Conus Descriptions Aren't Improving

By DR. ALAN J. KOHN

SEATTLE — During the summer of 1979 I continued my long-term investigation of the identity of the species of ***Conus*** proposed during the early decades of Linnaean binominal nomenclature, especially the first decade of the nineteenth century. Prior to that time, 339 species of ***Conus*** had been described. I reviewed these in earlier publications and concluded that 123, or 40 per cent, are valid species.

Between 1801 and 1810, eight workers described 98 more species of ***Conus***. Preliminary results from my study of the type specimens of these last summer suggest that only 23 are valid species. Thus the record of the eight early nineteenth century workers was poorer than that of their eighteenth century predecessors.

Unfortunately, this is likely an omen of what is to come as the chronological study of the described species of ***Conus*** advances toward the present — an increasing proportion of newly described species will prove to be synonyms of earlier described species. In fact, it suggests strongly that virtually all species of ***Conus*** described during the past half century will prove to be synonyms of previously described species. Although there is as yet little convincing, objective, quantitative evidence for this, I believe it is likely to be true.

Why does the record get worse and not better? Why is a species described in the modern days of the twentieth century less likely to be valid than one described in the eighteenth?

It is a sad fact that the descriptions of supposedly new ***Conus*** species in recent years are no better than those of Linnaeus, Gmelin, Born, and other eighteenth century workers. With modern communication and transportation, new methods of collecting, and appreciation of the importance of populations and variations of individuals for the way animals evolve, one would expect modern descriptions to be far superior to those of the eighteenth century. Unfortunately, however, the people who describe new species of molluscs in the late twentieth century seem to have learned nothing from the shortcomings of their predecessors.

In the rest of this account I will give, and defend, my own opinions on the desirability of describing new species, especially of ***Conus***, and I will mention the requisites of an adequate description.

The first thing to be said is that the description of a new species of animal is a scientific hypothesis. The author of a new species hypothesizes that the specimens he has studied are a sample of one or more populations of interbreeding individuals that do not interbreed with any other, different species. One can never be certain that this is the case; it is not possible to test the mating predilections of every animal in the entire species. In practice, it is usually not possible to learn this about any of them! So the hypothesis can never be proven.

But do not despair. This is true of **all** hypotheses in science. They can only be disproved (by discovering a fact that is inconsistent with the hypothesis). I repeat, scientific hypotheses can not be proven, only disproven. What scientists do for a living is essentially to try to disprove hypotheses. Those that are left — that are not disproven — we accept as valid theory or laws of nature.

So the author of a new species must support his hypothesis with indirect evidence, usually that the characteristics of the specimens he has been able to study do not intergrade with the characteristics of any other known species. This type of evidence supports but cannot prove the hypothesis; remember that hypotheses can only be disproven, not proven. To be convincing to others, the author must communicate objective, preferably quantitative information to other workers.

Does this mean that the taxonomist must be a highly trained biologist?

This is a difficult question. People who are not trained biologists have described many valid (as well as invalid) species. In ***Conus*** one thinks, of course, of Reeve and Sowerby in the last century. One of the delightful and rewarding aspects of biology is that anyone with a keen eye can see something in the biological world that no one has described before. One does not need a lot of training, or knowledge of how to use complex apparatus as one does nowadays in the physical sciences. However, this does not excuse the untrained biologist from playing the game by the rules.

Anyone who makes taxonomic studies that he considers of scientific importance, such as the description of a new species, should submit his reports only to carefully refereed scientific journals, where they will be critically reviewed by competent taxonomists who are knowledgeable about modern techniques of systematic biology. This system has worked well for many years.

The referees selected by the editor review the quality of the evidence presented and judge how well it supports the hypotheses advanced. The editor's decision on whether the paper merits publication is based on these reports. The system protects the author from making statements in print that he will later regret. Descriptions of new species in publications that do not meet these standards will be widely ignored, and justifiably so.

Another problem that seems endemic to molluscs is that many people do not understand that **absolutely no honor** is associated with describing a new species. There is only responsibility — a heavy responsibility, for the author of a new species stakes his reputation on his defense of the new name as denoting a real, previously undescribed species, representing breeding populations of many individuals, outside the range of variation of **all** previously described species.

Now I have complained a good deal about the poor quality of descriptions of new ***Conus*** species that have appeared between 1758 and 1979. Most do not really permit identification, and they have been responsible for the vast proliferation of new species names, now exceeding 3,000. Why can't specimens be identified from the original descriptions and illustrations of new species?

Typically, the reason is that the author fails to provide enough information about the range of within-species variation, and he fails to tell how to distinguish this from differences between species.

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Live *Voluta festiva* Collected for Study

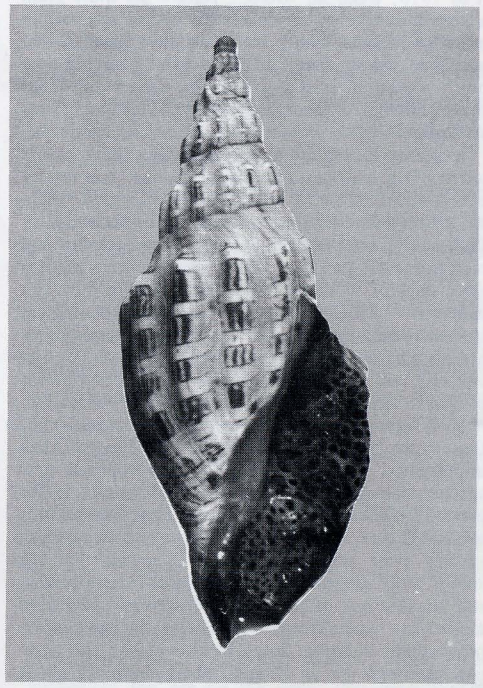

Photo: Ninomiya

By ELMER G. LEEHMAN

The well-known Japanese malacologist, Taizo Ninomiya, recently obtained three live-taken specimens of ***Voluta festiva*** Lamarck, 1811. Rare as live-taken specimens are, the really important aspect of this find is that one animal was kept alive by the skipper of the trawler that found it and has been delivered to Ninomiya in Tokyo for study.

To my knowledge, this is the first time a live ***V. festiva*** animal has been available for study.

The three specimens were trawled by a Japanese vessel in 60 meters of water, in sand and stones, in the Gulf of Aden off Somalia. Although I lack a detailed description, in a color photo the foot of the animal appears to be orange with black spotting.

The possibility that ***V. festiva*** is subject to sexual dimorphism was raised by former **HSN** editor Cliff Weaver (**HSN** Dec. 1978). At that time actual animals were not available to confirm or refute Weaver's hypothesis. Ninomiya's studies with the present specimens may provide a clearer answer.

CONUS DESCRIPTIONS

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This is an old problem; Lamarck was well aware of it in 1810, when he wrote, "Naturalists must grant a certain extent to the characters of the species, in order to include within its limits the varieties that appear to belong there. If one doesn't perceive how much variation exists, one is in danger of either overestimating or underestimating the extent of this variation."

It seems necessary to re-emphasize these words today. The bad descriptions of ***Conus*** species by early authors, who had few specimens, little communication with each other, less opportunity for field study, and little knowledge of geographic and morphological variation within species, have made a great deal of work for me in determining the right names for species on which I have made ecological studies. I have had to write a whole series of papers just to sort out the nomenclatural problems. Someone else will have to do this for the bad descriptions of ***Conus*** species by late twentieth century workers.

If all previous desciptions of new species of ***Conus*** are bad, what is a good description? Most importantly, it must distinguish the proposed new species from **all** previously described species in the genus. In practice, of course, in large genera this means the species must be distinguished from those species most similar to it. But it means that the author must examine **all** of the names, descriptions, and illustrations of previously described species to ensure that none already applies to the species he proposes to describe. I see little evidence that anyone has done this in ***Conus***, where there are about 3,000 available species-group names.

It is desirable to use as many sets of characters as possible in describing a species. For prosobranch gastropods, this includes external anatomy, internal anatomy (particularly radula and jaws, if any, because they are hard structures), egg capsules and development, any available biochemical information and, of course, the periostracum and the shell itself.

In practice, shell characteristics are the most important, because the type specimens of all previously described species exist only as dry shells in museums. So it is particularly important to present as much information as possible on as large a sample of shells as possible. Because all characteristics vary from individual to individual in sexually reproducing animals, some objective measure of variation should be presented. The simplest and most convenient is the coefficient of variation, the ratio of the mean to the standard deviation. The standard error of the mean is also useful as it permits a rough direct determination of the probability that two entities were drawn from the same statistical population, that is, that two sets of shells belong to the same species.

All textbooks on statistics give the formulas for calculating these very simple statistics. The most useful statistics book for taxonomists is **Biometry** by Sokal and Rohlf. It is coordinated with the important textbook by Sneath and Sokal, **Principles of Numerical Taxonomy,** which should be familiar to anyone who considers doing research in taxonomy.

There are basically three important sets of shell characters (exclusive of those of the periostracum):

Color pattern. This is the most difficult set of shell characters to describe objectively and quantitatively. Each researcher will have to develop his own methods of verbal or mathematical description of color pattern.

Size and shape. These statistics mentioned above can be used to describe variation in shell size. Shell shape can be described by the few basic parameters of shell coiling. For ***Conus***, Kohn and Riggs (**Systematic Zoology,** vol. 24, p. 346) have written a how-to-do-it paper.

Sculpture. Usually the coronation or tubercles around the shoulder is the most prominent sculptural feature of ***Conus***. The statistics mentioned above can describe the variation in number of these, as well as in number of spiral striae and/or lirae on the last whorl, and number of spiral striae on the spire whorls.

Finally, the description should compare the statistics generated in the above analysis with the same statistics for the most similar described species. Significant differences in a high proportion of the characteristics provide evidence that supports the hypothesis that the supposed new species is really different.

I have tried to put these principles into practice in a new species of ***Conus*** I have described. The results of my study of this, a Pleistocene fossil species, will appear in the **Journal of Paleontology.**

But, in truth, I have not been able to follow all the above principles. Specifically, I have not searched through all 3,000 described species of ***Conus*** to make sure that somewhere this species has not been described before. So, in staking my reputation on my new species, I take a risk. After my paper is published, someone may recognize the species as one that has already been described, perhaps in the last century. However, I have known of this particular species for 25 years, I know the entire fauna of its locality quite well, and I have searched in the literature for a name and description over 25 years. I hope the risk is low.

Will you be in London in May? Try to catch the Shell Show! Among the prizes is the new Conchologists of America Trophy. Admission will be free.

SHELLS FOR SALE