheless. It turned out that negligible deviations in metabolic processes and a wide variety of excitations of biological subjects induced degradational radiation differing in its spectral pattern and in the character of evolution of the spectra. The results convinced us that, first, an important and even a major reevaluation of what we mean by the term "protoplasm" should be made, and, second, new methods for classical physiology should be proposed for the purpose of revealing mostly intimate functional relations among various systems and organs, avoiding the usual techniques.

The spectral analysis opened really unlimited perspectives in quite different areas. Initially it was used by Frank only for demonstrating that mitogenetic rays really do belong to the UV range. But after we obtained—mainly to satisfy our curiosity—the first rather sharply outlined spectral band, the spectral analysis became an autonomous field of investigation. There was a great temptation to apply it in some other fields that otherwise would not attract our attention, for example, in nervous system research.

We did not expect that, by performing a spectral analysis of

nerve fiber emission, we would be able to demonstrate a qualitative variability of excitation processes. Nevertheless, the very first, timid experiment appeared to be the starting point for the development of one of the most fruitful and important areas of mitogenesis. As a result, we came to the following major conclusions: (1) the emitting substance of the nerve elements is at the same time the target of nerve excitation; (2) the analysis of mitogenetic phenomena makes it possible to penetrate into the essence of the molecular substratum of excitation processes.

In light of new data concerning degradational radiation, the following concept of neural excitation was formulated (A.A. Gurwitsch): An excited substratum consists of NMCs which form a three-dimensional "continuum" within the brain cortex. Even under physiological conditions, the emission of the elements of the neural system has the character of degradational radiation, that is, it arises as a result of continuous disintegration of NMCs just after their formation. According to these views, the substratum for neural excitation is a continuously oscillating system. Such a concept made it possible to create a rather coherent system interpreting the various properties of neural excitation.

We shall finish our review with a brief account of a large number of investigations with great prospects, and a very peculiar path of development.

Starting from our earlier data about the disappearance of blood radiation that correlated with various deviations from a normal physiological state, our collaborator S.N. Brainess suggested that grave physical exhaustion should also be accompanied by a temporary suppression of radiation. His investigations completely supported this idea. As a development of this idea, it was suggested that blood radiation should also be suppressed in the depressive psychical state which, according to Brainess, has some similarity to tiredness. On the other hand, he supposed that in maniacal states, the intensity of blood radiation should be higher than in the normal state. These speculations were completely supported experimentally.

Then, Brainess took a rather risky step. He attempted to cure patients in depressive physical states by injecting a small amount of blood from a maniacal individual. The result was positive.

The spectral analysis of "maniacal" blood radiation (the latter was not only intensive, but emitted mitogenetic rays for a long period after the blood was taken) revealed a single narrow spectral band in the 229 to 230 nm range. According to our previous data, this band corresponded to the fluorescence spectrum of amino groups liberated because of oxidative deamination of amino acids present in blood serum. Hence, "maniacal" blood is characterized by its increased capacity for deamination, while "depressive" blood, on the contrary, by a sharp decrease of this capacity.

It was of considerable theoretical interest that the therapeutic effect of a single injection of a small amount of "maniacal" blood into a depressive patient appeared to be rather prolonged. Suggesting that the therapeutic effect of the "maniacal" blood injections is directly linked with its increased irradiation rate, the author took the following step, although there was only a weak theoretical basis for it: A sample of citric blood from a depressive patient was irradiated with a mitogenetic source and injected back into the patient. This procedure had a good therapeutic effect. The next step was substitution of irradiation of blood for that of serum and then of irradiation of amino acid solution, which also demonstrated the healing effect after injection into patients. The latter procedure was based on the previously discovered remarkable fact: An irradiated amino acid solution itself became a source of mitogenetic rays acting for many hours—almost a whole day. It retained a therapeutic effect when injected at any time within this period. This led to a suggestion, that the injected active substance, which was analogous to the oxidative deaminase enzyme, was capable of self-reproduction with the organism. Such an idea—more intuitive than logically grounded—became an initial point for a new, extensive branch of research into mitogenesis.

[Here the authors discuss further the results obtained in the studies of self-reproduction of oxidative deaminase activity and of activities of various other enzymes in aqueous amino acid solutions irradiated by mitogenetic rays. The more detailed, up-to-date consideration of this highly exciting and puzzling problem is presented in the paper by A.G. and A.A. Gurwitsch, "The Problem of Autocatalysis (Autoreproduction) of Some Cyclic Compounds from Lower Amino Acids," *Enzymologia,* Vol. XX, pp. 1-16, 1958.]

Let us now summarize the complicated development of investigations and the present-day status of mitogenesis. One might forgive and forget its intricate, error-laden history of two decades if, at present, our major results and conclusions were really regarded as trustworthy. However, according to a viewpoint that dominates in scientific circles generally, the situation is unsatisfactory. Our own viewpoint is quite different, although we would certainly like the situation in this field to be much better than it really is. However, our motivations differ from the reasons of our opponents. As we have often mentioned, we cannot point to any case of a refutation of any of our positive experiments, that is, those demonstrating the existence of mitogenetic radiation. Thus, we have the right to object to a skeptical and distrustful attitude toward our data.

What seems to be absolutely unsatisfactory, and even hopeless, is the completely isolated position of the science of mitogenesis among other, neighboring sciences and the resulting fruitlessness of both our data and our theoretical constructions.

We might consider it rather natural and, at the beginning, even tolerable, if our colleagues, while remaining skeptical with respect to the theories derived from mitogenetic facts, accepted our experimental results as real. These facts could then be introduced into the everyday usage of the firmly established disciplines and, even if the conclusions drawn from them by specialists might differ considerably from our own, that would probably affect only our personal ambitions, rather than the interests of science. In any case, we would be satisfied that some fundamental problems within various disciplines should be seen in the light of new discoveries.

However, just because the acknowledgement of our results would lead to an inevitable break with habitual concepts, scientific circles prefer to abstain from accepting them. Actu-

ally, the hasty phrase of Hill, who we mentioned above, is a good illustration. As mentioned, mitogenetic spectral analysis of active nervous tissue brings new concepts to the fore, which are incompatible with the poor idea of the electric character of nerve excitation. The unwillingness to break with these deep-rooted concepts is understandable and pardonable. What is completely inadmissible is to justify one's own quietism by claiming, as Hill does, that "all of this seems to exist only in the fantasy of the Russian authors."

We cannot accept the facts of the rejection of our methods—methods that might be an inexhaustible source of new data. Nor can we accept an attitude of indifference to our results, which forces us to extend our studies into some fields in which we feel ourselves mere amateurs. No doubt, the specialists are able to treat the same problems on a higher scientific level. However, because they show no interest in our research, we have to do this job by ourselves, being satisfied if the data obtained are at least trustworthy and if the related theoretical concepts do not contradict those established in other disciplines. Even when our statements seem to be incompatible with generally accepted views, this cannot be a reason for the automatic rejection of the facts.

Our last task will be to outline the near, and probably more distant, future of mitogenesis. In this context the following question arises: Does mitogenesis remain as an unresolved problem, as a kind of scientific enigma, or, on the contrary, are the foundations of mitogenesis sufficiently elucidated to enable its use as a new, highly valuable concept applicable to a variety of fields in the natural sciences? Certainly we cannot draw boundaries between these alternatives. We can only speak of one of them predominating.

We suggest that the second part of the question—the use of the mitogenetic method—will prevail and will gradually push into the background the first, the question of mitogenesis itself.

Within the limits of the purely mitogenetic problem we can raise only two questions: What is the origin of weak UV emission from homogeneous and organized systems, and what are the mechanisms of the mitogenetic effect, that is, of the control of cell division? We find it warrantable to claim that the main question of the origin of UV emission during chemical reactions is elucidated to such an extent that further work will flow in a common physico-chemical channel, and thus UV emission will no longer remain a specific enigma for this field of science.

The situation with degradational radiation seems to be much more complicated. Of course, our explanation of this phenomenon is simply a preliminary construction in need of further development. We suggest that in the near future it will be the central, and perhaps even the only, problem of mitogenesis. It has already become a foundation for new concepts related to the main problems of biology—a field theory.

As for the the main mitogenetic effect—the induction of cell divisions by one or several photons—the striking parallelism between the action of weak UV of mitogenetic intensities upon amino acids, peptides, and whole cells, leaves no doubt that the mitogenetic effect of UV is identical to that of splitting (oligo)peptide (amino acid) molecules with a high probability of detaching hydrogen from the amino group. This process triggers the peptide synthesis. Why such elementary reactions lead to cell division is certainly one of the most important and difficult biological problems, and mitogenesis plays its role here.

However, the initial and intermediate links of the radiation-stimulated process, the intermittent irradiation, the transformation of stimulation into inhibition, and the striking disproportionality between the number of absorbed photons and the number of initiated mitoses, all remain obscure.

As we have mentioned, mitogenetic methods disclose microevents that cannot be extrapolated, as a rule, to macrosystems. This can throw doubt upon the prospects of studies in this direction. We believe that this area should be highly fruitful and that the discovery of microevents may be of great importance. First, these events may serve as signals of the existence of other microprocesses escaping direct observation. Application of our methods to the study of neural processes appeared to be fruitful for the most part. Here the role of mitogenetic radiation may be similar to that of "action currents," the latter also being the signals of something happening. Because mitogenetic effects show extreme sensitivity, and especially quantitative variability, they have an advantage over electrophysiological methods. The mitogenetic method permits analysis of the subject and the process of excitation. Just as the familiar form of spectral analysis became the foundation of modern concepts of atomic and molecular structure, mitogenetic radiation—being a signal of molecular processes—provides the possibility of deeper penetration into the properties of the excited biological substrata.

Only the future development of science will show whether the wall separating classical and mitogenetic physiology will crumble.

The same can be said of the mitogenetic analysis of the cancer problem. Here, also, we have no connections with classical oncology, except for some interest in the extinguisher phenomenon for cancer diagnosis. We consider these results less important than other mitogenetic data related to carcinogenesis.

An even more extensive field for mitogenesis has been opened by the discovery of degradational radiation. The amazing sensitivity of the degradational spectra, reflecting even minute influences upon living systems or particular organs, provides new means for elucidating the functional interrelations between different systems. If, indeed, an irritation (excitation) of a certain system A results in changes of the spectral composition of system B, the latter being, at first glance, quite independent of A, we should claim that the two are interconnected, even if this can not be registered by routine methods. But even in those areas where such connections are detected by classical methods, the use of degradational methods may disclose some new regularities.

Development of this boundless field belongs to the future. Mitogenetic methods should be in common use in physico-chemical studies. Yet, the technique for detection of free radicals, which we have elaborated, has been almost completely neglected up until now, although the appearance of free radicals is not just a mere signal; sometimes this event is important for understanding the mechanism of a process. This concerns, in particular, the analysis of those enzymatic processes

From archives of L. Beloussov

Gurwitsch's laboratory in Moscow in June 1948. Gurwitsch is second from right, first row. His daughter, Anna, is third from right.

in which only the final products, but not the intermediate ones, are known. This is true even for such a simple system as, for example, urease + urea. Resonance illumination of this system with mitogenetic rays made it possible to detect the existence of free radicals such as the carbonyl group (=CO). This may certainly be a determining factor for understanding the whole process.

One should keep in mind that we are probably dealing here with the main course of the enzymatic reaction, rather than with some negligible microevent. Such a conclusion is corroborated by the following: Mitogenetic analysis does not reveal radicals as such, but only those radicals that are excited by photons. It is appropriate to mention here that in some cases, even very weak UV illumination is enough for the detection of free radicals by our method of resonance scattering. Because the probability of photon absorption by a short-lived free radical is very low, the number of radicals excited by external irradiation will comprise a negligible part of the whole.

To summarize, the sphere of application of mitogenetic methods in biology appears to be almost inexhaustible. Obviously it would be ridiculous to make any scientific prognosis here, in light of the complete disregard for our results. But we made it a rule to evaluate the results of mitogenetic investigations regardless of predominant opinions.

It would be useless to enumerate all of the spheres in which mitogenetic methods can be applied. The general progress and expansion of the application of mitogenesis—which has already given us, and should give in the future, a new biological horizon—seems to be very important.

We would like to point out at least that mitogenetic methods permitted us to reveal the existence and importance for living systems of chain processes and of a regular nonequilibrium arrangement of molecules. Until now, these two concepts, although they are completely alien to classical biology and cytology, have been necessary for understanding the main biological processes. It is extremely important to take these concepts into consideration. Moreover, after they obtain general recognition, many conventional concepts will be looked upon in the light of new discoveries and will be subjected to reconsideration.

At the same time, this review, although incomplete, brings us to the following general conclusion: If further development of the idea of the mitogenetic phenomenon and the attitude of science toward it were to become normal, "mitogenesis" should become completely dissolved into the realm of related disciplines. The very term "mitogenesis," as the name of a specific discipline, should disappear, as its role would have been played out.