

SSZ 1970–1989: A View of the Years of Conflict

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May you live in interesting times

(ancient Chinese curse)

I entered the phylogenetic scheme of things as a graduate student at The Ohio State University in 1975. I stumbled over Hennig's book (1966) the first year of my OSU graduate career and by checking the references under Hennig in the *Scincoc Citation Index* I discovered the journal *Systematic Zoology*. It was as if I had fallen into another world, quite like Alice when she followed the rabbit down the rabbit hole under the hedge. "In another moment down went Alice after it, never once considering how in the world she was to get out again" (Gardner, 1960). Indeed the quiet and ordered world of botany gave way to the rough and tumble world of systematic zoology in the mid-1970s and I have never quite gotten out of it. In 1976 I wrote a number of people who were publishing in *Systematic Zoology* and asked them for advice and reprints. The one that wrote back was Don Rosen from the American Museum of Natural History (AMNH), the center of the cladistic movement in the United States. He invited me to visit and, for me, it was much like Alice's tea party; I met Don Rosen, Gary Nelson, Norm Platnick, Toby Schuh, Steve Farris, Niles Eldredge, Mary Micevich, and other members of the staff in the coffee/tea area of the Museum—a mad and fascinating group if ever there was one! I wanted to join. This tea party was followed by the AMNH Systematics Discussion Group and my first exposure to the famous New York Rules that governed or, more accurately, did not govern the proceedings.

At that time one could gain a perspective of phylogenetic systematics by first reading Hennig (1966) and then simply going to *Systematic Zoology*, beginning with Kluge and Farris (1969) and Nelson (1969) and reading through to the present. The articles, and even more so the points-of-view, laid bare the philosophy behind the phylogeny and classification of organisms; it was all there in

the journal. Before 1969, the discussion centered on the differences between traditional taxonomy, evolutionary systematics, and numerical taxonomy (NT, or phenetics), with supporters of this last approach claiming to be the only ones with an objective method. However, the heyday of NT as an indicator of phylogeny did not last. At first, Sokal and his colleagues had criticized the evolutionary systematists and traditional taxonomists and pretty much had the quantitative field to themselves. But soon there was a new kid on the block; the cladists had arrived. Both the evolutionary systematists and the cladists found NT to be less than useful when it came to generating phylogenetic hypotheses. That did not mean, of course, that the methods of Hennig were accepted by the evolutionary systematists or the traditional taxonomists. In fact, Hennig's phylogenetic systematic methods were dismissed by Mayr (1965), Sokal (1967), Blackwelder (1977), and just about everyone else; the fight was on for recognition. As a graduate student, I found the logic of Hennig combined with Popper and then Croizat very appealing and I jumped head first into the fray, loving every minute of it. Synapomorphy, monophyly, and parsimony were the watchwords, and we promoted them relentlessly in papers and symposia (see Hull, 1988) and argued about them passionately at the drop of a hat. In the mid 1970s, thanks largely to the efforts of Nelson, quite a number of zoologists around the world were practicing the methods of Hennig, whereas only a few other botanists were involved in the cladistic debate: Chris Humphries in the U.K. and Kåre Bremer and Hans-Erik Wanntorp in Sweden. The four of us endeavored to convince botanists around the world that this was the best way to produce phylogenies.

Since 1977, I have attended all but two of the annual meetings of the Society of Systematic Zoology/Society of Systematic Biologists (SSZ/SSB). During the years covered by this essay, the meetings were not well

attended, because every year except one they were held with the American Society of Zoologists between Christmas and New Year's Day. The council meetings were lively, as the battles raged over who would be elected a Corresponding Member and who would be Editor, and the speakers in the symposia were all of the primary players in the debates. It was stimulating to meet and talk with them. By and large, however, the membership did not attend, and although the small number of members at the meetings did not slow down the debate (the important arguments were published in the journal), some of us wished for greater numbers of participants. The battleground constantly shifted as control of the journal (editorship) moved from cladist (Nelson and Eldredge, 1974–76; Schuh, 1977–79; Smith, 1980–82) to numerical taxonomist (Schnell, 1983–86; Shipp, 1987–89) and back (Hillis to the present). Toward the end, most of us went back to our taxonomy feeling the war had been won; argument was the only thing keeping the NT movement alive and if we just ignored it, it would go away. However, the battle raged until the late 1980s, with the last NT editor leaving office in 1989.

David Hull was our own personal philosopher through these years, attending most meetings and adding much to the discussions. He also served as President of SSZ (1984–85). In 1983 he wrote a paper on the first 31 years of *Systematic Zoology*; later, he published a book about the history and philosophy of the cladistic movement as an example of how the scientific process really works (Hull, 1988). True to form, the book caused a lot of controversy, praised by some cladists and denounced by others.

Although the attendance at the annual meetings was sparse, several special meetings were held that played important roles in the development of phylogenetic theory. From my perspective, four in particular played a crucial role. Three were held in quick succession between May 1979 and October 1980; the fourth followed in 1984. First, in May 1979, was the symposium held at the AMNH, "Vicariance Biogeography: A Critique." The symposium was moderated and reviewed by V. Ferris (1980), and the proceedings were published (Nelson and Rosen, 1981). This was the first meeting that contained proponents of cladistics from around

the world and, because no NT people were in attendance, the debate focused on "what can we do with cladistics?" rather than "should we do cladistics?" Of course traditional biogeographers were present, but there were a large number of cladists in the program and in the audience, and the informal meetings were powerful in that new ideas were voiced and alliances were formed. In October 1979, the 13th Annual Numerical Taxonomy Conference was held at the Museum of Comparative Zoology (MCZ), Harvard University (Mitter, 1980). This proved to be the last great public clash between the NT supporters and the cladists, led by Sokal and Farris, respectively. However, the preceding meeting at the AMNH had given us cladists a taste of what we could do if we were free from the same old arguments and, although the arguments at the MCZ raged long and loud, no new ground was being covered. To some of us it seemed the time had arrived to gather a group of like-minded Hennigians and plan our own meeting. Accordingly, in October 1980, more than 70 systematists from Great Britain, Sweden, Canada, and the United States gathered at the University of Kansas, at the invitation of Ed Wiley, to inaugurate the Willi Hennig Society. The meeting was reviewed by Schuh (1981), and the proceedings were published by Funk and Brooks (1981). Many of us who participated in the first meeting, however, felt that the society would last only a few years, until the principles of phylogenetics became widely accepted, by which time the society would no longer be necessary and would simply fade from existence. Of course, that is not what happened.

Near the end of the NT meeting in 1979 during his presentation, David Hull said something like the following: "The cladists don't know it yet, but they have won; the interesting thing will be to see how they react when something new comes along." Of course, what happened is that as soon as the pressure from outside opposition was lifted and we were able to concentrate on developing the field of phylogenetic systematics, we immediately escalated the arguments we had begun to have among ourselves. By the time of the fourth Hennig Meeting, held in 1984 at the British Museum in London, serious trouble was brewing. The discord at this meeting resulted in several members of the Hennig Society

Council either being voted off the council or resigning. One of the issues was control of the Society: A small but powerful group was insisting on maintaining a limited group of individuals in the inner circle, while others wanted to broaden the membership. But the division was deeper than that. Some of these issues are discussed in the first volume of the journal *Cladistics* (1985). The two groups have been called “phylogeneticists” and “pattern cladists” by both sides of the argument (Brooks and Wiley, 1985; Kluge, 1985). However, that does not really reflect the positions because the former group could also be called the “evolutionary cladists” because they wanted to use cladograms as phylogenetic trees to study evolution. Someone once described the two groups to me as the “converters” and the “slayers of infidels” and I will leave the decision to you as to which has had the most influence in the field of systematics. The ultimate result was that approximately 5 years after it was founded, a substantial number of cladists left the Hennig Society meetings and began to work with SSZ to move the date of the annual meeting to facilitate broader participation by the members. But that, as they say, is another story.

I feel fortunate to have come into the fray when I did. To be able to participate in the birth of a new idea, to try to find ways to get people to understand why it is important, and to push it to its limits was an exhilarating experience. To learn that nothing is perfect and that things continually change—sometimes in directions one does not like—has been equally valuable. Cer-

tainly the events during these years were rewarding as well as heartbreaking.

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