

Foundations of Systematics and Biogeography

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Foreword by Gareth Nelson

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Dedicated to

Agnes Arber (1879–1960)

Adolf Naef (1883–1949)

Léon Croizat (1894–1982)

Lars Brundin (1907–1993)

Rainer Zangerl (1912–2004)

Colin Patterson (1933–1998)

Ronald H. Brady (1937–2003)

Foreword

“But where shall wisdom be found? and where is the place of understanding?”

Job 28: 12

Where, indeed? Today in systematics and biogeography, DNA is revered as the source of all. One reads for example of the “unlovable mass of nucleotide sequence characters that are the foundation of virtually all well-supported phylogenetic trees” (Palmer et al. 2004:1443); and “Much of the improved understanding derived from new genetic data and allows us to date important evolutionary events and, in some cases, to trace the actual geographic routes travelled by early peoples over the earth” (Orr 2006:18). Such assessment says nothing of the long history of human effort in systematics and biogeography, as if that were now rendered irrelevant by modern biology and its techniques of reading DNA sequences and of their computer-assisted phylogenetic analysis.

Two generations ago palaeontology was similarly revered. Then one could read for example that for mammals “Their fossil record is unequalled and allows an almost magical view into the past” (Darlington 1957:320; reaffirmed by Briggs 1974:249). Today’s attitude towards DNA is much the same except that there is no “almost” about it. A lesson from the past, a sense of proportion widely overlooked, is Blackwelder’s (1977:115) dictum that “New types of data are potentially of great importance, but they do not replace other types except in problem cases.” His perspective grew from consideration of overblown claims offered for the “new kinds of data” of his time: chromosomes, behaviour, serology, genetics, a list that today would be augmented by organelles, membranes, nucleic and amino acids, genomics, proteomics, etc. Even so, the abiding reality remains: “there is no such thing as magic.” And, alas, to Job’s queries there are no easy answers.

In 1813 AP de Candolle observed (p. 68) that in earlier times “the plant that one botanist considered related to some other would later be far removed from it by another botanist, with neither opinion capable of being proven either true or false.” For this dilemma he saw the remedy to be “the natural method,” which took all characters into consideration and relied on character congruence for support of one opinion and refutation of another. His view prevails to the present, but its focus was improved by Hennig’s (1949) distinction between primitive and advanced

characters – his plesio- and apomorphies – that is the basis of the modern discussion of cladistics.

The present volume broadens the discussion by incorporating the pre-Hennigian German literature from Goethe, Haeckel, Naef et al. – what in the anglophonic world is usually dismissed as the romanticism of “German idealistic morphology” (Levit and Meister 2006). Through the ageless eyes of the “modern synthesis” the broadening must seem to approach Marx’s (1852) apotheosis of The Past: “The tradition of all dead generations weighs like a nightmare on the brains of the living” – *Die Tradition aller toten Geschlechter lastet wie ein Alp auf dem Gehirne der Lebenden*.

Nightmares notwithstanding, nothing for long, it seems, can safely be ignored.

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Prologue

The views expressed in this book are an amalgamation of the works of several 18th, 19th- and 20th-century comparative biologists, notably Johann Wolfgang von Goethe, Louis Agassiz, Geoffroy St. Hilaire, Richard Owen, Adolf Naef, Rainer Zangerl, and Gareth Nelson. In one sense this book is simply an anthology of their studies on *homology* and *classification*, as expounded in such classic studies as *Die Metamorphose der Pflanzen* (Goethe 1790), *Essay on Classification* (Agassiz 1857, 1859) *Idealistische Morphologie* (Naef 1919), and *Systematics and Biogeography: Cladistics and Vicariance* (Nelson & Platnick 1981), ideas that form the basis of a comparative biology for the 21st century.

In another sense, the intention behind writing this book was to explode some myths currently part of biology and reassert—and restate—the principal aims of comparative biology, especially in relation to evolutionary studies. The prevailing attitude of many systematists and biogeographers has led to conflicting ideas being combined under an apparent evolutionary synthesis (Mayr & Provine 1980). In order to alert prospective students of comparative biology to the dangers and pitfalls involved in systematics and biogeography, this book intends to provide three principal messages:

1. Biological classifications and explanatory mechanisms are separate entities. Classifications form either artificial or natural groups, the latter being discovered through *homology* and *monophyly*, the former simply assertions, imposed rather than discovered.
2. Most, if not all, concepts of homology originated prior to the work of Charles Darwin, a fact that emphasises the rich, varied, and valuable work of 18th to 19th-century anatomists—the history of its development a subject still not exhausted.
3. The underlying concept for all of comparative biology is *relationship*—neither “similarity” nor “genealogical hypotheses of descent” are sufficient.

These three messages echo the work of the last three centuries of comparative anatomy, most recently expressed in Gareth Nelson and Norman Platnick’s book *Systematics and Biogeography* (1981), now over 25 years old. *Systematics and Biogeography* was the first book of the 20th century to provide a historical account of the ideas behind the emerging cladistic revolution.

Nelson & Platnick's book was not the only attempt at a summary of methods for classifying organisms. Two very different books appeared that covered, in part at least, the history of comparative biology: *Phylogenetic patterns and the evolutionary process* (Eldredge & Cracraft 1980) and *Phylogenetic Systematics* (Wiley 1981). Both of these books interpret what amounts to the same history discussed in *Systematics and Biogeography* but adopt a mechanistic approach rather than what might be called the descriptive or pattern approach.

The appearance of these three books, all stemming from the same institution (the American Museum of Natural History), at the same time, interpreting the same history in three different ways, implies a conflict of interpretation—and some viewed the immediate aftermath as a "war" between methodologies, documented by historians biased towards a particular viewpoint—that of mechanisms (see Hull 1988). The "war" of interpretation would have made far more sense if all sides had noticed that each was influenced by a different part of that same history—interpretation did not in any fundamental sense differ at the level of methodology but at the level of intention.

Posing the question, "Why study comparative biology?" yields two very different answers, if any positive answer is given at all. One sees virtue in classifying organisms (Nelson & Platnick 1981), the other sees virtue in explaining that classification (Eldredge & Cracraft 1980). Oddly, it is a third group, those who wished to mingle classification and explanation, where confusion has been most mischievous, exemplified by Wiley (1981)—it is, thus, perhaps no coincidence that Wiley intended his book to be regarded as an update of one foisted on a previous generation, one buried in the "modern synthesis" as an explanation for organismic diversity (Mayr 1969). Only after consideration of the significant incompatibility—or the fundamental clash between classification and explanation—can the enterprise of classification begin to progress. At present, it has stagnated.

The "war" between explanation and classification is rarely discussed, most preferring the more conventional world of explanation—possibly because there is a belief that explanation conveys greater meaning than classification.

We felt that by recording some history, along with some interpretation, we were able to understand the numerous—and reoccurring—disagreements between comparative biologists: A history that clearly separates the differing intentions of its practitioners does not exist. One detailed history of the more recent period does exist: David Hull's *Science as a Process* (Hull 1988; but also see Craw 1992). Even Hull's title suggests a viewpoint from the outset, presenting a "process-orientated" reading of history. We present the missing version—the version based on pattern and on classification.

David M. Williams and Malte C. Ebach,
London, Paris & Berlin, November 2006

Acknowledgments

The ideas we explore in this book are, like most dynamic and exciting fields of enquiry, continually changing and developing. Since the book's inception, some six years ago, biogeography and systematics have both undergone dramatic changes, especially with the accumulation of massive amounts of molecular data and the plethora of new methodologies for its analysis. The entire scientific landscape has altered since the Cladistic revolution began some 40 years ago, starting in the mid- to late 1960s. We felt it was necessary to examine not just the immediate past of our subject but to explore some of the lost avenues of research in its more distant past, with the desire to show that data are not now necessarily the major source of problem solving, many conceptual issues are outstanding. This volume represents the combined work of our investigations and insights into the foundations of systematics and biogeography over a working partnership spanning 6 years. The content of this book summarises several key works, most notably, Williams (2004, 2006), Ebach (2005) and Williams and Ebach (2004, 2006) (with permission).

The *Foundations of Systematics and Biogeography* would not have been possible without valued, and at times heated, debates with many of our colleagues. While we are grateful to them all for putting up with our constant quest for their input and tolerance of our insistence, we would especially like to thank Mark Carine, Peter Forey, Juan J. Morrone, Gary Nelson, Lynne R. Parenti, and Robert Scotland. We would also like to thank Bob Press, Johannes Vogel, and René Zaragüeta for assisting with these preoccupations when, perhaps, we should have been dealing with more pressing matters.

Finally, and naturally, we wish to thank Lorraine Williams and Caitlin Hulcup, who had to witness not only the birth and subsequent growing pains involved in the writing of this book but also its rather prolonged adolescence.

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