ASPB Pioneer Member

Eberhard Schäfer

I was born on June 23, 1945, in Thuringia, East Germany. I grew up in a small village in Lower Saxony (North Germany) and, later on, in Duisburg, the city with the largest inland harbor in Europe. There, my father worked as a merchant engaged in the oil business. Regarding the education of his two sons, however, he hoped for them to study something "real", instead of following his example as a merchant. Thus, the two sons soon discovered and developed a strong interest in the natural sciences; my brother studied mechanical engineering in Karlsruhe and I inscribed for physics at Freiburg University. After finishing my Masters degree in physics, I had originally intended to use this knowledge for medical research. Even though I was interested in biology as well, I decided to concentrate my studies – in addition to physics – on mathematics, with an emphasis on the theories of differential equations. In retrospect it was pure luck!

A friend in the Physics Department, a doctor of medicine, gave me the good advice to obtain my doctorate in medicine if I wanted to do research in this field; otherwise, I would very probably end up as a slave for a boss in medicine. Unfortunately, my father died of a heart attack shortly before I finished my diploma/Masters in Physics in 1969; thus, I had no financial support to study medicine. Although I was offered an interesting topic to continue my PhD in the Physics Department in Freiburg, I was rather obsessed by my dream to do research in either medicine or the closest possible discipline which, for me, turned out to be biology.

As the new faculty for biology in Freiburg had a very good international reputation, I tried to obtain an offer to do my PhD work there. At that time there was no Biophysics Department or faculty and, therefore, I contacted Bernhard Hassenstein, a well-known expert in cybernetics. But he had, unfortunately, decided to change his research interest and gave up studies in biophysics and biocybernetics. This is why, as a next step, I tried to contact Rainer Hertel, a professor in molecular biology, who had just come back from the US. But he had already employed an Assistant, the physicist Dieter Marmé, who had just finished his PhD in Hans Mohr's lab. Therefore, Hans Mohr had an open position available and needed someone with a background in physics for his photobiological equipment. I got this job with an excellent chance to work on my PhD.

In vivo spectroscopy of phytochrome, using the Ratiospect – built by Butler and Norris at Beltsville, became my hobby and my job from then on. Thus, I did not attend any lectures in biology, but instead read the few available research articles on phytochrome and immediately started to do experiments. Luckily, the instrument, half automated by Dieter Marmé during his thesis, worked very well, so that I could measure the accumulation, degradation, and steady states of phytochrome. I had always been interested in mathematics, especially in systems of differential equations, and this modelling worked nicely with the phytochrome system. I realized at an early stage of my PhD work that the so-called High Irradiance Response of phytochrome presented a problem that could be solved only with the help of mathematical modelling.

Two events during my thesis research were of great importance. Firstly, a NATO meeting on photomorphogenesis in Eritrea. There, I had a wonderful chance to meet most of the experts in this research field and started discussions with them. Secondly, Peter Quail had accepted a postdoctoral position at Freiburg University in Hans Mohr's group and, quite surprisingly, he decided to cooperate with two

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crazy physicists, Dieter Marmé and myself. I tried to teach Peter some physics and mathematics, and he taught me cell biology and biochemistry. I am not sure if this endeavor has been very profitable, but it has been a wonderful time. Owing to a number of discussions with Peter Quail and, later on, many regular guests in Hans Mohr's laboratory, I was enabled to learn a lot about the differences between the Anglo-Saxon and the German approaches toward physiological questions. Later, I met Masaki Furuya from Tokyo University, who invited me for regular visits in Japan at least once a year. I was even invited to spend a three-month sabbatical there at the RIkken Institute, where I became acquainted with a new way of guiding a research group and began friendships with a number of Japanese photobiologists.

I succeeded in finishing my thesis by the end of 1971, and, quite surprisingly, was immediately offered a permanent position in the department, which meant the start of my own research group. This opened a very promising time with a lot of top scientists, including Winslow Briggs and Peter Ray, who visited our biology faculty in Freiburg. Thus, I had a chance to continue my mathematical modelling with the aim of gaining my habilitation in 1975, and shortly afterwards my professorship, comprising duties of teaching plant physiology and biophysics. This meant an enormous amount of work, but a great pleasure at the same time.

Beside phytochrome and photomorphogenesis, I became interested in the optics of plant tissues and in phototropism, especially in Phycomyces. I received an invitation from Max Delbrück at Cal Tech, and, at the same time from Winslow Briggs, to a GRC conference at Oxnard, California – my first trip to the US and from then on a normal procedure two times a year.

I spent a first short sabbatical with Winslow Briggs at the Carnegie Institute at Stanford, where, together with a very ambitious postdoc, Moritoshi Iino, I had to work like a slave. This proved to be a rather successful time and opened a chance for me to return this slavery treatment to Winslow, when, owing to his Alexander von Humboldt award, he worked in my lab. After his official retirement as Director of Carnegie, we found an arrangement for Winslow as a "postdoc" in my laboratory.

In the 70's, so to say, the time of plant molecular biology started with an explosion, and I decided to change my research approach. Again, I was lucky enough to be surrounded by friends and colleagues in Freiburg who were experts in plant molecular biology; this included Klaus Apel, Klaus Hahlbrock, Günther Feix and Hans Kössel. With their help, my graduate students got excellent training, which opened a new path towards plant physiological research using molecular-biological, cell-biological as well as genetic tools. This resulted in a wonderful time of collaboration and the chance of publishing a number of nice papers together and to become lifelong friends.

Another important event happened when Ference Nagy, formerly a member of Nam Hai Chua's laboratory, was offered a position at the Fredrick Miecher Laboratory in Basel. On his way to Switzerland, he decided to visit me in Freiburg. From then on, we have continually worked together to this day. Friendships between the members of later labs in Szeged, Hungary, and Freiburg have been the basis for a long-lasting collaboration. Ference helped me obtain a better feeling for plant molecular biology, whereas I tried to give him a feeling for physiology and systems biology.

When I was offered the chair and succession of my former teacher, Hans Mohr, this meant new opportunities would be opening for me. I decided to initiate several independent research groups in my department, and it has always been a pleasure to discuss with them their different, independent research topics. In addition, more guests, including Ference Nagy and Masamitsu Wada as Alexander von Humboldt laureates, and Alan Jones as a Humboldt fellow joined my lab for their sabbaticals. In 2010 on the occasion of my retirement, we had a symposium that included speakers who had once been guests in my lab or had been constant collaborators.

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I recall that I became a member of ASPB in the late 70s or early 80ies. When I retired, I handed over all my bound Plant Physiology and Plant Cell journals, which I once had loved to have in my hands to read them in the evenings; I enjoyed the very early Plant Cell issues from a time when knowledge in almost every area of research was exploding by means of the new tools available.

What are my most important contributions?

On the occasion of a three-month sabbatical in 2005 at Berkeley with Peter Quail, he told me that I had made one remarkable contribution – just one! It was not my mathematical modelling of phytochrome kinetics, which allowed a good explanation of the action of phytochrome under continuous irradiation, i.e., the far-red High Irradiance Response. In his opinion, it was the measurement of the dark reversion of phyB and its control by interaction with ARR4. Thinking about this comment in retrospect, Peter may be even, at least, partially correct.

In 1973, together with a graduate student, I succeeded in measuring the strong temperature dependence of phytochrome dark reversion. Almost 40 years later, this discovery allowed Cornelia Klose and me to collaborate with Philip Wigge and Jorge Casal to show that Phytochrome B is a thermosensor during the day and nighttime, using dark reversion as a tool. This discovery was also based on some modelling work that I had re-started together with Christian Fleck after my official retirement. These steps finally led to a more elegant and much more data-based model of the HIR and the function of PhyB as a dimer.

A further important step is the following: In 1994, we discovered the light-dependent import of a transcription factor into the nucleus. We thought this to be an important missing link in phytochrome signaling from cytosolic-localized phytochrome to the control of transcription in the nucleus. After Akira Nagatani's pioneering work on nuclear import of phyB, we thought this was an artefact due to the GUS fusion; together with Ference Nagy, we decided to apply, in contrast, phy GFP fusions and to use the cytosolic phytochrome as a control to study light dependent transport of other factors. However, this control did not work and thus we could not only prove that phyB, but also all phys A-E, show light dependent nuclear import. In the next step, the various mechanisms of nuclear import of the different phys had to be investigated. This work and the analysis of interactions of different phys are still in progress through collaboration between Cornelia Klose and Ference Nagy's group.

Probably of similar importance as proof that phyB is a thermosensor is the discovery that UVR8 is a UVB receptor. Together with Klaus Hahlbrock, I have been working on the UVB problem since the mid-80s, and in collaboration with Ference Nagy, I could initiate an independent research group headed by Roman Ulm to work on this problem. We succeeded in solving it and got it published just after my retirement in 2010.

What advice would you give individuals at the start of their career in plant science?

You should possess love and enthusiasm for science, otherwise it would be better to do something else. It is important to be frustration-tolerant, because not every approach will lead to success. Try to become an expert in a special field, no matter how small. Broaden your knowledge based on your experiments, but do not try to do everything on your own. Share your experience, because it is a lot of fun to collaborate and bring different opinions together. In the long run this means you will have a lot of friends in the scientific community around the world, and this will help and compensate you for possible frustrations.