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Interactive comment on "Microbial diversity-informed modelling of polar marine ecosystem functions" *by* Hyewon Heather Kim et al.

Anonymous Referee #2

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General

The manuscript provides an analysis of the bacteria dynamics and ecosystem functioning in the surface ocean layer of the coastal West Antarctic Peninsula, based on in situ measurements and an ecosystem model. The authors develop and validate an existing model and apply it in a 0-dimensional configuration to analyze the bacteria dynamics and the link between the bacterial characteristics and the ecosystem functions. To investigate the impact of climate change on the ecosystem, they use the model to assess changes on bacteria fluxes and ecosystem functions under temperature increase and sea-ice melting conditions. The manuscript makes novel contribution with respect to

ecosystem modelling, in which bacteria are often under-studied despite the fact that they play a crucial role in the ecosystem. The manuscript is well written and organized. However I have the following main concerns that should be addressed before I can recommend its publication: some aspects on the description of the model and its specific implementation for this study should be justified and clarified; the authors carried out a validation effort but, as the validation performed in the study of Kim et al. (in review) is not accessible and the specific implementation is unclear, this effort should be fleshed out to gain additional confidence in the model results; also discussions on model performance, on limitations and weaknesses of the modelling should be included.

Main comments

1/ Description of the ecosystem model and its implementation for this study

One of the objectives of the work is to extend an existing model (Luo et al., 2010) applied by Kim et al. (in review) in the study area, by refining the bacteria compartment of this model. The fact that the manuscript of Kim et al. (In review) is not yet accessible makes it difficult to understand the extension and the specific implementation (period, 1-d/0-d, boundary conditions) performed for the study presented here. The modelling is 0-dimensional. The authors should justify the choice of the 0-dimensional study instead of a 1-dimensional study as performed by Luo et al. (2010) and that is usually done for ecosystem modelling at a water-column measurement station. Is 0-dimensional (and even 1-dimensional) modelling appropriate for this coastal site? Is there a significant influence of lateral transport of organic carbon or nutrients on the ecosystem in this region? If a 0-dimensional is justified here the limitations of this 0-d modelling should be clearly discussed. In the Supplementary Material, the authors specified the boundary conditions of the model during the growth season for the nutrient and dissolved organic matter: "The boundary conditions of nitrate, phosphate, SDOC, SDON, and SDOP are set to 30.9 mmol m-3, 2.4 mmol m-3, 6.5 mmol m-3, 0.6 mmol m-3, and 0.03 mmol m-3, respectively". I find unclear the description of the boundary conditions for this 0-dimensional modelling. What are the boundary conditions for phytoplankton, zooplankton and particulate organic carbon? Are the given conditions at the base of the 10m depth layer? A constant concentration of variables over time at 10m depth does not seem appropriate for representing a seasonal cycle of the ecosystem. The vertical fluxes of the different model variables at the base of the modelled layer should be better specified in the case when the MLD is greater than 10m or at least references to a similar 0-dimensional study describing this should be included (Luo et al. (2010) study is a 1-dimensional modelling study). The authors should clarify and justify the forcing and boundary conditions of the modelled surface layer and specify the depths of the euphotic and mixed layer here.

2/ Data assimilation

The authors show that data assimilation and parameter optimization can reduce model/observation errors, especially for bacterial stocks and flows. However, the simultaneous assimilation of climatological data and data corresponding to the given year raises questions, notably given the strong link between nutrients and phytoplankton time evolutions (Kim et al 2016) and the possibility of a time lag in phytoplankton growth from one year to another. The authors should justify the choice to assimilate climatological data for chlorophyll and microzooplankton instead of not assimilating these data if they were not measured in the simulated year as is done for nitrate and POC? Does this choice lead to some inconsistencies?

3/ Validation of the model results

The authors present a comparison of the model results with the available in situ data. First, the description and discussion of these comparisons should be a little more substantial. For instance the error on primary production appears significant in some years (e.g. 2012-2013) and data assimilation does not seem to bring an improvement in modelled primary production for all periods and years (e.g. January/February 2012) (Figures S1-S5). Also a negative correlation is obtained in some years for phosphate. The

authors should mention and discuss these points. Second, a description with a figure of the comparison of the modelled and observed climatological or 4-year seasonal cycles of nutrients, phytoplankton, zooplankton and bacteria (in addition to the error that is presented) in the main text or in the Supplementary Material would increase confidence in the capacity of the model to represent the seasonal cycle of the ecosystem. The authors use their model to explore the impact of an increase in temperature and a decrease in sea ice concentration, predicted as a result of climate change, on the WAP ecosystem, in particular on bacterial fluxes, primary production and POC export flux. A validation of the model's capacity to reproduce the already observed climate trends of the ecosystem as mentioned in the introduction L49-52, over a longer period for some of the model's variables (POC, chlorophyll), fluxes (primary production) and/or indicators (for instance time and magnitude of the maximum concentration of bacteria, phytoplankton, DOM or POM or primary production or annual averages) would strengthen confidence in the model for the study of the impact of future changes. This is perhaps presented in the study by Kim et al (in review) but could be redone with this new version of the model and added in this manuscript, perhaps in the Supplementary Material. Another possibility would be to compare the interannual variability obtained with a modelling without data assimilation and with the climatological model parameter set of the 4 simulated years (2010-2011 to 2013-2014) to the observed interannual variability (by specifying the potential anomaly in temperature and sea-ice concentration for those years).

4/ Interannual variability

The following comment is in line with the previous one. The ecosystem model is applied to 4 consecutive years, 2010-11 to 2013-14. The results show interannual variability in bacterial carbon stocks and fluxes. As the changes of primary production and POC export flux are analyzed under varying temperature and sea-ice concentration conditions, those fluxes could also be presented (in Figure 4) and discussed for the 4 modeled years. The authors do not discuss the link between the interannual variability of bacterial C flux and that of meteorological and physical forcing. The authors should consider adding a short description of the meteorological and physical forcing for these 4 years. A figure of the forcing in the Supplementary Material would also be helpful. The authors could specify the potential anomaly in temperature and sea-ice concentration for these 4 years and consider adding a discussion on the interannual variability in ecosystem functioning and in particular bacteria dynamics in response to the interannual variability of forcing.

5/ Discussion on modelling limitations and results

Section 4 should be flushed out with discussions on weaknesses and limitations of the model such as 0-dimensional modelling, short duration of the simulation to explore impact of climate change, errors in some variables or fluxes, data assimilation. The authors mention a “microzooplankton model-observation misfits” in their model outputs (L 163). A discussion on the potential impact of this discrepancy on major results of the study, for instance on distribution of loss terms (including grazing) of the BCD presented in 3.2 and discussed in 4.1, seems to me to be necessary. The authors refine the bacterial compartment of the model. It would be interesting that the authors specify if they have compared the results of this new version with those of the basic version and if so, if they obtain a significant improvement of the modeling of bacteria concentration and production, primary production and POC stock with this new version. It would be relevant to know the potential positive contributions of this complexification of the ecosystem model to guide future works on ecosystem modelling. The use of the term “POC export flux” for the calculated sinking flux of particulate organic matter at 10m depth does not necessarily seem appropriate to me. The term POC export generally refers to POC export under the euphotic layer or the mixed layer. What are the depths of the mixed layer and the euphotic layer in this region? This term could be replaced by a more appropriate term at least in the introduction, discussion and abstract sections.

Minor comments / technical corrections

L55-58 : Could you be more specific and indicate the growth of what, the depth of the Palmer Station, the period of comparisons with observations? L118-119 : Could you specify the depth of the site modelling? L 299-301 : The authors should justify the choice of perturbations applied on temperature and sea-ice concentration by citing previous studies on climate change in the study area. L 406 : Replace “phytoplankton account” by “POM production by phytoplankton accounts” ? L 415 : "experiments"seems more appropriate than "scenarios" considering the duration of the simulation. L 418, 431 : Warming temperature/ temperature warming : Remove temperature or replace warming by increasing. Figure 4 : black titles written over blue colours are difficult to read in Figure 4b, the values in the colour bar overlap. The comparison of HNA and LNA bacteria biomass and fluxes would be easier with an identical range in the colour bar of both panels. Figure 5 : The reading of this figure could be simplified by a colour code for the different compartments. Figure 8b : via instead of vi on the fourth panel.

In Supplements: Figure S1-S5: Some labels of y-axis on figures S1 S5 S6 are cut and grey lines are visible. How are the model outputs and observations normalized? Are they both divided the observed means? Please indicate units of the model/observations errors. L 117: “Kim et al. in prep” : Is it the same article as Kim et al. in review? L 209 : Do you mean June 1, 2012 instead of June 1, 2011? Tables S2-S6 : Please indicate units of the parameters.

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