

Biogeosciences Discuss., referee comment RC2
<https://doi.org/10.5194/bg-2021-351-RC2>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on bg-2021-351

Anonymous Referee #2

Referee comment on "Reviews and syntheses: Assessment of Biogeochemical Models in the Marine Environment" by Kaltham Ismail and Maryam Rashed Al-Shehhi, Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-351-RC2>, 2022

I do not think that this paper meets the expected standard for a Reviews and Syntheses paper in this journal. The Abstract, the Introduction and the Conclusion mostly fail to explain to the reader what the authors' overall purpose is. The Results are a rather verbose and poorly organized recitation of many details without much of an overall coherent structure.

First, one might want to step back and ask why another review on this topic is needed and what differentiates it from existing ones. Ideally this question should be addressed in the first paragraph of the Introduction. This paper never addresses it at all. Secondly, why a review rather than a primary research contribution? There are today a great variety of ocean biogeochemistry data products that are in the public domain and available to anyone with an internet connection. Why not attempt some systematic evaluation of a set of biogeochemistry models against one or more of these, similar to past experiments in the cited literature (e.g., Friedrichs et al, 2007; Kwiatkowski et al, 2014). Instead, this review offers a subjective and meandering assessment of the existing literature and various authors' conclusions regarding how well their models simulate various observables (e.g., in Section 3.1). If the paper were clearly organized around a set of questions or metrics I might be more charitable, but it is not.

The classification of models seems very subjective and arbitrary. Why can not a carbon-cycle model incorporate either an NPZD model or a PFT model? Indeed, most modern ones do. The history of the field is the gradual replacement of simple HamOCC or OCMIP type approaches to parameterizing uptake and remineralization of carbon and nutrient (usually P) with explicit biology models. The question is which biology model, and how much complexity is justified and useful? I do not believe that this review sheds much light on this history or offers new and useful information that could help to guide such choices in the future.

The only part of this paper that really contains anything new is Table 5. This Table

contains a huge amount of information. The only way I can see to salvaging this effort is to reorganize this information around some kind of coherent structure. The nearest historical precedent I can think of is Totterdell (1993, in Evans and Fasham, eds, "Towards a Model of Ocean Biogeochemical Processes") (10.1007/978-3-642-84602-1_15). But the current version seems more like a "data dump". The authors have gleaned a great deal of information from the existing literature. But to justify publication of such a review they need to present it in a way that is useful to the reader.

On a personal note, I will tell the authors that early in my career I had a similar paper rejected by a journal editor. It is never a pleasant experience, but we can learn from it.