

Interactive comment on “Living coccolithophores from the eastern equatorial Indian Ocean during the spring intermonsoon: Indicators of hydrography” by Jun Sun et al.

Anonymous Referee #2

Received and published: 1 August 2017

General

This paper attempts to describe floristic characteristics of coccolithophore assemblages in the eastern equatorial Indian Ocean during the spring intermonsoon period in relation with hydrographic conditions. Planktonic coccolithophores have not been well investigated in the Indian Ocean compared with those in the Atlantic and Pacific. However, this attempt is not successful, primarily due to methodological unclarity and failure to reach solid conclusion as described below.

My fundamental concern is on species identification and enumeration. Cell counting was made exclusively by light microscopy in the present study. It is well recognized

[Printer-friendly version](#)

[Discussion paper](#)



that some coccolithophore species can be identified by light microscopy, but others not. In particular, counting of small species requires use of SEM. For example, *Gephyrocapsa oceanica* was dominant (Table 1) and no other *Gephyrocapsa* species were mentioned in the paper. I wonder whether the other species were absent in the study area, overlooked or counted as *G. oceanica*. General practice of cell enumeration of coccolithophores is by the use of SEM, or careful comparison of images between SEM and light microscopy prior to cell counting using authors' own material. Even with the latter procedure, a certain number of small coccolithophore cells remain unidentified. A total of 26 species were identified in the present study (p. 4 l. 3). Then, a question can be raised how adequately unidentified cells by light microscopy were treated in data processing. Since procedures regarding this issue are not stated at all, I cannot judge the reliability of species identification from the information provided. The quality of floristic data is crucial in the present paper, and I cannot evaluate results of authors' statistical analysis without due explanation on this matter. Other issues on the methods are given below.

Another criticism is the lack of solid conclusions. While the paper aims to characterize the coccolithophore assemblage during the spring intermonsoon period, I do not see how similar and/or dissimilar the assemblage is between monsoon and intermonsoon periods, largely because comparison of authors' results with existing knowledge is superficial and in-depth analysis is lacking. Even though our knowledge of ecology of modern coccolithophores is limited in the Indian Ocean, key literature should be referred carefully at least, among which some essential papers are ignored such as Takahashi and Okada, 2000, *Mari. Micropaleontol.*, 39, 73-86; Schiebel et al., 2004, *Mar. Micropaleontol.*, 51, 345–371; Young et al., 2017, *Proc. Int. Ocean Discov. Prog.*, 359-111. In addition, the text is not well organized, that is, the results and discussion sections are not well differentiated, and there are too many figures for the length of the both sections. This implies each topic is treated superficially and not adequately discussed. As a consequence, overall impression of the both sections is disjointed, and not well focused.

[Printer-friendly version](#)[Discussion paper](#)

Specific

Abstract: The abstract is a simple list of authors' findings and should be much more focused.

Introduction: The introduction should state a rationale for the study: why the investigation during the spring intermonsoon period is needed. But, the current introduction is a simple and insufficient compilation of existing knowledge, and findings of several key papers as mentioned above are not considered.

Materials and methods

P2 L31: What does "initial" mean?

P2 L33: Seven depths were sampled. But, data from eight depths are plotted in Fig. 11.

P3 L 10 and L12: How was cellular dimension measured? Was measurement made on all cells, or selected individual cells of each species? If the latter, how many cells were measured?

P3 L12, L16: According to these lines, authors appeared to adopt published values to cell dimension of own material. However, this can cause potentially significant error (Smayda, 1978, Phytoplankton Manual, Unesco, p. 273-279). Slight errors in cellular dimension result in significant errors in volume calculation. I reserve to judge the suitability of the POC and PIC calculation.

P3 L15: Species name should be given.

Results: Too many graphs with insufficient explanation and interpretation have made the paper unfocused.

P3 L33: It is unclear whether advection or watermass extension from the data presented.

BGD

Interactive
comment

Printer-friendly version

Discussion paper



P4 L5: What is the purpose to show the plates? The plates are not needed for the current explanation. Methods on SEM are not given in the MM section.

P4 L8: I do not see what “frequency” means.

P4 L12: Authors should pay attention to significant digits.

P4 L12-18: Numbers are given for which depth?

P4 L15: *F. profunda* appears to be most predominant from Fig. 3.

P4 L35: Ecological significance of the ratio should be given. The ratio can be subject to sample handling, and I wonder how authors differentiate coccospheres and coccoliths from intermediate stages of coccolith aggregation between fragments of a collapsed coccosphere and a single isolated coccolith.

P5 L5: Section names should be used instead of use of “inner, outer”

Discussion: Detailed specific comments are not useful at this stage.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-112>, 2017.

Printer-friendly version

Discussion paper

