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Biogeosciences
Discussions

Interactive comment on "Chlorophyll signatures and nutrient cycles in the Mediterranean Sea: a model sensitivity study to nitrogen and phosphorus atmospheric inputs" *by* M. Pacciaroni and G. Crispi

Anonymous Referee #4

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General comments

This paper presents results from a high-resolution coupled bio-physical model of the Mediterranean Sea, where a range of atmospheric nutrient loadings of N and P have been applied. The authors then seek to distinguish how variation of these loadings impacts the modeled ecosystem. There are some very basic flaws with this report that are described below in the general comments that I have developed. Without addressing these basic needs, some or all of which may be solely a matter of including additional detail that has been omitted and improving how the information is presented,

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it is quite difficult to make an assessment of the results that are discussed. At this juncture, I cannot recommend publication of this manuscript. However, investigating how aeolian mineral deposition affects oceanic ecological variability is certainly of high topical interest, and I would urge the authors to work to improve this contribution as it has good potential to contribute to our evolving understanding of atmospheric-oceanic linkages.

Specific comments (ordering is not indicative of relative importance)

Comment 1:

There are several aspects of the model structure and application that are troubling and at least need to be clarified for readers benefit.

1) There appears to be no inclusion of air-sea O2 exchange in the configuration, based on Eq. 21 where none of the terms depend on wind speed. If this is really the case, this is a fundamental flaw. The reference provided as source for this aspect of the model (Gromiec, 1983) appears to be applicable to river applications and therefore may be a relatively poor choice as a basis for a marine application. However, it is also not clear why an oxygen compartment is needed since no mention of whether anoxic or hypoxic conditions regularly manifest in the Mediterranean is included in background information.

2) The configuration of the model's light field components also needs to be better explained and justification of the approach is a must. In particular, prescribing regional attenuation coefficients based on observations (section 2.3) is questionable and is a concern, as it may predispose the results. Especially since these K values are applied in the Chl transforms (Eqs. 23, 24). A much more satisfying approach would be to allow the model to determine attenuation freely and then have this available as an additional diagnostic for comparison to observed characteristics.

3) Description of the numerical experiments is incomplete. The duration of the main run

BGD

4, S664–S668, 2007

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simulations is not prescribed in the methods (Section 2) and only mentioned in passing in the caption for Table 8 and within the section detailing the results. Also, there is a need for more detail in describing the forcing fields applied to the model, as opposed to the generic reference to forcing being based on NCEP reanalysis. Which fields are applied? Why is the 1980-1988 time frame chosen? Does this choice relate to some specific aspect of conditions over the Mediterranean region for this period?

Comment 2:

In general, the presentation of material is difficult to track since its ordering is rather disjointed. For example, in the introduction the motivations for the study are described on the bottom of p. 911. However, this is then followed by text that is more suitable as background information that should come prior to model motivations. Similarly, the last few paragraphs of the hydrodynamic model description (section 2.1) would be more appropriate to include as part of introductory material.

Comment 3:

The description of the model's two main components is quite detailed. In particular for the biochemical application being presented here, details on the physical model portion are quite extensive. Has the physical model been reported elsewhere and could that be cited as a reference for interested reader? If there is not such a resource, then it is really a necessity to report on how well the physical model performs. Indeed, with the 1/4 degree resolution employed for this application, it is disappointing that analysis of the model results is largely limited to assessing/describing annual mean fields and characterization of mechanistic interaction between physical environment and the biological components is not addressed.

Why is it necessary to provide conservation equations for both T and S. Since there is no inclusion of penetrative component of irradiance within the heat flux equation, the structure of these equations is identical.

BGD

4, S664–S668, 2007

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For the biotic model description (section 2.2), there are several cases where explanations need to be clarified or amplified.

1) What is meant by ‘detritus depend on evolution of dynamics’ (2nd paragraph)? This is in general true, so either it is trivially obvious or possibly there is some specific mechanism being referred to and there is need to make this understood by reader.

2) It would be more accurate to state that the generic B equation is based on the tracer equations (T or S), with additional sources and sinks associated with biogeochemical processes, as opposed to linking B to the momentum equation set where there is the additional Coriolis term. Also, what is meant by ‘local derivative terms’? This needs clarification and should have been mentioned in presentation of Eqs. 1, 4, and 5 as well since it would appear to be originating there.

3) Need to provide explanation for the water column and biological instabilities that are noted on p. 916. Without additional context, it is left completely up to the reader to extrapolate the intended meaning.

In general, the sections describing the biotic compartments of the model need to be better developed. For one thing, there is inconsistent inclusion of multiply signs (x) within the equations (particularly 15-17). As well, some of these sections include good textual descriptions of the processes associated with the various terms within the pertinent equation (for example Eq. 17) while in others the reader is left to ascertain these processes with Eq. 21 in particular needing further detail.

Comment 4:

The referencing is dated. Of the 14 cited sources that appear since 2000, 4 are technical reports and 1 is an abstract. Also, none of these is more recent than 2003. Furthermore, there appears to be only one citation that relates to testing sensitivity of marine ecosystem to atmospheric deposition of nutrients and this cited work is the aforementioned meeting abstract. There are a number of modeling studies appearing

BGD

4, S664–S668, 2007

EGU

over the past 4 years that investigate sensitivity of marine systems to aeolian deposition and some comparison to, and acknowledgment of, these works is a necessity. Indeed, its lack of currency is a substantial shortcoming and could be an indicator that this manuscript has met with difficulty in past attempts at publication.

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4, S664–S668, 2007

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