ASPB Pioneer Member

William L. Ogren

My science career started after I graduated from the University of Wisconsin-Madison in 1961 with a B.S. degree in chemistry. I took a position at a chemical company in Detroit where I did research on inorganic conversion coatings, processes used in the automobile, appliance, and other industries where metals used in their fabrications are chemically treated to provide corrosion resistance and a paint base. With free time on my hands and the thought that pursuing additional course work would enhance my ability to do my job, I spoke to the professor in charge of the chemistry graduate program at Wayne State University, a largely commuter university in Detroit with an extensive evening program, about attending graduate school at night. Despite an unexceptional undergraduate record, he admitted me on the spot. I was disappointed when the course schedule was announced, because the only chemistry course offered at night was biochemistry, a subject in which I had no interest. But since biochemistry was all there was, I signed up for it. I thought I could do more, so I also signed up for a course in chemical engineering that was a bit more relevant to my job. It turned out that biochemistry was more interesting than I thought it would be and the course went for a second semester. I did well on the examinations and, midway through the second semester, the professor, David Krogmann, sought me out and invited me to have dinner with him before one of the lectures. Dave was just starting his career, having been well trained in photosynthesis by Andre Jagendorf and Birgit Vennesland, and during dinner he extolled the wonders of photosynthesis and how great it would be for me work on it with him in his laboratory. I thought about his offer for three or four months, and finally decided to give the company I was working for their one-month resignation notice and joined Dave's lab as a chemistry teaching assistant in time to begin the fall semester as a full-time graduate student.

For a thesis topic, Dave suggested I look at pyridine nucleotide metabolism in chloroplasts. The basic finding in my thesis was that light induced the conversion of NAD(H) to NADP(H) and the reverse occurred in darkness, which was consistent with the role of light in photosynthesis and basic catabolism in the dark and showed that chloroplasts regulate pyridine nucleotides to adapt to differing metabolism in light and dark. While I was finishing my doctoral research I got a telephone call from Bill Rinne, who had been a postdoc in the lab adjacent to Krogmann's and was someone I got to know well in Detroit, telling me that the U.S. Department of Agriculture at Urbana, Illinois, was looking for someone to work on soybean photosynthesis. Bill had recently taken a job in the group there to work on soybean lipids. I was very hesitant about this since I had not actually worked on photosynthesis and the job was housed within the Department of Agronomy at the University of Illinois, and I had no clue about agriculture or agronomy. But I applied for the position, was interviewed, and was offered the job. Still harboring extreme doubts, I put off deciding until given an ultimatum to either take the job or they were moving on, so I said OK, I'll take the job and I became a plant physiologist in the U.S. Regional Soybean Laboratory, with the objective of improving soybean photosynthesis.

I began my new career with assistance from Dick Hageman, a very successful crop physiologist studying nitrate reductase in the Department of Agronomy at the University of Illinois. I was working alone, and Dick brought one of his students, Paul Curtis, to work with me. Paul successfully finished up his doctoral research a year later

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after looking at photosynthesis in several soybean varieties. While Curtis was writing up his thesis, Hageman came to see me again and introduced me to a new postdoc, George Bowes. Dick gave George the choice of working in his lab on nitrate reductase or working in my lab on photosynthesis, and George chose to work on photosynthesis. We decided to examine soybean RuDP carboxylase, and George ultimately found that the carboxylase was an oxygenase as well, so that the carboxylase initiated both the photosynthesis and photorespiratory cycles. I consider the discovery of RuDP oxygenase activity to be the most significant and impactful work to be done in my laboratory. Some years later the biochemistry nomenclature people renamed RuDP to RuBP, and the enzyme with its two activities, ribulose-1,5-bisphosphate carboxylase/oxygenase; it is often abbreviated to rubisco.

Two other works from my laboratory have also had a major impact on plant science and photosynthesis. In 1978 Chris and Shauna Somerville came to my laboratory with their concept that Arabidopsis could be used as a model organism to select for directed mutants in plants, similar to the way E. coli, Drosophila, and C. elegans were being used as model organisms. Chris believed he could select for mutants defective in the photorespiratory pathway and then select for revertants that might have reduced photorespiration. Chris quickly isolated several photorespiration mutants by selecting for Arabidopsis plants that survived in high CO_2 but not in air. Characterization of these mutants, specifically one deficient in phosphoglycolate phosphatase activity, confirmed that RuBP was the source of photorespiratory glycolate, still a point of some contention at the time. I believe the isolation of these photorespiration mutants, along with Chris' subsequent isolation and characterization of Arabidopsis lipid and plant hormone mutants elsewhere, played the major role in igniting the Arabidopsis revolution that has transformed the conduct of plant science.

One of Somerville's high CO_2-requiring mutants was not a photorespiration defect, but rather was unable to activate rubisco in vivo at atmospheric levels of CO_2. A few years after the Chris left the lab a new postdoc, Mike Salvucci, arrived. Mike decided to study this mutant and found two missing protein spots when he ran 2-dimensional chromatographic gels of soluble chloroplast polypeptides from Arabidopsis wild type and the mutant. We, along with Archie Portis, Jr., a fellow USDA scientist who had a laboratory across the hall and whose lab continually interacted with mine because we had many common interests, found in genetic studies that these two polypeptides correlated with the ability of the plant to activate rubisco. Archie and Mike also found that stromal extracts from wild type chloroplasts activated rubisco when illuminated in a reconstituted chloroplast system while extracts from the mutant plant did not. These observations led to the conclusion that the two polypeptides were a specific chloroplast enzyme required for rubisco activation in vivo and was designated rubisco activase. CO_2 and Mg^{2+} had previously been shown to activate rubisco in vitro and it was widely believed that these two factors also activated rubisco in vivo, but the isolation and characterization of the rubisco activase mutant demonstrated this was not the case. Portis and Salvucci focused much of their subsequent splendid careers characterizing and solidifying the role of rubisco activase in regulating rubisco activity and thereby photosynthesis. It is my belief that in the absence of this mutant we would still be unaware of the existence and necessity of rubisco activase, and one wonders what other unknown aspects of photosynthetic carbon metabolism remain to be discovered.

Some of the reasons for success in my career running a joint federal/university laboratory can perhaps be identified and replicated, but others occurred more by chance and can be best described as lucky breaks. As I stated earlier, the first two people to work in my lab were generously supported by a kind professor benefactor, Dick Hageman. Chris Somerville asked to come to the lab perhaps because

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of a previous search for reduced photorespiration in mutagenized soybeans grown under a low CO_2 atmosphere, experiments devised by Jack Widholm and carried out by Jack and me. Also, I was not particularly ambitious in the sense that I tended to be satisfied with the financial resources I had at hand, so when students or postdocs came to my lab there were no particular experiments that needed to be done and so we were always able to agree on a mutually satisfactory research project. In my first dozen years I typically had one person in the lab with some overlap between those going and coming; increased funding for personnel in in my later years typically came from my USDA budget, competitive funds within my agency, or people brought their own funds with them. But there were perhaps a couple things I was able to do to stimulate the creativity of the students and postdocs in my lab and have them stimulate my thoughts. As I mentioned above, the people in the lab and I mutually agreed on their research project, so they tended to stay motivated in their work. Also, I always assumed they knew more about what they were doing than I did, so I listened carefully to what they had to say. As a corollary to this, they were always free to pursue their thoughts into uncharted territory, which led to several significant discoveries and breakthroughs and is a major reason for the overall success of my laboratory. I want to illustrate this important point with some pertinent examples.

After telling graduate student Bill Laing that I didn't see how rubisco could simultaneously regulate both carboxylase and oxygenase activities, he sat down at the nearest desk and started writing. He quickly derived a series of equations based on enzyme kinetics which described the interaction of RuBP, CO_2, O_2 and rubisco, equations which he later showed also described the interaction of these factors as well as temperature in regulating soybean photosynthesis. These equations have become the basis for describing photosynthesis and photorespiration in all kinetic models of photosynthesis. A half-dozen years later, student Doug Jordan decided on his own to look at the CO_2/O_2 specificity of Chlamydomonas rubisco after characterizing the soybean enzyme and found it to be significantly different from soybean. To me, this was an unexpected finding and led to a survey of rubiscos from many diverse photosynthetic organisms. Doug found a wide range of rubisco specificity factors, with analogous organisms having identical specificities. This work showed rubisco has evolved over time, with the oldest organisms having the least efficient rubiscos in terms of specificity, and it also provided some optimism that photorespiration might be reduced if further improvements could be made in rubisco CO_2/O_2 specificity. At one time in the early 1980s I had two postdocs in the lab, Bob Spreitzer, a Chlamydomonas geneticist interested in improving rubisco through mutagenesis, and Marty Spalding, a plant physiologist interested in photosynthetic carbon metabolism. They put their heads together to create and characterize the first algal mutants with defects in the CO_2-concentrating mechanism, the way that algae reduce RuBP oxygenase activity and thereby photorespiration. These mutants were the first step in determining whether the algal CO_2-concentrating process might be suitable for modification and applicable to C_3 crop plants. Finally, graduate student, Jeff Werneke, whose primary research interest was molecular biology, came to the lab at a time when rubisco activase was a topic of great local interest, particularly in the Portis lab. Although Jeff was not working with rubisco activase, he was subjected to the daily chitchat about it between our labs. One day he came to me and said he thought he could determine the molecular basis for the two Arabidopsis rubisco activase polypeptides. I told him to get to it, and he found that the two Arabidopsis rubisco activase polypeptides arose from alternative splicing of a single mRNA. This research answered a question that had bothered us from the start of our work on rubisco activase, specifically why were there two polypeptides, and I believe it was the first demonstration of alternative splicing in plants other than that of transposable elements in maize.

Based on my career it is hard for me to know, much less describe, the parameters for personal

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success in science. I tended to go where the currents of life took me, and somehow these currents mostly took me to great places. I ended up working on photosynthesis, and thus by necessity with plants, but when I entered graduate school, my primary interests were inorganic and analytical chemistry, and if courses in those subjects had been offered at night when I started instead of or in addition to biochemistry, my life trajectory would have been entirely different. From time to time, I wonder how that other life would have worked out. I started out as an industrial chemist and ended up looking at photosynthesis from the perspectives of biochemistry, physiology, genetics, and molecular biology, and so my advice would be to not put any artificial restrictions in how you approach your science. In all cases, I think, one needs a certain degree of curiosity in what you are doing, a willingness to work and think hard, and a high degree of respect for the thoughts and ideas of your colleagues and associates. From that base, enjoy the journey wherever it may take you.