



האקדמיה הלאומית הישראלית למדעים
THE ISRAEL ACADEMY OF SCIENCES AND HUMANITIES

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Professor Joshua Lederberg
President Emeritus
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Dear Josh,

During our recent meeting, you shared with me the content of a letter written to you by Max Delbruck in the late 1940's. This reminded me of my own encounter with Max in 1953, in which you might be interested. Following is a short account of my first meeting with Max.

During 1953, I stayed in the U.S., at Columbia University, as a post-doctoral student and spent the summer of this year in Cold Spring Harbor Laboratory attending the Bacterial Genetic Course. During the summer, I met a variety of interesting scientists who were also spending their summer there. One of them was Max Delbruck, who was well known for his work on Bacteriophage.

When I got to know him better, he told me that he stopped working on Phage, and started a new field of research, the study of Phycomyces. When I asked him why he changed fields, he complained about the "not very collegial relations" between members of the Phage Group. He said that many members of this group were reluctant to exchange bacterial and Phage strains and to share information. He did not like the competitive relations between the scientists in the group and therefore decided to start a different and new field of research, the study of Phycomyces.

The research system he developed was simple and elegant. He used the aluminum caps usually used to cover test tubes, filled them with breadcrumbs, autoclaved them, and inoculated them with spores of Phycomyces. After the development of the Mycelia, on the bread surface, the vertical sporangiophores started to grow rather rapidly. They emerged above the rim of the aluminum cap and their growth rate and direction could be measured through a horizontal mounted microscope.

During the summer of 1953, Max was working on phototropism of sporangiophore growth of Phycomyces. By putting the aluminum caps between two light sources in the dark room, he could show that the sporangiophores grew in the direction of light source. The direction of

growth changed when he alternated the direction of the source of light at constant times. In such experiments the growth alternated in its direction^{and} the growth curve when traced through the microscope was a spiral. He found that in a dark room, that organism was rather sensitive to low light intensities but he found it difficult to measure the lower limits of light sensitivity in this organism. When he discussed with me this problem, I suggested to him to use luminescent bacteria as a light source.

During their log growth, they emitted the amount of light corresponding to their concentration. The light emission was a factor of the density of bacterial culture. By diluting the culture, it should be easy to calculate the amount of light which such suspension emitted.

Delbruck's reaction to this proposal was rather violent. "There are no luminescent bacteria," he declared. "Bioluminescence", he stated, "is one of the inventions of the biologist." It was clear from his attitude that he had no confidence whatsoever in facts reported by "biologists", which were, to say, the least unreliable.

I had worked with marine bacteria from the Mediterranean, and had some practical experience with luminescent bacteria. (The classic paper describing them came from Beijerinck in Delft at the end of the 19th Century). Therefore, instead of disputing Max, I decided to isolate the bacteria, which was very easy.

A few nights after our discussion, I knocked on Max Delbruck's door, and showed him a strong light-emitting bacterial suspension in an Erlenmayer flask. He looked rather flabbergasted and surprised. But, his reaction surprised me. He asked if I would be ready to join him for the rest of the summer in his research on *Phycomyces*, an offer I was very happy to accept.

We tried the dilution experiments that next morning and they turned out to be successful.

Max made one provision for my joining him in his work. I was forbidden to go to the library and read about *Phycomyces*. Max was so sure that the literature gave unreliable facts, and confused descriptions, that any study of it was counter productive.

Personally, I enjoyed working with Max during this summer. He was very nice to me and even suggested that I continue to work with him for a few weeks after the Bacterial Genetics Course was over, an offer I could not accept as I had to return to New York to complete my work there.

During this summer, when working with dilution of luminescent bacteria as a low intensity light source, we saw for the first time, "Dark Adaptation" in *Phycomyces*. After exposure to strong light, it took sometimes a long time until the mold could "see" the low intensity light. The lag time of this response to low intensity light, after being exposed to strong light, was proportional to the intensity of the strong light. This phenomenon of dark adaptation of vision was known and studied in men. I do not know if it was at this time already known to occur in plant phototropism.

I found working with Max very interesting. He was very different from all other scientists

with whom I cooperated until then in his attitude to biological systems. The possibility of objective exact measurement and quantitations were a pre-condition for his acceptance of essential experimental system. He analyzed the system we worked with according to mathematical models he proposed.

His thinking was absolutely logical & straight. There was little place for chance observations of Nature, or trying to observe new phenomena. He wanted a system which would respond to logical questions put to it, and give quantitative rational answers. I had the feeling that he brought to biology the way of thinking of a physicist. The mathematical analysis of the growth curve and its response to light of the organism was his objective in this summer. During the summer, we also found that the degree of response to light also depended on the spectral composition of the light, and we were able to construct an "Action Spectrum" of light. I thought that such a finding indicated the existence of a pigment, part of the mold light receptor, and suggested that we try to extract this pigment. Max was absolutely opposed to this suggestion, because it involved "chemistry" and he refused to use any chemical approaches to biology.

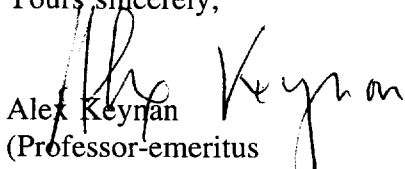
During this summer he also had some violent discussions with Seymour Cohen, who tried to study the biochemistry of Phage. Max was absolutely sure that by asking simple questions to the system he studied based on a mathematical model, he could really "understand the system".

Max was very nice to me, but rather rude and aggressive when he attended student seminars. He interrogated the students with questions posed to expose their inability to think logically or show their ignorance. I did not like this side of his personality. Listening to his questions to the students, I got the impression that he believed that his "framework of thinking" was the only valid one for the study of biology. He did never consider the possibility that there are other valid approaches, attitudes, and "thought frameworks" which also could lead to the understanding of the phenomenon of life.

For many years, I had friendly relations with Max, saw him every few years, but we never collaborated again.

Please be free to make any use you wish of this letter, or any other part of our scientific or policy-oriented correspondence, or of my unpublished papers, including postings on the public web sites of the National Library of Medicine.

Yours sincerely,


Alex Keyran
(Professor-emeritus

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