



growth changed when he alternated the direction of the source of light at constant times. In such experiments the growth alternated in its direction<sup>and</sup> 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".

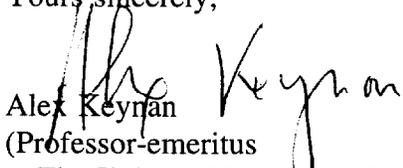
Max was very nice to me, but rather rude and aggressive when he attended student seminars. He interrogated the students with questions posed to expose their inability to think logically or show their ignorance. I did not like this side of his personality. Listening to his questions to the students, I 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.

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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,

  
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