Geoscientific

Model Development

Discussions

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Interactive

comment

Interactive comment on "A coupled pelagic-benthic-sympagic biogeochemical model for the Bering Sea: documentation and validation of the BESTNPZ model (v2019.08.23) within a high-resolution regional ocean model" *by* Kelly Kearney et al.

Anonymous Referee #1

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This manuscript presents a thorough description and validation of a coupled physical-biogeochemical model for the Bering Sea. The physical model is the Bering10K and the biogeochemical counterpart is the BESTNPZ. The latter aims at characterizing the complex ecosystem of the Bering Sea by representing variables and processes not only in the water column, but also in the benthos and the sympagic sea ice environment. Both models have been used in previous publications, although both have been modified since the last published papers. The manuscript explains in detail the changes

(and the reason behind these changes) of the Bering10K and BESTNPZ since the latest publications and presents a thorough validation of several aspects of the coupled model. Overall, the authors conclude that the model skill is quite good in terms of the physics (e.g., patterns of water movement, mixing, stratification, water masses distribution), but there is still room for improvement in the ecosystem module (e.g. limited ability to replicate large scale patterns in nutrient cycling, primary production and zooplankton community composition).

General comment:

I found the manuscript well written and thoughtfully organized. Model performance is well assessed with a suit of metrics and figures; furthermore, technical details and model equations are well documented. I think that the clear identification of previous issues in the model and the explanation of their fixes (along with the honest description of the things that are still suboptimal, unnecessary and/or still not working well) will be very useful for fellow modellers encountering similar issues. Below, I make a list of some minor comments, which are mostly intended for clarification. Therefore, I recommend this paper for publication in Geoscientific Model Development.

Comments:

P5.L30: It'd be useful to state which publications had this nudging, since it is a major issue.

P7.L11: why did the approach change from adjusting CORE to adjusting CFSR? It'd be interesting to know.

P8.L19: Usually, modellers sub-sample the model to compare against observations. However, in this case, I understand that you have to interpolate the observations to look at the ice edge location. It would be useful to know how much your results depend on the interpolation method chosen and also how much interpolation is needed (ie, are there just a few data points missing here and there, or are there times when most of

the domain is being interpolated from a few points with satellite data?).

P9.L21: It would be useful to have the three regions shown in a map (a new figure or even in another panel in figs 1 or 2)

P10.L6: Given that you already calculated the Simposon parameter (SI), why not check the location of the front by using this parameter? Some authors have used a critical value of SI to determine the position of the fronts (eg, Bianchi et al 2005), while others have looked at the region of largest change in SI (eg, Wang et al 2004).

References: * Bianchi, A. A., Bianucci, L., Piola, A. R., Pino, D. R., Schloss, I., Poisson, A., and Balestrini, C. F. (2005), Vertical stratification and air-sea CO2 fluxes in the Patagonian shelf, J. Geophys. Res., 110, C07003.

* Wong, L. A., J. C. Chen, and L. X. Dong. (2004), A model of the plume front of the Pearl River Estuary, China and adjacent coastal waters in the winter dry season. Continental Shelf Research 24.16: 1779-1795.

P11.L33: Is there a reason or a reference for the choice of attenuation length scale of 45 m?

P12.L1: "our model-derived estimate of satellite-visible chlorophyll is a rough one": I'd suggest to mention explicitly that, while the model is vertically integrated, the satellite can only measure the surface.

P12.L17: I believe there is a typo in the biomass unit (it should be 10 gC m^{-3} rather than $^{-2}$). Also, is there a reference for this value?

P14.Fig4: I'd suggest to add the location of the observations as small dots in the top panels, so we can se where interpolation is taking place.

P15.Fig6caption: "on the eastern shelf": Should it say "in the model domain" instead?

P16.L5: I'd suggest to mention that "structural fronts" are also known as tidal fronts

P17.1stParagraph: this text needs to point/refer to a figure. Is it Figure 10?

P18.Fig8caption: I'd suggest to mention explicitly here that these lines used the 0.5deg approach. Also, I'm curious: how would they look if you were using a critical SI instead?

P19.L2-4: when comparing the model to the observations at the surface, it would be fair to mention that there are no observations in the top ∼10 m (but we rely on the interpolation).

P25.L20: it feels to me that a word is missing in this sentence to make it clearer, maybe ". . . capable of being differentiated between EACH OTHER with this type of biomass box model."

P28.FigA1: This is a great way of showing a complex diagram. However, some colours are hard to differentiate (e.g. tan vs gold).

P28.FigA1caption: I'd suggest to mention the circles explicitly, eg: "CIRCLES SHOW STATE VARIABLES (gold = nutrient, green = producer, blue = consumer, brown = detritus). Edges (lines) represent fluxes between state variables and curve clockwise from source node to sink node; edges colors indicate. . . "

P34.Table6: Usually, alpha values for diatoms are larger than for small phytoplankton. Since you have alphaPhS > alphaPhL, I'm wondering if it is a typo or if there is a reason for this choice of values.

P46.L21: Does the 1% of Ph/Det lost to denitrification lead to a direct flux of NO3 into or NH4 out of the sediments? Since such fluxes are not shown in the equations, it seems that this 1% just adds to the 20% lost out of the DetBen pool. It would be nice to have a clearer explanation about this choice.

P50.L27: typo: "for" is written twice

P55: "MATLABCOMPILE" row: I'd suggest to replace "my" in the third column by "K.Kearney's" or any other way that is appropriate

P58.L2: I'd also suggest to rewrite to avoid "my" and "I"

P58.TableC4: given that all types are "RHO-variable", I'd suggest to mention that in the caption (along with an explanation of what "RHO-variable" means" and leave the column showing only 2D or 3D.

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Discussion paper