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Interactive comment on "A zooplankton diel vertical migration parameterization for coastal marine ecosystem modeling" *by* Ariadna Celina Nocera et al.

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General comments

The authors present the design and results of a theoretical numerical study of mesozooplankton diel vertical swimming behaviour (DVM) in coastal ecosystems. They develop a Eulerian framework to study more specifically the consequences of interactions between some external forcing (i.e. vertical turbulent mixing, phytoplankton concentration and light levels within the water column) and inherent properties of mesozooplankton populations (i.e. grazing rates and maximum swimming speed) on the vertical distribution of planktonic biomass and organic fluxes. They develop original, yet simple,

indices to evaluates the emerging properties of their simulations. While it is currently a simple 1D water column setup, the authors made several assumptions and choices in their study's design in order to assess quantitatively the role of DVM on the organic carbon budget simulated by coupled bio-physical NPZD-type models.

While I think the current manuscript is a valuable contribution to marine ecology, I think that it remains very theoretical in the absence of any data to validate the output and, further, that its applicability to other numerical studies is hampered by a critical assumption of the authors: the "coastal" environment represented in their study has to be deep enough to allow for diel vertical migrations to be, in part, cued by light levels. It would be very valuable to also take care of the majority of the cases occurring in coastal areas where troughs, channels and basins are actually few and separated by shallow areas where the proposed mechanisms may not play the same role at all. The article as it is now could also benefit from improvements in both the presentation of the ideas and the writing in general.

Detailed comments

1. P2 L4: "... their high biological productivity" The authors should provide the numbers about the relative proportion of the ocean surface they represent vs the relative proportion of the new production they contribute to; it is always useful for this kind of generic introduction.

2. P2 L5: I think reducing the efficiency of the biological carbon pump to zooplankton DVM is problematic... The authors should be more thorough in their description of the carbon pump. It is all the more important since their study is probably most relevant for computing the carbon budget of coastal ecosystems...

3. P2 L17: did the authors actually mean "organic matter" instead of "nutrients" here?

4. P2 L26-29: The authors literature review about vertical swimming behaviour of zooplankton is lacking. I think they should read carefully at least these two papers:

Pinti et al. 2019 http://doi.org/10.1098/rspb.2019.1645 and mostly Sainmont et al. 2013 http://doi.org/10.1007/s12080-012-0174-0.

5. P2 L32: The authors should provide some values as example to understand what kind of “intense vertical mixing” they think of, because counteracting zooplankton vertical swimming behaviour implies quite high mixing!

6. P2 L35: “stratification” appears here for the first time... I am not convinced yet that the authors managed to explain the role of stratification on zooplankton DVM.

7. P3 L2: I do not understand how “migratory behavior of zooplankton” is different from “diel vertical migration” ? How come “migratory behavior of zooplankton” would not impact “diel vertical migration” ?

8. P3 L3: what about primary production, then? One important assumption of this study is that it implements fully coupled NPZD-type model of pelagic production. Why are the author disregarding top-down control in their objectives definition, while they discuss it eventually?

9. P3 L17: I really do not think the “heart of this paper” is about “elucidating the causes of this migration”! The authors should refocus their message around the second part of their proposition (“... establishing significant correlations...”).

10. P3 L19: the opening sentence of this paragraph is really awkward. It should be reformulated.

11. P3 L29: for equation (1), even if we accept the authors assumption that zooplankton swimming speed roughly follows an hyperbolic tangent attenuation profile, it is regrettable that they did not provide any data (most likely to come from detailed acoustic studies) to at least empirically calibrate their CORE swimming speed function!

12. P4 L3: what is the value of the critical Pmin parameter in this example simulation (Fig. 1)? Pmin = 0?

13. Fig. 1 (again): a simple side panel showing the shape of the swimming function at noon over the whole water column would be very useful.

14. Fig. 2: as it is now, this figure is not very useful. And its legend is confusing... it looks like it represents the swimming speed at the different depths represented by the dashed line in Fig. 1, not as “a function of time for the light...”

15. Fig 3. This figure is really confusing... The distinction between space and time is unclear. For example, is there a connection between the euphotic and aphotic areas during the day? How do you decide about the “intensity” of the relationships?

16. P7 L1: the grazing function being so important to the analysis it would be better to provide it: for example, does “sigmoidal form” mean a Holling type III ? What is kg ?

17. P7 L7: “...between 0.2 and 20 mm”. It should be clearly stated that the authors aimed at achieving one common parameterization over two orders of magnitude in size.

18. P7 L11: since Fig. 5 is described before Fig. 4, both should be swapped.

19. P7 L11: “restored” when? At the end of a calendar year? Why?

20. P7 L11-12: The authors should explain in more details why having similar mixed layer depths is an important requirement of their modeling set up.

21. P7 L13: why is it different than the two-week relaxation time from above?

22. P7 L15: Regarding vertical eddy diffusivity specifically, surface wind stress is an important component (especially in 1D water column setups), but what about the turbulent kinetic energy created by horizontal shears? This is typically overlooked in 1D simulations, unless there is some form of minimum background level applied throughout the water column. Did the authors consider this? If so, how?

23. P7 L19: How did the author select a priori the parameters to be tested? How did they avoid the risk of overlooking something unexpected?

24. P7 L30: please provide the parameter space explicitly: name of parameters, range values.

25. P7 L31: this “indicator” approach is very interesting!

26. P8 L5: again, please provide the actual value required for Kz to counter the given Wz_max tested! I am positive some values will be ruled out as impossible...

27. P8 L9: Why did the author establish this threshold of vertically integrated zooplankton biomass. Integrated abundance has nothing to do with aggregative behaviour in their simulations!

28. P8 L23: about the RC:N = 7 ; did not the authors state in the Methods that there were 2 distinct C:N ratios, one for phytoplankton and one for the rest ?

29. P9 L5: Fig. 5 did not show the functions phi, psy and omega ?!

30. P9 L15: the authors choices for the values are arbitrary and should be better motivated.

31. P10: Table 2; I think the experiment numbers are not used within the text, which is a waste. . .

32. P10 L6: “. . . based on the literature”. This is NOT enough. What processes did you want to explore with these specific values you did sensitivity analyses for?

33. P10 L11: really confusing sentence.

34. P11 Fig. 4: I DO NOT understand the organization of the panels. Please refer explicitly to the letters a) through f). As it is now, it does not look like the result of a factorial design, and I do not know what was the rationale for showing these particular results... Is there any migration at all in a), by the way?

35. P11 L1: about the light levels (Ic) : and what about the visual capability of the migrating zooplankton? Can they detect 0.01 W m-2 ? Alternatively, are there organisms

that are actually "camouflaged" at a light intensity of 10 W m-2 ?

36. P12 L1: the averaging over a full seasonal cycle is a choice. Why did the authors do it? Why did they not focus on the productive season?

37. P12 L5: “. . . and the relationship between the mentioned parameters is not so evident” maybe so, but this is not really acceptable here, since it is the authors duty to tease them appart.

38. P13 L22: BE CAREFUL! I don’t think any of the references here deal with "experimental" work!

39. P13 L30: This part of the discussion should be tied much more directly to the Eulerian framework used in this modelling study. Actually, all the results discussed here have a meaning only in this peculiar context.

40. P17 L4: I guess the maximum swimming speed is important too?

41. P17 L7-9: I do not understand the argument about instantaneous grazing rate. I would like the author to develop and clarify their idea.

42. P17 L9: I understand, though, that this parameter is useless in a configuration where there is no feed-back of zooplankton on phytoplankton concentrations, i.e. an offline coupling which remains rather common in 3D coupled models of phytoplankton-zooplankton models. This situation can occur when simulation fields from distinct models or in situ observations are used, or in situ data.

43. P17 L18: please quantify how “intense” the carbon export is.

44. P17 L20: the authors can certainly provide the numbers from the literature they think their results agree with.

45. P17 L25: But the DAILY grazing rate should/could be modified accordingly and increased (under certain constraints) to allow for a migrating organism to graze enough in a shorter period at the surface! This is certainly the essence of the asynchronous

night-time behaviour observed in some zooplankton species, i.e. individuals go up to feed until they are satiated, then go/sink down, go back up again if necessary and in any case manage to get what they need during this time period (e.g. Sourisseau et al. 2008 http://doi.org/10.1139/f07-179)

46. P17 L30: "... global change related processes" which ones?

47. P17 L32: "proportion/preference" please avoid this kind of shortcuts and explain what you mean when you collate two distinct notions like that.

48. P18 L6: Since there are no data provided, I think that there is nothing in this article that provide evidence that a model including DVM "better" or more "accurate" estimates coastal marine ecosystem productivity. The authors have just showed that the resulting dynamics is different with and without DVM.

Typos / minor modifications

1. P1 L18: replace "and/or" by "and".

2. P2 L8-9 and throughout: remove "relatively" and "potentially". Please abstain from using such modifiers (adverbs); it just dulls the authors' thesis.

3. P2 L26: replace "one copepod specie" by "one copepod species"

4. P3 L1: replace "... dynamics with DVM" by "... dynamics including DVM"

5. P3 L2: replace "... to characterize if in which" by "... to characterize in which"

6. P3 L2: "... zooplankton impacts"

7. P3 L21: replace "relatively easy interpreted" by "interpreted clearly".

8. P6 Fig. 3 caption: in general, prefer "relationship" over "relation".

9. P3 L21-22: replace "Zooplankton swimming behavior we impose here..." by "Simulated zooplankton swimming behavior..."

10. P3 L23: remove "mainly"

11. P3 L27: replace "irraidance" by "irradiance"

12. P8 L12: replace "prominence" by "concentration".

13. From here on, I provide an annotated pdf version of the paper to help with typos and writing issues.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2020-10/bg-2020-10-RC2-supplement.pdf

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-10, 2020.