

Interactive comment on “Effects of upwelling duration and phytoplankton growth regime on dissolved oxygen levels in an idealized Iberian Peninsula upwelling system” by João H. Bettencourt et al.

Anonymous Referee #1

Received and published: 21 November 2019

This article analyses some important physical and biological factors of dissolved oxygen variability in an oceanic upwelling coastal zone. For this they couple a hydrodynamical model of the Iberian Peninsula with a "simple" biogeochemical model considering phytoplankton, zooplankton and dissolved oxygen interactions, and air-sea oxygen exchange flux. A number of results is discussed, in particular, that oxygen net rate of change is inversely proportional to the duration of the upwelling season.

This is an excellent work, very well written (with almost no typos) that helps understanding timely questions concerning the biological and physical factors most influenc-

[Printer-friendly version](#)

[Discussion paper](#)



ing dissolved oxygen in the oceans. The numerical work, despite its complexity, is very detailed and properly structured, analysing and discussing in detail the different biological and physical factors. I strongly recommend this article for publication after the authors have addressed some minor changes that I hope will help the reader to follow better this article: i) There is no mention to the numerical method to integrate the advection-reaction-diffusion Eq(1). Is it Eulerian, semi-Lagrangian, other? What about numerical diffusion, is it relevant? ii) Contrasting with this, Appendix A was very detailed. Is it necessary to include so many details to obtain a numerical fixed point when standard softwares (mathematica, matlab,...) makes the work easily? In any case I found this Appendix difficult to follow and I would ask the authors to smooth it out. iii) I think it can be helpful for the discussion if you show the full set of advection-reaction-diffusion equations with all the terms and not in different pieces (Eq (3-4), and all the discussion below). In particular, the way the authors address the air-sea exchange flux term was unclear for me. iv) After reading several times, I do not understand Fig. 6e. Please clarify its meaning. Is it really helpful? v) In the discussion/conclusions section I missed some discussion on the relevance of the results of this work on permanent marine oxygen minimum zones.

Interactive comment on Nonlin. Processes Geophys. Discuss., <https://doi.org/10.5194/npg-2019-47>, 2019.

Printer-friendly version

Discussion paper

