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Interactive comment on "A perturbed biogeochemistry model ensemble evaluated against in situ and satellite observations" *by* Prima Anugerahanti et al.

Anonymous Referee #1

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This manuscript examines the structural sensitivity (to equation formulations) of a marine ecosystem model by using ensemble analysis across various ocean sites. The topic is highly relevant and I like what the authors are trying to achieve. The protocol is well thought out and this study can potentially make a useful contribution to the literature. There are however some significant weakness in the manuscript which need to be addressed.

Major comments:

(1) My major criticism is that, in general, the model appears to show major discrepancies with the data, undermining the credibility of the whole exercise, including the

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Discussion paper

conclusions. To be effective, the default model run should show reasonable correspondence with the data but, in several instances, it appears not to do so. Just because the MEDUSA model is already parameterised and published in this regard does not save the situation here because the work involved changing the parameterisations of sinking, maximum and grazing rates (that's rather a lot; page 6, line 7). For example, I am not convinced about the new parameterisation of sinking, namely a sinking rate of 0.1 m d-1 (page 6, line 17) which seems much too low. At PAP, the blues stars (default run) are way too high relative to the blue crosses (observations) indicating a major discrepancy for chlorophyll (Figure 4). The average chlorophyll values for the oligotrophic stations look ok, but the depth plots do not look good at all in this respect (the deep chlorophyll maxima look poorly reproduced; Figure 6). I need more convincing that the model is credible at these sites. There also seem to be large discrepancies for L4 (Figure 4). The modelled vertical concentrations of nitrate at PAP look way too high compared to the data (Figure 3). Why have box and whisker plots not been produced for nitrate, comparing model and data? And why does the appendix (supplementary material) focus only on chlorophyll, and not nitrate? Overall, I am left in doubt as to whether the model, as parameterised for the default run, is credible. The authors could help the situation by looking at some other metrics, if only for the default run. For example, what is predicted primary production at the different sites and how does this compare with data (even just comparing annual average would be highly useful)?

(2) The ensemble run at each station is initialised using in situ measurements (page 6, line 31). What is needed is a stable initial condition, which will not be potentially vulnerable to initial condition instabilities. So surely what is needed is to run the first year over and over (do a spin-up) until a repeating cycle is reached, from which the run through the various years can then be undertaken.

(3) A major conclusion of the work is (page 15, line 29) that "small perturbations in model structure can produce a wide range of results". This is a very significant conclusion and I think the authors can justifiably make it. For the most part, however, the

results as shown in the Figures don't show this directly, because they involve various parameterisations acting simultaneously. There is plenty of text in the Results section to support their contention, focusing on individual parameters. I wonder if this conclusion could be better represented in the graphical representation of the results.

(4) The Introduction is generally well written, introducing the topic of model complexity nicely. The Discussion should mirror the Introduction, saying what the current study has said in context of the wider picture. Instead, the Discussion is mostly just an extended re-hash of the Results and does little to address the big picture. For example, what do the authors conclude about model sensitivity in context of complexity science and the onward drive to produce model of ever increasing complexity? A much bigger play could be made on the need to do sensitivity analysis, an important conclusion with which I fully agree. Articulate the benefits of the ensemble analysis over previous studies that have focused more narrowly on particular parameterisations. Etc. There is plenty of scope and I would say the Discussion section needs a significant overhaul in this regard. It needs re-emphasis; a few extra lines of text will not do.

Other comments:

(1) The authors articulate two types of uncertainty (page 2, line 26): "parametric, associated with the choice of parameter values; and structural, which relates to the underlying model equations". Structural uncertainty can also refer to the structure of the model itself (number of compartments, linkages, etc). This should be mentioned, stating that the authors are only looking at structural uncertainty to do with equation formulations.

(2) On page 9, line 12, there is "A selection of ensemble results are presented". A selection? On what basis?

(3) Some of the text associated with the Figures is microscopically small.

(4) Be sure to cite Le Quere, not Quere without "Le".

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