January 28, 1963

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Dear Coauthors:

It has been a very difficult task to read your long manuscript. Congratulations.

Enclosed is an outline of criticism and suggestions.

Best regards.

Sincerely,

H. B. Stenzel

HBS:jr

Enclosure

REMARKS ON MS BY FISHER, RODDA AND DIETRICH

As far as I could ascertain there are no major bloopers in the manuscript.

MAJOR POINTS

(1) dumosa vs. symmetrica. The new Rules of Zoological Nomenclature (Article 24, p. 25) discard page precedence as the main decision in selecting the correct name and substitute for it the first reviser. The first reviser was Conrad, 1855, himself; or if he does not suit you, it was Cossmann, 1899. If you don't like that one either, Gardner, 1945, is final. I have not looked up these three works, but judging from your synonymy symmetrica must be used instead of dumosa.

(2) The treatment given on pp. 15-25 and text Figures 4-9 is wide open to objections. These collections are enormously biased. Some people pick up only large specimens. There are great ecological differences between localities, even between those from the same stratigraphic level and the same lithofacies. Athleta petrosa is very much larger in Stove Creek than at Claiborne, although both are Gosport Sand. For a similar situation read Joysey, 1959, Biological Reviews, v. 34, No. 3, p. 297 etc., especially pp. 300-301. Such curves as Figure 4 are so full of bias that they are practically meaningless. Read also Hallam, Geol. Mag., v. 96, pp. 99-108, 419-420.

The treatment given on p. 26 is better, although here collecting bias enters too.

If you wish to treat size or height or width percentagewise as to number of specimens collected, you should go out to selected localities and collect all the specimens visible in one square meter of one bedding surface, irrespective of their sizes.

(3) Ornamentation and sculpture. These features you refer to the body whorl. However, because of size differences the body whorl is no good as a reference. Naturally, a larger specimen will show fewer spines, and a collection containing many large specimens will show up differently than one having few large specimens. As a consequence your Figures 14-15 and the accompanying treatment of ornamentation and sculpture are based on very bad collecting bias.

All these objections can be avoided by using two methods. First, reinvestigate ornamentation and sculpture with reference to the first whorl, second whorl, third whorl, fourth whorl, etc. If you had a graph showing the ornamentation and sculpture on, for instance, the seventh whorl of the subspecies, you would have no collecting bias whatsoever. The Second method would consist of choosing specimens of the same height throughout. Say, use only specimens that are 30 mm high. This method eliminates collecting bias too. Both methods should be used for several whorls and several sizes, respectively. Ultimately you could show by a graph on which one of the whorls each subspecies attains a certain number of spines/whorl. Such a graph would show the evolutionary acceleration, if there is any.

MINOR POINTS

p. 5 - Stratigraphic chart. The Montian is now regarded as largely, possibly entirely, contemporaneous to the Danian. See publications by Marliere.

The Ypresian is now regarded as composed of Sparnacian at base and Cuisian at top. See publications by Feugueur (Comptes Rendus Acad. Sci. Paris, v. 254,pp. 3717-3719, 1962). Throw out the Cuisian and expand the Ypresian.

Wills Point Formation. The Wills Point is the same formation as the Porters Creek and should be dropped in favor of the latter. You might as well accept this obvious fact. See Stenzel, 1952, Mississippi Geol. Soc. Guide Book, Field trip 9, p. 33.

Therrill is best treated as a formation. It is traceable from Mississippi to south Texas and gets to be several hundred feet thick. (See Stenzel, 1952, op. cit.)

Exact correlation of the Gulf Coast with the Paris Basin is dubious. So if you omit the horizontal dividing lines between the Stages, you depict our present knowledge better.

p. 7. A lineage can not be defined arbitrarily. It is defined by descent and separation from other lineages. However, the species of one lineage are defined arbitrarily by gaps in the stratigraphic record or by the paleontologist.

p. 29, text Fig. 12. How are you showing size on this figure? Is it maximal size or mean size or what? Why not show a scale?

p. 30. You talk as if you had early and late forms of A. p. smithi. Do you mean variants at the extreme end of the variation curve?

p. 50. Trends in A. tuomeyi. How can it have trends if you have specimens from one stratigraphic level only? Is your terminal forms a confusion with extreme variants of the assemblage? Or am I confused?

p. 54. Under Plejona, give Stewart credit for investigating the situation for the first time (Acad. Nat. Sci. Philadelphia). Refer to Palmer, 1937.

For validity of Volutilithes Swanson, 1831, consult Swanson carefully and compare with the new Rules of Zoological Nomenclature (1961, p. 19). Volutilithes may be out.

p. 72. Without examining the holotypes of Voluta parva and V. vanuxenii you can not be certain that they did not come from the Upper Lisbon [= Cook Mountain] of Claiborne Bluff. If they do, their names have to be applied to the Cook Mountain subspecies.

p. 113. Use type species instead of genotype, see new Rules.

Indio Formation is terribly antiquated; if the same as Wilcox Group.

The so-called uppermost part of Midway Group in northern Mexico is in reality the basal part of the Wilcox Group. You have to know how this region was mapped to understand the situation. I have seen the evidence and have talked to the old-timer field geologists who worked there. (See also p. 91.)

Where was A. tuomeyi found in the Midway of Louisiana? This too may be basal Wilcox. Write to Louisiana Survey (Clarence Durham) for exact information.

p. 86. Phyletic species is a poor word to use. All species are phyletic, or else they are not species. Why not use main-stem species or axis species or center-line species or main-line species, etc.?

p. 91. diphyletic. Talk to one of the zoologists on the campus, perhaps Frank Blair. I think it is not possible to produce two so similar lineages separately from possibly different species of Volutocorbis. If that were possible and had really happened in this case, you would be forced to use two different genera names for the two lineages, because the same genus cannot be derived twice from a third, ancestral genus. The only way out you would have in that case is to use Athleta so as to include Volutocorbis too. See drawings.

The best answer to all evidence is that the whole sequence is monophyletic, but split into two parts very early.

If you wish you could come to Houston some time and we could discuss these and other points.

H. B. Stenzel
1/28/63