

BOOK REVIEW

Sakai, K. 2006. Upogebiidae of the world (Decapoda, Thalassinidea). *Crustaceana Monographs* 6, i-ix, 185 pp., 23 textfigs. Koninklijke Brill, NV, Leiden, The Netherlands, ISBN-13: 978 90 04 15150 8, hardcover, Euro98/ U.S.\$132.

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Katsushi Sakai began publishing on the taxonomy of the decapod infraorder Thalassinidea in 1962 and has published almost 70 papers, large and small, on this group of burrowing shrimps. Sakai must be acknowledged as a major contributor to the field that includes several significant decapod researchers, Michèle de Saint Laurent and Nguyen Ngoc-Ho in France, Brian Kensley, Ray Manning, Austin Williams and Darryl Felder in USA, and Peter Dworschak in Austria. The volume and significance of his work and the quality of his species descriptions and illustrations are noteworthy. After 27 years of work, Sakai embarked on a series of synthetic works - a type of project many senior taxonomists would be well advised to consider for their special taxon. Notable are Sakai's timely syntheses of Axiidae (Sakai and de Saint Laurent, 1989), and Callianassidae and related families (Sakai, 1999, 2005). Now it's the turn of Upogebiidae and we are told Axiidae are in the pipeline again.

In this, the second of his *Crustaceana Monographs*, Sakai reviews the world Upogebiidae. The 1-page introduction is mostly a summary of the contents. In the next 150 pages, the family, two new subfamilies and 11 genera are diagnosed. Keys are presented to genera (two new) and to species within genera, the last separated into species of different regions. Species are listed alphabetically by region with comprehensive synonymies (numbering hundreds of papers for *Upogebia pusilla*), type locality and distribution. For some species, diagnoses, remarks and new illustrations are provided. The reference list totals about 1300 titles. The 'Taxonomic index' is to species in various combinations, i.e., genus (with or without subgenus) first, making it impossible to search by species name.

The value of compiling species lists, and presenting keys and complete synonymies cannot be disputed. I am aware of only four species omitted from his compilation: *Upogebia quddusiae* Tirmizi & Ghani 1978, *U. toralae* Williams & Hernández-Aguilera, 1998, *U. australis* Thatje & Gerdes, 2000, and *Gebiacantha albenagai* Ngoc-Ho, 2005.

Sakai's publications have generated more controversy than the discipline of crustacean taxonomy usually attracts. His classificatory decisions are often disputed or simply ignored. For example, a dispute about the differentiation of species of *Nihonotrypaea* in Japan remains unresolved (See Editorial note following Sakai, 2004a). His views on a new higher classification of Thalassinidea remain untested and

contradict more widely accepted classifications (Sakai, 2004b). And his synonymy of many widely accepted genera of Callianassidae with a catch-all *Callianassa* (Sakai, 2005) has been greeted with dismay (Dworschak, 2007). This volume continues that pattern. Already Ngoc-Ho (in press) has refuted many of the decisions in Sakai's work by detailed counter-argument over generic synonymies and species definitions. These are not criticisms or details that I will repeat but instead consider how such differences of opinion arise.

In this work Sakai divides Upogebiidae into two subfamilies, Upogebiinae and the new Neogebiculinae [which he attributes erroneously to Sakai (1982)]. His justification is "The Upogebiidae can be divided into two subfamilies ... by the form of the uropods. In the Neogebiculinae, the uropods are narrow and leaf-like, whereas in the Upogebiinae they are broadly foliaceous." In the first few pages of this volume, Sakai illustrates *Paragebia leptomorpha* (Neogebiculinae) and *Mantisgebia vonvaupeli* (Upogebiinae), whose uropods are equally leaf-like. Uropod shape is an arbitrary choice of character and, worse, the distinction appears not to hold up. Strahl's (1862) division of *Upogebia* into those with chelate (*Upogebia*) and subchelate (*Calliadne*) first pereopods was a similar arbitrary division found recently to be unsustainable in the light of many other characters. It is often possible to divide a taxon into two subtaxa on the basis of one character and different subtaxa on the basis of another. Why choose uropod shape when chelation or rostrum shape or any other convenient character could be equally valid? It is one of the principle aims of phylogenetics that reasoned choices between options be made using defined criteria, parsimony or maximum likelihood or whatever. No such criterion here and the subfamilies are best ignored (whether you are a cladist or not) until a more rigorous analysis is done.

New genera are similarly diagnosed without much justification: *Arabigebicula* because "its morphology is very distinct from that of other species of the family Upogebiidae", a statement followed by remarks on similarities to other genera. No morphological feature is mentioned and I can't spot anything "very distinct"! *Mantisgebia vonvaupeli* is type species of a new genus because of its an unusual telson and rostrum but other species remaining in *Upogebia* are unusual in other ways. Why was this species picked out and not others? While erecting new genera on weak grounds as he did with Callianassidae (Sakai, 2005), Sakai synonymises others much better defined. He doubts the validity of *Gebiacantha* and *Austinogebia* because "... most of the defining characters overlap ...". He proceeds character by character to demolish the unity of the genera by pointing out exceptions. Such an approach denies convergence between species in these and other genera, and that the two genera may have diverged from a common ancestor. Justification of these two genera has been much more convincingly argued

by Ngoc-Ho (2001, in press) than for the new genera erected by Sakai in this review.

Sakai tends to be a lumpner at the species level too. His synonymy of *U. octoceras australiensis* with *U. bowerbankii*, *U. foresti* with *U. kempii*, and *U. rupicola* with *U. carinicauda* are all disputed by Ngoc-Ho (in press). And the ever-contentious *U. darwinii* gets another airing over 13 pages.

As it stands, Upogebiidae comprises about 30 species in ten or more small, possibly monophyletic genera plus the remaining 100 plus species in a paraphyletic or possibly polyphyletic *Upogebia*. I have no particular objection to paraphyletic taxa but the family cries out for a rigorous phylogenetic analysis and a stop to erection of new genera on the basis of possession of single character states or no explicit characters at all.

Most species are rare in collections, one or few specimens from a single locality, and the temptation to describe a new species on the basis of small differences is high. Some will view similar individuals from widely separate localities as one species (particularly from within the Indo-West Pacific region renowned for widespread species) while others will see specific differences. Thalassinidean taxonomists will continue to argue about species synonymies as long as infraspecific variability is poorly understood. These disagreements will never be solved until intra- and interspecific differences are detected in larger collections. Molecular data may provide a new insight - or it might not.

The work is to be recommended because it brings together the vast majority of what has been written about Upogebiidae since 1792. However, the reader should be aware of its uneven quality. Although the acknowledgements reveal that the text has been read by reputable crustacean taxonomists, sections of impenetrable text and misreporting have been allowed through. A thorough critical review by an expert on Thalassinidea would have detected errors and differences of opinion. Differences of opinion will always remain but the editor might have insisted on more clearly justified argument for some of the taxonomic acts proposed.

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