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Interactive comment on "Influence of oceanic conditions in the energy transfer efficiency estimation of a micronekton model" *by* Audrey Delpech et al.

Anonymous Referee #1

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I- General comments

The main question addressed by this study is to determine optimal sampling regions in which micronekton biomass observations can provide useful information to better estimate the energy transfer efficiency coefficients associated with an ecosystem model. Those coefficients are shown to be tightly linked to specific combinations of four indicators, depending on different environmental conditions (also referred to as regimes in the manuscript). To examine the influence of each indicator, different configurations of environmental regimes are built based on a cluster analysis. The optimal configuration is then investigated using Observing System Simulation Experiments (OSSEs), in which synthetic observations are first randomly selected within the tested regions, then based on two existing observing networks. To assess the quality of the conducted OSSEs, the authors used three metrics: the mean relative error (actual score of the experiment), the residual value of the likelihood (accuracy of the experiment) and the number of iterations of the optimization scheme (convergence speed). The authors found that the optimal combination of the four environmental indicators is associated with productive, warm, moderately stratified waters and weak surface currents, such as those found in tropical regions along the eastern margins (and therefore with the PIRATA moored array). The mechanisms based on the interaction of biological and physical processes that influence the micronekton biomass are also identified.

After some clarification and some necessary major changes (see below), I believe this manuscript is suited to Biogeosciences, as it presents an interesting and novel methodology to identify relevant combinations of environmental forcing variables, prior to performing OSSEs for biogeochemistry. Even though I consider that major revisions to the manuscript are required, note that I do not think that further experiments or diagnostics are necessary.

II- Specific comments

1) I slightly struggled with the overall organization of the manuscript. The reader would benefit if the authors follow a more strict structure: e.g, (1) describe the ecological model configuration with more details on the physical forcing, including limitations and caveats about the representations of the biological/physical processes, (2) the Clustering approach and (3) the OSSE system design (i.e., the twin simulation, the data assimilation scheme along with the MLE approach, and the synthetic observations). Then, introduce theoretically the different metrics used to evaluate the observing networks and follow this with the discussion of the results. In Sections 4 and 5, some elements of perspectives seem to be scattered over multiple places, I suggest gathering them together for the sake of clarity.

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Discussion paper

2) It is important for the casual reader to better introduce the clustering method in subsection 2.2, and explain what is the added value in comparison with more classical sensitivity or correlation analyses. Possible limitations related to clustering could also worth a mention (e.g. possible misleading statistical interpretations, etc).

3) The subsection 2.3 should also better introduce the OSSE procedure, as specific guidelines need to be followed. An overview of those guidelines can be found in the review paper by Hoffman and Atlas (2015), https://doi.org/10.1175/BAMS-D-15-00200.1, while a rigorous framework of strategy and validation techniques is described, for example, by Halliwell et al. (2014), https://doi.org/10.1175/JTECH-D-13-00011.1. Also, note that describing your OSSEs as “twin experiments” is misleading here, as your nature run (TRUTH in the manuscript) has different initial forcing fields than the control run (TWIN in the manuscript). Further information can be found in the two references given above.

4) The results mostly show that the performance of each OSSE depends on the geographical locations associated with the synthetic observations rather than the actual design of the in situ networks used to perform the acoustic transects. Could the authors please comment on that matter.

5) To facilitate comparisons between the different OSSEs, the histograms presented separately in Figures 2, 4, 6 and 7 could be gathered together.

6) The first paragraph in the discussion (Section 4) mostly presents conclusions of the previous sections, I would suggest to move it in the last Section (Conclusions).

7) In the Conclusions, limitations and caveats associated with the OSSE results need to be further discussed, in addition to the methodological limitations discussed in subsection 4.3.

8) It might sound minor, but the authors should consider to properly cite the PIRATA and the BAS projects in the acknowledgements section, along with their institutional

support. It would help the readers to find the data if they want to use it too in further studies, and it is important for sustaining and justifying long term time series associated with both projects.

III- Technical corrections

An annotated manuscript (see supplement) is provided along with this document to provide some technical corrections. Note that the annotations on the PDF can be displayed using Google docs.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-353/bg-2019-353-RC1-supplement.pdf

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