Southern California Association of
Marine Invertebrate Taxonomists

July/August 2004

SCAMIT Newsletter

Vol. 23, No. 3&4

SUBJECT:	**Crustacea Part I - Ampeliscids, Oedicerotids Phoxocephalids & Lysianassids**
GUEST SPEAKER:	**Meeting Lead: Lisa Haney**
DATE:	**8 November 2004**
TIME:	**9:30 a.m. to 3:30 p. m.**
LOCATION:	**Los Angeles Co Museum of Natural History 900 Exposition Blvd.**

***Cladonema*?**
from the CSD display tank
approx 2-3 mm

Due to the fact that the July meeting was canceled (the enormous work-load imposed by the B'03 project was taking its toll) and obviously there are no minutes for that month, this volume is a combined July/August issue.

AUGUST MINUTES

The day began with some general discussion. It was remarked by a number of members that there has been a lot of press coverage of the worm *Osedax* spp. found by MBARI. For more info, see their web site at;

http://www.mbari.org/news/news_releases/2004/whalefall.html

As for upcoming meetings, Don mentioned the meeting of AMPHIPODOLOGISTS will be in Cork, Ireland at the National University of Ireland on July 24-27, 2005.

We then moved on to the topic of the day. Our speaker, Kimo Morris, a PhD candidate at UCLA gave a great talk entitled: Oceanic Fronts: A Meeting Place. He is currently studying the affect of oceanic fronts on zooplankton populations.

He began his talk with a brief history of two influential people in the field of early zooplankton research. Ernst Haeckel, well known to most biologists for his work on phylogeny, began as a zooplankton researcher. He developed very careful techniques for capturing delicate pelagic organisms, such as radiolarians and jellyfish, for observation and illustration. He used small boats and fine meshed nets. The delicacy of his methods are exemplified by his illustrations published in "Kunstformen der Natur" (Art Forms of Nature) 1899-1904.

In contrast to Haeckel, Victor Hensen (1823-1924) was a professor of physiology at the University of Kiel in Germany with little previous experience with zooplankton. Hired by the Kiel Commission on north Atlantic fisheries, Hensen set out to describe and quantify plankton diversity and spatial arrangement. Hensen put forward the idea of using big nets towed by large vessels in extensive arrays throughout the ocean. However, since this method tends to count only the hardiest species, e.g., crustacea, it leads to the under-reporting of the more delicate groups such as gelatinous zooplankton. Additionally, Hensen averaged his plankton volumes over very large distances, which gave the false impression that plankton are spread evenly throughout the ocean in low abundances. Haeckel intensely criticized Hensen's approach, however in the end, Hensen won. Well into the 1960s, Hensen's view persisted, where the pelagic realm was seen as a homogenous mix of low-density plankton with jellyfish as insignificant contributors. To this day, Hensen's techniques are still included in plankton manuals as "the" standard method of quantifying plankton.

Jump forward to the 1970's. William Hamner, an ornithologist by training, began to ask why there were no in situ observations of zooplankton. Hamner noted that gelatinous zooplankton were often considered a nuisance when they were captured in the large plankton nets and usually discarded. In response, Hamner and others pioneered blue water diving techniques resulting in the first rigorous observations and quantification of gelatinous zooplankton.

Kimo's contention is that zooplankton does not occur homogenously across the open ocean. Everything in the ocean is patchily distributed. One of the areas in the ocean where they appear to concentrate is along water mass boundaries or fronts. This also results in the concentration of the animals that feed on them. Kimo has looked at three different types of fronts in his research.

The first is an area of upwelling in Monterey Bay. There, the colder water from upwelling meets the relative warmer water mass circulating, counterclockwise, within the bay. This boundary is predictable and can be followed with remote sensing. Using vertically towed nets Kimo has shown that the highest concentration of zooplankton is found on the boundary between the two water masses. This is confirmed with ROV observation near the surface. In the video clip he showed one can see large aggregations of *Chrysaora melanaster* oriented along the same water mass. He used an attached CTD to orient the ROV along the front.

His second investigation involved a near shore front in Santa Monica Bay. This front results from the meeting of the relatively stratified water mass offshore and the tidally mixed water near shore. The two water masses do not mix, thus forming a front. This front acts as a boundary or barrier. Kimo is looking at two things: characterizing the physical dynamics of the front, and also the larval transport across the front. Preliminary results reveal the existence and extent of a front as described above and that there appears to be a strong correlation with plankton species assemblages and the inshore and offshore water masses.

Thirdly, Kimo has been investigating short-lived, or ephemeral fronts. This is a phenomenon evidenced by slicks of calm water at the surface. These ephemeral fronts are generated, in part, by wind interacting with the ocean surface or by swirling eddies. The waters in Santa Monica Bay around the Palos Verdes peninsula were chosen for Kimo's investigations. Past researchers have noted that a number of shore birds gather at these areas feeding on fish egg aggregations. Here large aggregations of salps, dolphins, and ocean sunfish have also been observed. While his research is still incomplete, he has hypothesized that these zones concentrate zooplankton, e.g., salps, as an important food source for large animals. In the course of his study Kimo has developed modifications of Hamner's blue water diving techniques for quantifying macrozooplankton assemblages along these ephemeral fronts.
For a more information on Kimo's research visit his website at:

http://pukashell.net/kimo/ucla/

NEW LITERATURE

It has been months since I have provided information on new literature to readers. It has continued to accumulate, and I will start mining the pile now that B'03 field and lab work is virtually complete.

First on the docket is a wonderful compendium of information on the organisms associated with hermit crabs (Williams & McDermott 2004). The mollusk shells they have appropriated often form a complete biocenosis; a mini-rock on soft bottom substrates. The authors draw together on extensive literature (the initial table of relationships runs to 48 pages and lays out crab species, associated species, type of association, habitat where it occurs and source of information). Between this and the extensive bibliography (20 pp.) the authors have prepared a nice summarization, discussing both what is currently demonstrated, and what remains to be determined or proven.

A small subsection of this territory is also covered by Daly et al (2004), who describe a new sea-anemone and review the taxonomy of those anemones known to produce carcinoecia. These structures are the plastic-like artificial shells secreted by the anemone to entice it's commensal partner hermit crab not to leave in search of a larger shell. In cases where the two partners find each other early, nearly all of the "shell" occupied by the adult hermit crab may actually be faux shell secreted by the anemone. These persist for some time after the death of the two parties and can be most confusing to a conchologist contemplating the resultant empty structure. Similar shell enlargements formed by milliporine corals and bryozoans are not covered here, but are covered in the more generalized presentation of Williams & McDermott.

Another class of modification to the appearance of animals by associates is dealt with by Gillan & Dubilier (2004) and Gillan, Ribese & de Ridderv (2004). This time it is the ferruginous deposits seen on the setae and appendages of peracarid crustaceans. These are often found on local *Ampelisca* species, but those on the burrowing *Urothoe poseidonis* are described by Gillan, Ribesse & de Ridder. While they report on a protozoan epibiont too, most of the information presented dealt with a group of filamentous bacteria. These appeared to be involved in the production of the ferrous sulphate deposits which formed on the amphipod. This process probably assists in control of sulphide in the waters the animal inhabits. Similar iron deposits are also observed in bivalves (Gillan & de Ridder 1997, 2001), usually near the siphons.

In both these cases it is likely that this is a mutualist association, with the bacteria deriving benefit from living room, and exposure to enhanced water movement, while the amphipod or clam gets a boost in its ability to control exposure to toxic sulfide by bacterial enhanced (or mediated) iron deposition. Ah, the convoluted joys of commensal interactions!

Switching gears back to taxonomy Ocaña, Sánchez-Tocino, and García (2004) consider ontogenetic variability in the radulae of the opisthobranch genus *Tjamba*. Previous work with other gastropods has shown that tooth shape can be affected by wear, and that different food substrates affect these wear patterns. This alone makes taxonomic discrimination of related congeners on the basis of radular structure more risky. Once the sort of ontogenetic changes reported by the present authors are factored in, even greater caution is indicated. Minor differences in denticle count, exact plate shape and cusp length should be recorded and evaluated with a large grain of salt. Radular variability seems as common as shell variability, and is probably just as unpredictable; some species hardly ever showing significant variation in shell shape and ornamentation while others vary extensively. Additional investigations in other opisthobranch groups should be performed along the lines used here, testing for developmental differences in radular detail along the road to adulthood.

Ron Velarde brought the following paper to my attention at a recent SCAMIT meeting. It is available on-line at

http://www.mnhn.fr/publication/zoosyst/z01n3som.html

It is authoritative but incomplete, being merely a summary list of literature (and personal research results of the authors) derived status of the current supraspecific taxa within the molluscan class Scaphopoda. Steiner & Kabat (2001) don't provide the evidence and rationale for the current status (where it differs from recent contrary decisions by themselves and others), so caveat emptor. I personally would place a great deal of trust in their results and what they present here (although it necessitates major reorganization of the SCAMIT list) because the authors have contributed greatly to refining and improving the scaphopod taxonomic framework for many years. But, plenty of controversy still remains, so each reader should review and evaluate what is presented here for themselves. The authors plan a species level treatment in the future. Fortunately they give a species allocation in the current paper, but don't deal with specific level synonymy.

We often see lovely video of loliginid squid jetting around in midwater and cuttlefish hanging motionless in the hyperbenthos or motoring slowing through the lower parts of the water column. What we don't see much is non-octopoid cephalopods in association with the bottom itself. Anderson, Mather & Steele (2004) describe such behavior in our local *Rossia* species. Their observations are based on aquarium maintained specimens.

Reimer et al (2004) look at molecular evidence for speciation (or lack thereof) among a group of four described congeners in the cnidarian genus *Zoanthus*. These are often massive, clonal forms which can dominate intertidal and shallow subtidal substrates in tropical and sub-tropical areas. The authors used the mitochondrial cytochrome oxidase subunit 1 gene, used widely in molluscan molecular systematics at the species level. They examined four field morphotypes, previously considered four discrete species, and found nearly 100% correspondence in base pair sequences in three of them. The fourth differed by just over 1% of the sequence and may be a separate species. They suggest a reevaluation of the criteria used in species level taxonomy of *Zoanthus* and other zoanthids: good call!

The last article I will mention is an even more massive compendium than the first, but covering a much different subject...the selection of character states used in cladistic analysis (Jenner 2004). The author has been critically reviewing numerous aspects of cladistic methodologies as a friendly and interested practitioner, not as a critic of the enterprise per se. He presents the remarks in the current report as constructive criticism, and hopes they will be taken in that fashion. Nearly every recent cladistic analysis of metazoan phylogeny is brought into question here, however, for use of characters whose conception, definition, or coding are not fully defensible. In many cases he finds that the authors have not used a truly unbiased selection of characters for their analysis (either unintentionally or intentionally) leading to a lack of falsifyability of the results and a consequent failure to achieve the goals of this type of analysis. Everybody gets a bloody nose here. Hopefully the value of an apraisal of this type will be in forcing much more critical character evaluations in future analyses. The author would be gratified at such a result, but has undoubtedly bruised a few egos along the way to this end-point. This is not a paper to read, like a novel, from end to end. The commentary is too dense and (sadly) repetitive for that, as similar mistakes are made over and over by many different investigators.

Of course, others may rise to the defense of particular actions or take exception to the analysis presented here by Jenner. All to the good! Controversy and discussion of the issues raised should be fruitful and further improve the developing subdiscipline of character selection in cladistic phylogenetic analysis.

- D. Cadien

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