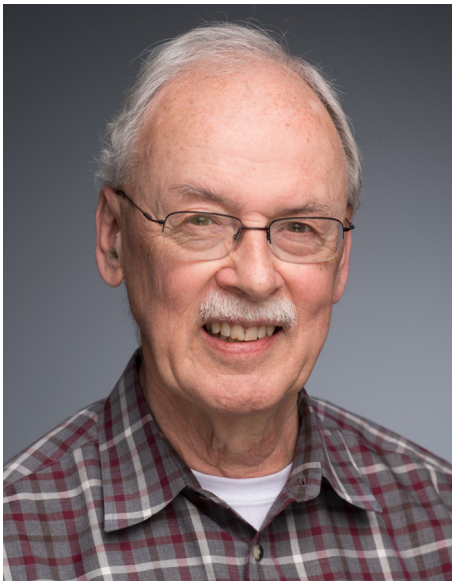
Chris Somerville



How did you spend your career?

I completed a PhD in 1978 at the University of Alberta, working on the regulation of amino acid biosynthesis in *E. coli*. As I was finishing my thesis, my adviser received a gift of EcoRI from Howard Goodman, and we started playing around with DNA. My wife, Shauna, who had just finished an MS in plant breeding, and I formed the idea that genetic engineering was going to become possible in plants, and so during a several-month sojourn in Paris at the Pierre and Marie Curie Institute, we settled on Arabidopsis as a promising model plant for molecular genetics. In the beautiful library of that institute, we also learned of Bill Ogren's ideas that plant productivity might be improved by suppressing photorespiration, so we joined his lab at the University of Illinois, where I did a postdoc and Shauna did her PhD. We isolated a series of mutants of Arabidopsis with mutations in genes for enzymes associated with the photorespiratory pathway that proved useful in resolving some issues associated with the mechanisms of photorespiration and CO_2 fixation.

In 1981 we accepted positions at the University of Alberta but found it difficult to obtain the resources needed to develop a robust research program. So in 1982 we moved to the DOE Plant Research Laboratory in East Lansing, Michigan. I was appointed as an associate professor and, after a short delay, Shauna was appointed assistant professor of plant pathology. She was initially discouraged by her department from continuing to work with Arabidopsis, because it was a useless weed. But after she obtained tenure, she was able to return to work with Arabidopsis.

Our big idea in those days was to try and encourage widespread use of Arabidopsis as a model system. So rather than continue working on photorespiration and photosynthesis, we started exploring the use of Arabidopsis for dissecting other areas of plant biology, with a view to seeding a bunch of topics that we saw as "demonstration projects." We had the idea that our students and postdocs would go on to found labs working on Arabidopsis and we would create a community. We could not have guessed at that time how large the community would eventually grow to be. Shauna found mutants and ecotypes with altered pathogen responses, and I initially dabbled in various topics such as mutants with altered responses to phytohormones, herbicide resistance, starch synthesis, lipid synthesis, cell wall synthesis, and other things. The topics my group worked on were primarily related to problems in biochemistry and the use of plants as sources of renewable materials.

In 1994 we moved to the Carnegie Institution at Stanford, where I was the director and Shauna was a staff scientist. During the very enjoyable years at Carnegie, I was engaged in advancing the application of genomics through activities such as helping coordinate the international group that sequenced the Arabidopsis genome and setting up The Arabidopsis Information Resource with my former student Sue Rhee. At the same time, the ecologists in the institute educated me about climate change, which led to me proposing a new Carnegie Institute dedicated to research in global ecology. My concerns about climate change eventually led Shauna and me to move to the University of California (UC), Berkeley, in 2008, where I cofounded the Energy Biosciences Institute with a $350 million grant from British Petroleum and also participated in writing, with Jay Keasling and several colleagues, the $125 million DOE grant that supported the Joint BioEnergy Institute. I retired from UC Berkeley in 2016 and took a position as a science program officer in a philanthropy funded by Facebook cofounder Dustin Moskovitz and his wife Cari Tuna.

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What do you consider to be your most important contributions to plant science?

I think that our efforts to develop Arabidopsis as a model system for molecular genetics were quite impactful. It was a team effort that initially included Shauna, me, Elliot Meyerowitz, Maarten Koornneef, and Dave Meinke, but it gained steam when people like Fred Ausubel, Dick Flavell, Mary Clutter, Machi Dilworth, Jeff Schell and Marc Van Montagu, Jerry Fink, Ron Davis, Howard Goodman, and many others adopted and supported the idea.

My group published a lot of research articles, but my favorite was probably a *Science* paper with my postdocs Pierre Broun and John Shanklin in which we were able to convert a fatty acid desaturase into a hydroxylase by introducing four mutations. The work built on a series of earlier papers from my group that included a collaboration with Brian Fox and Eckard Münck in which we used Mössbauer spectroscopy of a desaturase that we had cloned and functionally expressed in media with saturating Fe^{57} to work out a probable reaction mechanism for desaturases. On the basis of that work, we hypothesized how a single reaction center could be used to make double bonds, hydroxyls, epoxides, and alkynes. The work provided a theoretical mechanistic explanation for much of the chemical diversity of fatty acids found in nature. I particularly liked that paper because of the broad implications, but also because we proceeded from hypothesis formulation to a test that supported the theory and made predictions.

However, looking back on my years as a professor, what I have found most satisfying has not been any particular paper. Rather, it has been the privilege of spending my career in the company of bright, curious, honest scientists and watching my students and postdocs go on to their own careers in science.

When did you become a member of ASPP/ASPB?

I became a member of ASPP in 1982 at the suggestion of my postdoc mentor, Bill Ogren, who advised me that the Society was a force for good that I should support. Also, in those days membership resulted in home delivery of *Plant Physiology,* thereby reducing the amount of time I had to devote to the library at the expense of time in the lab. In the succeeding years I was involved with some of the committees of the Society, such as serving on editorial boards and chairing the Publications Committee during the eventful years when we arranged a change in the editor-in-chief of *Plant Physiology* for the first time in about 30 years and founded *The Plant Cell* in the face of protests from loyal members who did not see any value in publishing a second journal.

How did the Society impact your career, and what motivated you to become a Founding Member of the Legacy Society?

I think of scientific research and teaching as highly social activities in which a group of people who share a passion for knowledge collaborate at the societal level to maintain, expand, and transmit knowledge. It is somewhat remarkable how weakly this activity is organized. There is no organized global or national bureaucracy that sets standards or goals for scientific activity. And yet all scientists in the world share a common understanding and practices and goals and standards. I think that one of the forces responsible for this unique human activity is scientific societies such as ASPB, which provide virtual and physical meeting places for scientists to share knowledge. In particular, I consider the journals of the Society to be the bedrock of knowledge in plant biology and deeply appreciate the fact that many papers published many decades ago continue to be cited as definitive source of knowledge. I think the role of ASPB as a source of knowledge is even more crucial now that the Internet has become a bottomless source of disinformation and misinformation. Mostly, the Society supported my career by providing a high-quality venue for papers from my group, even when the topics were unfashionable at that moment. ASPB also offered me many opportunities to present my group's work to the community and to get to know people from many different subfields. I received several awards from the Society, which I very much appreciate and which may have helped catalyze some of the opportunities to contribute to scientific leadership that I enjoyed.

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

One of my nieces is currently finishing a PhD in plant biology, so I could write a book based on the many discussions we have had during her education. Creating a career as a scientist is very complex and involves a lot of difficult decisions—particularly about which topics to study, how to assess when to abandon a line of research, how to manage competition and collaboration, and countless other issues. Additionally, the amount of training required for uncertain employment prospects at an unknown location and a modest income requires quite a lot of self-confidence or single-mindedness.

Perhaps the best advice I gave to many of the postdocs and students who worked in my group was to not define themselves in terms of whatever problem they worked on, but rather to think of themselves as problem solvers. Thus, not a "lipid biochemist" or "pathologist" but simply a curious problem solver. This reflects my own experience. I started working in bacterial genetics, then worked in plant molecular genetics and biochemistry, then led a renewable energy institute that had a big chemistry and chemical engineering theme, and now I allocate funds for biomedical research. I have enjoyed the continual learning and found the various transitions to be challenging and enjoyable in equal measures.

Academic Family Tree
https://academictree.org/plantbio/tree.php?pid=76913