Scientists

Jack of All Trades

Jeff Doyle

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Courtesy of Jane Leclere Doyle.

Jeff Doyle's pioneering work in the field of plant phylogenetic systematics caught our attention. Curious to know more, *BioTechniques* contacted him to find out about the ambitions, character, and motivations that led to his success.

What are your most important goals and how are you working towards achieving them?

Systematics is the scientific study of the kinds and diversity of organisms and any and all relationships among them. My major interest is in polyploidy in the legume family. It is common in plants to have hybridization followed by genome doubling, which poses an opportunity for variation and also a regulatory challenge for the plant, as it will need to silence some of the genes to be effective and may need to enhance the expression of one copy over another. We really are just learning how all of that happens.

As a systematist, I'm a "jack of all trades." I need to be a bit of an ecologist, molecular biologist, geneticist, and so on. In my lab right now, we are doing metabolomics, proteomics, physiology, molecular biology and genomics, morphological work, population genetics, computational biology, and reconstructing evolutionary patterns and relationships. Other fields require a lot of interdisciplinary knowledge as well, but systematics is particularly diverse.

Was there a pivotal event that led to your current research focus?

In my fourth year of graduate school at the University of Indiana, my mentor, Dr. Charles Heiser, Jr., forced me to take a plant molecular biology course that met all day on Saturday, with extra sessions on Sundays and evenings during the week. The class schedule that semester was awful, but I learned how to isolate DNA, run gels, and perform several other techniques that I hadn't learned during my training as a systematist. More importantly, the class prepared me for a postdoctoral position with Roger Beachy at Washington University, where I studied DNA-based systematics.

At the time, there was no tradition for postdoctoral training in the systematics field. And because of a compartmentalization of organismal and molecular departments and labs, there was little opportunity for an organismal biologist to be trained in molecular techniques. However, I was fortunate to get a postdoc that allowed me to do that.

What was the academic environment like for a molecular systematist at the beginning of your career?

At first, I was unsuccessful in my search for a job. A career in plant molecular systematics simply did not exist at that time and there was hostility and suspicion between molecular and organismal biologists that grew as resources diverted from traditional programs to molecular biology.

With the advent of DNA-based systematics, there came a sentiment that those who were working on molecular data were traitors. During one of my interview seminars, a well-established senior faculty member in systematics stormed out and declared to the search committee chair that he would resign if they hired me because I was not a real systematist.

Meanwhile, faculty members at Cornell had the foresight to understand that molecular biology was becoming an important field in which they needed active research if the university was going to remain a leader in biology. This realization led faculty members to propose plant molecular biology positions in four different departments. A chemical systematist, Dave Young, argued for a plant molecular biologist in his department, and I secured that position. My position was the first hybrid in the country, if not the world, between DNA-based molecular biology and traditional plant systematics.

What was your biggest professional obstacle?

For every over-reaction there is an equal and opposite over-reaction! Right on the heels of this resistance to accepting molecular phylogenetic approaches, the pendulum swung. There came an era of wishful thinking that DNA was a panacea, or silver bullet, that solved all problems in phylogenetics.

Systematists then believed that you could figure out the relationships between species by looking at the relationships between the genes. This is fundamental to systematics, but in 1984 when I arrived at Cornell, population biologists were just realizing that relationships between genes might not be directly transferable to relationships between species. A dramatic example is in the case of horizontal gene transfer, where a gene from a fungus is incorporated into a plant genome. If you sequence that gene, you infer that the plant might be more closely related to a fungus than to another plant. So gene trees and species trees can be different from one another.

I wrote an article reviewing these ideas from the population genetics literature (Doyle, J. J. 1992. Gene trees and species trees: molecular systematics as one-character taxonomy. Syst. Bot. *17*:144-163). It was held up a long time by some pretty biased reviewers—good scientists, but stubbornly unwilling to concede that there were limitations to what molecular data could do. It took two years after submission for that paper to get published. Resistance to change comes because, as scientists, we like to believe that after all of our work, our answer is the right answer.

Interviewed by Kristie Nybo, Ph.D.

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