Remembering Max Delbrück: The Scientist and the Man

Makkuni Jayaram

Epigraph

I wrote this article rather hurriedly, stringing together thoughts as they floated in, without attempting to polish or embellish them. These are my recollections of how my association with Max Delbrück came about and of my interactions with Delbrück and the Delbrück family during the few years I spent at Cal Tech. To those readers who find my style too anecdotal, or too self-centered, I apologize. And to those who wish to know more about Delbrück, his impact on science and scientists, I strongly recommend the reading list at the end.

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Looking Back in Time

When I was asked to contribute to this issue of *Resonance* honoring Max Delbrück, I was instructed to address myself to the students in Indian universities at the bachelor's and master's level. That advice took me back over a quarter of a century in time, to my own undergraduate and graduate days, first as a science major and then as a biochemistry major, at the University of Kerala, the Indian Agricultural Research Institute (IARI) in New Delhi, and finally at the Indian Institute of Science (IISc) in Bangalore. At the time that I entered my bachelor's degree program in chemistry, the double helical structure of DNA had been solved for more than twelve years or so. Yet, even at the time of graduation, my own knowledge of DNA was woefully inadequate. I only knew it as a chemical polymer; a rather monotonous one constituted by sugar-phosphate backbones and the attached four bases. I was oblivious to the geometric elegance of the molecule, and ignorant of the mechanism by which it harbored and duplicated the blueprint of life. I had not even heard the names Watson and Crick! How times have changed! Thanks to the advances in the curricula, im-

provements in teaching methods, the advent of popular science magazines and journals, and the revolution in communication technology, I believe that almost all of the current university students in India (and perhaps a significant fraction of high school seniors as well) have some awareness of the universal impact of molecular biology, genetic engineering and biotechnology. It is amusing, even paradoxical, to note that Max Delbrück, whose work with the bacterial viruses and the fungus *Phycomyces* represented the antithesis of 'big science', had unwittingly provided the intellectual leadership for the *'new biology'* that was to become the latter day *'Molecular Biology'*. In one of the first international congresses that I was exposed to, I heard Delbrück being described as the *'high priest'* of the *'church of molecular biology'*! I wonder how Max would have reacted to that title! (Max abhorred pompousness; he was Max to everybody, his children, his colleagues and even strangers!)

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Meeting Max Delbrück

My own association with Max came about as the result of a curious set of circumstances and coincidences. By the end of 1976 or early 1977, my wife Rasika (Harshey) and I had finished our PhD degrees, and were on the lookout for postdoctoral positions abroad. Rasika had studied the mechanism of RNA synthesis in *Mycobacterium* in T Ramakrishnan's laboratory in the Microbiology Department at IISc. My own work was done under Ganguly's supervision in the Biochemistry Department, and dealt with vitamin A and steroid hormone metabolism. While Rasika's work qualified her to be a 'molecular biologist', I most definitely did not fit the bill. Nevertheless, molecular biology fascinated me. I remember the excitement I felt, while still a Master's student at IARI, at hearing the news of the chemical synthesis of a gene by Khorana and his associates. I had toyed with the idea of doing my thesis work with T M Jacob, an ex-colleague of Khorana, who had established his own laboratory at the Bangalore Institute. My first impressions of Jacob (which turned out to be most definitely wrong) were that he was intellectually too placid. I wanted a more rigorous advisor, and

in Ganguly received perhaps even more than what I had bargained for! I suspect that Jacob's assessment of my own potential and discipline, as a graduate student was not terribly favorable either. Anyway, I regularly attended, and at times actively participated in the weekly noon molecular biology meetings. I liked the molecular biology approach to addressing and tackling problems; I wanted to be a molecular biologist.

And so I was quite excited (and quite nervous) when Max came to India in early 1977 to inaugurate an International meeting on virology and Ramakrishnan recommended to him Rasika and myself as potential postdoctoral research fellows. I had read here and there about how Schrödinger's book *What is Life?* had inspired a number of eminent physicists to explore biology, and how Max was lured by the fascination of revealing new laws of nature unique to the living world. I also knew that his work with Luria on the theory of mutations in bacteria had won him the Nobel Prize in medicine. I knew little, next to nothing, regarding his work on the sensory responses of *Phycomyces*. And so when, one evening during the virology conference, Max took me to his hotel room, introduced me to his wife Manny, chatted briefly about my PhD thesis, and asked me and Rasika to go to Cal Tech and work with him, I was elated. This was my passport to 'molecular biology'! Little did I know that I was in for a big surprise!

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The Journey in Quest of Molecular Biology

We arrived in Los Angeles on the fourth of July, 1977, watched the independence day fire works on Santa Monica beach, and were driven to Pasadena the next day by a friend of a friend. Max welcomed us, arranged a room for us at the Athenaeum (the Cal Tech guesthouse), and then by way of introducing us to the members of his laboratory, asked me to give a seminar on my thesis work. This was my first taste of the many weekly 'confession sessions' that were to follow, and that Max loved to preside over. He interrupted me a few times to ask questions, and smiled rather inscrutably as I answered, making me nervous. Later on

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I realized that I could have gotten away with almost anything, for neither Max nor his colleagues knew anything about vitamin A metabolism except perhaps for the role retinol played in vision. Because of my familiarity with isoprenoid biochemistry, I was assigned a project on analyzing the light induced synthesis of β-carotene (LICS; pronounced licks) in *Phycomyces*. And Rasika, with her molecular biology training, was given the task of studying the genome organization of *Phycomyces*. After a couple of days of puttering about in the laboratory, I realized that the kind of molecular biology that I had come in search of was missing from the Delbrück laboratory. Except for some modest and quite limited contribution towards analyzing the 'C_0t' curves' of DNA hybridization, described in Harshey and others (1976), my work at Cal Tech fell completely outside the realm of molecular biology.

Learning *Phycomyces* Sensory Physiology

Getting started in an American laboratory and getting used to the American way of doing science produced its humorous and mildly embarrassing moments. Disposable pipettes were a novelty. I watched pipettes being used, and immediately being thrown into the trash can. I followed that example, and without realizing the distinction between disposable plastic pipettes and reusable glass pipettes, I dispatched them all to the same trash can. The transgression was quickly discovered, and the culprit unveiled, but the incident was dismissed with some hearty, good-humored laughter. Although my dream of exploring the molecular biology of *Phycomyces* under the tutelage of Delbrück was short-lived, understanding the behavioral responses of *Phycomyces* posed a fascinating challenge. The initial sense of disappointment was significantly ameliorated by this realization.

Phycomyces is a remarkable unicellular, but multinucleate fungus. It responds with almost astounding sensitivity to a number of external stimuli including light, gravity, stretch, and barriers (see *Figure* 1). The 'giant' sporangiophore of *Phycomyces* can

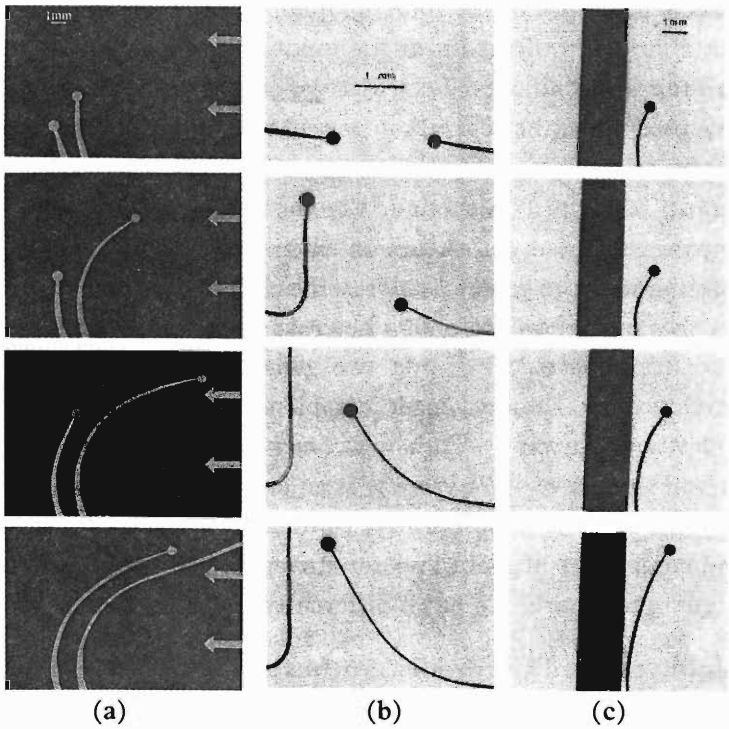

Figure 1. Time lapse pictures showing phototropism (a), negative geotropism (b) and barrier avoidance (c). The progression of the experiments is from top to bottom. Panel b shows wild type Phycomyces (right) and a mutant (left) whose negative gravitropic response is much more rapid. For the experiment in a, red light was used to expose the film. (Reproduced and adapted from Phycomyces, Cold Spring Harbor Laboratory, 1987).

perceive blue light over an amazingly wide range of intensities (10^{-8} W/m^2 to 10 W/m^2), and responds to it by exhibiting positive growth to symmetric illumination (photomeicism), and directed bending to asymmetric illumination (phototropism). The sporangiophore is negatively geotropic, shows positive growth response upon stretch, and avoids solid objects placed in its vicinity. The mycelial responses to outside stimuli are also fascinating. The mycelium responds to blue light by stimulated synthesis of β-carotene, increased initiation of sporangiophores, and enhancement in the formation of sporangia. When mycelia of the opposite sex meet, an elaborate mating ritual is triggered. The mating partners synthesize large amounts of β-carotene, and form special sexual hyphae called zygophores that pair and undergo identical morphological changes to form zygospores. Max and his colleagues (notably Enrique Cerda Olmedo and Arturo Eslava from Spain and Tamotsu Ootaki from Japan) had isolated, and characterized a number of mutants with striking

defects in several of the sporangiophore responses: the 'night-blind' mutants affected near the input end of the light signal, and the 'stiff' mutants affected at the output end. These were classified under the general category of *mad* mutants. The name was an acronym for *Max Delbrück*, but fittingly described their erratic behavior. Although, the multinuclear character of *Phycomyces* posed difficulties in obtaining mutants and performing standard genetic analyses, the clever technique of grafting sporangiophores to obtain heterokaryons (mycelia regenerated from spores containing two genetically marked nuclei) developed by Tamotsu made complementation analyses easy and straightforward. To a newcomer like me, the apparent overall simplicity of *Phycomyces* as an organism (which in some ways turned out to be illusory) contrasted by the multiplicity and complexity of its behavioral attributes was simply too intriguing to ignore. To put it differently, I was hooked.

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Light Induced Carotene Synthesis

One of the early experiments that I did with Max was to quantitate the differences in the amount of β-carotene in *Phycomyces* grown in the dark, and then exposed to defined fluences of blue light [2, 3]. We used a rather ordinary looking, home made device put together by Mike Walsh and David Presti (Mike Walsh ran an electronic shop at Cal Tech, and David Presti was Max's last graduate student) to do this. The whole set-up was housed in a dark chamber placed inside a dark room illuminated by a dim red light under which the experimenter recorded the readings. The photo-balance experiments that I will describe later were also carried out in this dark room. Max had designed a very clever and simple scheme to measure the mycelial carotene content *in vivo* rapidly and in large number of samples. Each layer of mycelium was sampled directly on the petridish by passing successively through it light beams of 455 nm (blue) and 633 nm (red) wavelengths, and recording the intensities of the transmitted light using a photodiode placed under the plate. The rationale was that the absorbance of the mycelium in the red represents the growth of mycelium (β-carotene does not absorb

in this region of the visible spectrum), or non-specific absorbance. On the other hand, the blue absorbance represents the sum of the growth and the β-carotene content. From a set of data points obtained with a reasonable number of plates, one could easily derive a linear regression curve between $\log I_R$ (I_R = intensity of red light) and $\log I_B$ (I_B = intensity of blue light). By doing this for a set of unirradiated control plates and plates subjected to blue light illumination, one could estimate A_{LICS} – the absorbance due to light induced carotene synthesis. In other words, for a pair of unirradiated and irradiated mycelial mats of identical red absorbance (equal growth), the difference between their blue absorbance values is a measure of the amount of β-carotene produced in response to the light treatment. Using this method, I was able to show that LICS was a biphasic response in *Phycomyces* consisting of low fluence response and a high fluence response. Furthermore, the former was insensitive to inhibitors of transcription and translation (now, there is a bit of molecular biology!), while the latter was almost completely eliminated by them. With my biochemical bent of mind, I was mistrustful of the measurements, although the data looked clean. So with a small set of samples, I redid the assays by laboriously extracting carotenes from the mycelium using petroleum ether, and measuring the β-carotene absorbance with a spectro-photometer. The close agreement between the two methods surprised and delighted me at the same time. The cleverness of the physicist had so remarkably and so effectively reduced in time and effort, the burdensome chore of the biochemist! If I recall correctly, I also showed that in one of the interesting regulatory mutants for carotene synthesis, the low fluence LICS was wiped out whereas the high fluence response was preserved. Overall, a model in which the two responses were mediated by two separate photoreceptors could accommodate the results. The primary blue light photoreceptor of *Phycomyces*, believed to be a flavin receptor, was responsible for the predominant high-fluence form of LICS. By contrast, β-carotene itself seemed to serve as the photoreceptor for its own synthesis under low light fluence. As far as I know, this was the first time that biphasicity has been

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revealed for a behavioral response in *Phycomyces*. I believe that certain other responses such as photophorogenesis (initiation of sporangiophore formation in response to light) were also found to follow suit, particularly in the work of Enrique and colleagues.

The Phototropic Neutrality Experiment: Blue Light-Green Light Balance

I also carried out a set of phototropic balance experiments aimed at characterizing indirectly the *Phycomyes* 'photoreceptor' for blue light photoresponses. If the primary photoreceptor is riboflavin, as it is generally thought to be, one might be able to substitute it by a flavin analog, which has a different absorption spectrum, say shifted to the green side of the spectrum. If the new photoreceptor were biologically active, a doped sporangiophore should be able to see green light better than a normal sporangiophore. This argument was originally formulated by Jose Reissig, and became testable when Max obtained a generous gift of the analog roseoflavin from a German laboratory. Wild type *Phycomyces* could grow in medium containing roseoflavin, although growth was somewhat inhibited. This was a good sign, indicating that the compound was indeed taken up by the fungus and presumably incorporated into flavoproteins. At least some of the substituted proteins would be expected to act less efficiently than their wild type counterparts, accounting for the slower growth phenotype. Furthermore, a riboflavin auxotroph of *Phycomyces* could also be grown in the presence of various ratios of riboflavin and roseoflavin exogenously supplied in the medium. The actual experiment consisted in placing a live, growing sporangiophore (normal or doped) between a blue light source of fixed intensity and a green light source of variable intensity (*Figure* 2). Every so often, one would test which light source the sporangiophore was bending towards. If it were bending towards blue, the intensity of the green light would be turned up. If it were bending towards green, the intensity would be turned down. By following this protocol, for each sporangiophore one could enforce phototropic neutrality,

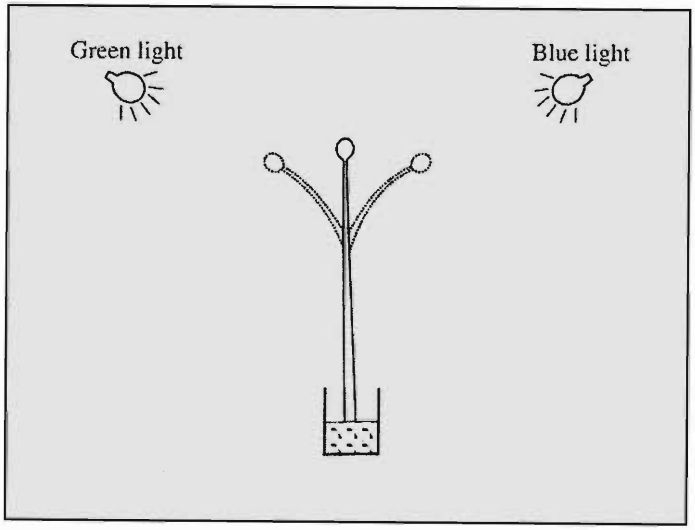

Figure 2. The experimental set-up for the photoneutrality experiment is given schematically. The goal of the experiment is to match the green and blue light intensities (as perceived by Phycomyces) so that the sporangiophore grows straight. Any imbalance would cause the sporangiophore to bend towards one source or the other (indicated by dashed lines).

that is, the sporangiophore would see both lights as equally bright and grow straight (without bending towards either light source). At times, the assays would become quite tricky and frustrating as, after several hours of struggle, one would just not find the right balance point. Interminable patience and tenacity were an absolute prerequisite! From a large number of measurements, it was apparent that there was a clear trend: the roseoflavin-doped sporangiophore indeed balanced the blue light at a slightly lower intensity of the green light than did the normal sporangiophore. Manfred Otto, who was trained as a flavin biochemist, was brave enough to redo the neutrality assay using the riboflavin auxotroph grown in carefully titrated ratios of riboflavin and roseoflavin. His results were cleaner, and confirmed my earlier observation. The data fitted two possible explanations. Either the roseoflavin was substituted in a large fraction of the photoreceptor but the quantum yield from the new receptor was quite low; or, the substitution was poor but the quantum yield was normal or close to normal [4].

Application of Molecular Tools to *Phycomyces* Sensory Behavior

Max's real dream was to understand sensory physiology using molecular tools. He was constantly seeking colleagues who

might help him achieve this goal. My own departure from the *Phycomyces* and entry into yeast molecular biology had Max's blessing. This was the time that gene manipulation methods were being applied to yeast with remarkable success. The yeast plasmid called the *2 micron circle* was proving to be a valuable tool in the construction of plasmid vectors that could be shuttled between yeast and *E. coli*. Max encouraged me to study the yeast plasmid and explore the yeast system in more detail with the hope that we might be able to exploit the technology to clone and characterize the sensory genes of *Phycomyces*. And so it was that I took up my second postdoctoral stint with James Broach who was the *expert* on the 2 micron plasmid. My own independent career in science was largely moulded by what I learnt in the Broach laboratory. This was the 'real molecular biology', the stuff of my dream that had eluded me for so long! And it has stood me in good stead ever since.

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I did make a brief attempt to return to *Phycomyces*. With much help from a Spanish colleague, Jose Revuelta, we succeeded in setting up a method for gene transfer in *Phycomyces* (albeit an extremely inefficient one), and cloned a *Phycomyces* gene and characterized its organization (the gene coding for a multifunctional enzyme in the tryptophan biosynthetic pathway). However, much to my surprise, our efforts were not well received by at least a section of the *Phycomyces* community. Arturo and Enrique and their colleagues had also become interested in *Phycomyces* molecular biology. In fact, Arturo's group had developed a low efficiency transformation system contemporaneously to us. Having stayed away from the field for three or four years, I had become an outsider to the '*Phyco* group'. The fact that Jose was Arturo's graduate student and was now my colleague added to the mistrust of the Spanish groups. Max was no longer around, and as is often the case with the passing of a strong patriarch, the unity and cohesiveness of the family was not quite the same any more. I have not seriously considered studying *Phycomyces* since. One reason for this was, of course, to avoid creating ill will. A second and a more practical one was the

general apathy towards *Phycomyces* on the part of the federal funding agencies. *Phycomyces* had earned a reputation (perhaps undeservedly so) as a genetically intractable organism. And with the advances in the molecular genetics and biology of plants such as *Arabidopsis*, the science pundits were challenging the rationale for using *Phycomyces* as a model system for exploring photophysiology. Sad to say that support for *Phycomyces* work in the States has all but dried up. And one wonders whether the day will ever come when some brave maverick scientist, breaking away from conventional wisdom, will unveil the molecular secrets behind *Phycomyces* phototropism, photomeicism and barrier avoidance! Were it to happen, Max would be both pleased and amused.

Max Delbrück: His Love of Science and Zest for Life

The two and a half years I spent in the Delbrück laboratory were a unique experience for me. In a practical sense, it certainly did not prepare me for the kind of research career that I had come to pursue. When I came out of the Delbrück world and entered the land of the real molecular biology, I found myself to be a misfit. It took me a while to regain my bearings, and the process of adjustment was not painless. Nevertheless, the time spent at Cal Tech was well worth the while. It exposed me to Max the scientist and the man. At first I was in awe of him, too much so to be intellectually productive. The manner in which his brilliant analytical mind (no doubt influenced by its training in physics) perceived and tackled problems set forth a lofty example to aim at, even though an impossible one to achieve. Max was as critical of his own experiments as he was of everyone else's. The weekly group meetings (the confession sessions) revealed Max's penchant for critical thinking, and his intolerance for intellectual indifference. His impatience with poorly organized scientific presentations made him come off occasionally as mean and uncharitable. And this, I was told, was a mellowed Max. There were stories aplenty about how Max in his younger days had prided himself in intimidating many a

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seminar speaker at Cal Tech. However, beneath this harsh and overbearing scientific exterior was a kind and generous human soul, caring and sympathetic. I experienced it first hand during a difficult period of time when my own faith in personal trust and loyalties was seriously challenged.

What was most striking about Max as a scientist was his almost insatiable enthusiasm for intellectual pursuits. Max's oft-quoted riposte to his colleagues, "*I do not believe a word of it*" was almost always a well-meaning challenge to them to prove him wrong. Having been shaped in his formative years by the cultural and intellectual tradition of Europe, and having enjoyed the unrestricted freedom to exercise them in America, Max combined the best of both worlds. His mind often transcended the practice of everyday science, and grappled with difficult epistemological, philosophical and even theological questions. He dealt with several of these issues in a course on 'evolutionary epistemology' that he offered to students during his final years at Cal Tech. Thanks to the efforts of Gunther Stent, Peter Fisher, Solomon Golomb, David Presti and Hansjakob Seiler, these lectures (or essays) have been organized into a book called *Mind from Matter*?[1]. The title reflects Max's obsession with the question of how a 'mind' capable of the most profound intellectual and artistic pursuits could have arisen from lifeless matter and evolved by a Darwinian process.

[1] Reviewed in this issue.

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Unlike the molecular biologist of today, who at the end of a hard day's work, carries more of it home in his/her brief case, Max was always keen on finding time for relaxation. He often warned us against confusing 'hard work for hard thinking'. Family and friends were of utmost importance to him. There were many gatherings organized by Manny in the Delbrück garden that included the Delbrück family, the neighbors, prominent visiting scientists, and of course members of the Delbrück laboratory. The party usually started with outdoor activities like volleyball and badminton, followed by dinner, and then retreat in the Delbrück house for fireside chats and parlour games. And there were the frequent hiking trips to the mountains outside

Pasadena, and camping trips to the deserts of California that the Delbrücks could not live without. They had their own chosen campsites, rugged and pristine, the farthest possible from civilization. They organized long and arduous walks through difficult terrain properly named the 'death marches'. Being rather sedentary in my ways, I was not a particularly enthusiastic participant in these activities. I avoided the outings with the Delbrücks whenever I could, and was worn out by the few I could not escape from. Max never understood how anyone could shrink away from the pure joy of being liberated by the wilderness!

Box 1. Delbrück and Physics

Max Delbrück started his career with a problem in astronomy, relating to the so called 'novae' (stars which suddenly increase their brightness). But he was unhappy with having to scan through the relevant literature in English and also with the mathematics used by the astrophysicists, so he turned his attention to physics, and indeed spent time at some of the great centres such as Bristol, Gottingen, and Copenhagen. His thesis was on the quantum chemistry of lithium, but his best known contribution, which is still referred to in the textbooks as 'Delbrück scattering' came later in the field known as quantum electrodynamics.

The early 1930s saw acceptance of Dirac's concept of the vacuum as a sea of an infinite number of filled negative energy states, which were normally invisible. But the promotion of an electron to a positive energy state by a photon with enough energy (greater than 1.1 million electron volts) would leave behind an empty 'hole' which would behave as a positive charge. One consequence of this picture was that in principle, a quantum of light could be scattered, i.e. change its energy and momentum, by encountering just the electric field of a nucleus. The classical ideas of Maxwell would say that the electric fields of the light wave and that of the nucleus just add up without each affecting the other. So this is a purely quantum process, in which the light wave temporarily creates an electron and a hole, one of them gets scattered by the nucleus, and they then recombine to produce the scattered photon. The calculations were quite difficult to do using the methods available at that time, and the final answer showed that the process would be very weak in most cases. It is now history that Delbruck decided soon after to enter biology.

But the story has another twist. One of the trio who developed quantum electrodynamics much further in the late nineteen forties was Richard Feynman, Delbrück's colleague in the California Institute of Technology. Using Feynman's methods, a student can now calculate Delbrück scattering in an hour or less. But Feynman himself started taking great interest in biology towards the later phase of his career, and was a weekly visitor to Delbrück's lab. Apparently, he was able to make an important discovery but a few weeks later than another group. This prompted Delbrück to remark in a lecture given in Bangalore that one can't afford to work one day a week on an important problem, "*even if one is Feynman*". But in any case quantum electrodynamics finally caught up with Delbrück!

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Max Delbrück was a great scientist and a rare human being. The apparent confidence and arrogance that he projected belied an almost childlike warmth and insecurity that he tried to conceal even from those closest to him. His elegant work with Luria on phage mutations had set a yardstick by which other experiments of that time in the phage field would be gauged. In this now classical 'fluctuation test', Luria and Delbrück divided equal aliquots of a young *E.coli* culture into several tubes, and after a given number of generations counted the number of colonies that each tube yielded upon emptying its contents on a plate overlaid with the killer virus T1. A cell that was resistant to T1 phage could only have seeded each surviving colony. The numbers of resistant colonies fluctuated wildly from tube to tube, giving rise to a large variance about the mean. By contrast, the corresponding numbers from equivalent samples of the mother culture (also grown for the same length of time) were clustered closely around the mean. From these results, Luria and Delbrück deduced that the resistant colonies had arisen in the tubes prior to challenge by the phage. Tubes that, by chance, had a resistant cell form early during growth would contain larger number of the resistant cells (by multiple divisions) than tubes that had a resistant cell form later, or even not at all. The phage itself had nothing to do with inducing the resistant character in *E.coli*. Thus, the origin of mutations was random, not adaptive. Members of the phage group yearned to measure up to Max's high standards. Yet he walked away from phage when he felt that the field of his own creation had grown beyond the need for his parenting. His separation from phage united him with *Phycomyces*, which he believed would prove to be the 'phage of behavior'. And so he replaced the phage group with the *Phyco* group. Unfortunately, *Phycomyces*, unlike the phage, did not live out its promise; and the scientific impact of the Phyco group was not the same as that of the phage group. Yet, for those who pursue science for the sheer joy of doing science, *Phycomyces* has never been a disappointment. And all of us who were fortunate to share in that joy will be grateful to Max.

Suggested Reading

[1] R M Harshey, M Jayaram and M Chamberlin, DNA sequence organization in *Phycomyces blakesleeanus*, *Chromosoma*, 73, 143–152, 1979.

[2] M Jayaram, D Presti and M Delbrück, Light induced carotene synthesis in *Phycomyces*, *Exp. Mycol.* ,3, 42–52 ,1979.

[3] M Jayaram, L Leutwiler and M Delbrück, Light induced carotene synthesis in mutants of *Phycomyces* with abnormal phototropism, *Photochem. Photobiol.*, 32, 241–245, 1980.

[4] M K Otto, M Jayaram, R Hamilton and M Delbrück, Replacement of riboflavin by an analogue in the photoreceptor of *Phycomyces blakesleeanus*, *Proc. Natl. Acad. Sci.* (USA), 78, 266–269, 1981.

[5] Ernst Peter Fischer and Carrol Lipson, *Thinking about Science*, W W Norton & Company (a biography of Max Delbrück).

[6] Max Delbrück, *Mind from Matter*, Blackwell Scientific Publications Inc., 1986 (a collection of mildly edited essays).

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