Comment on gmd-2021-91
Anonymous Referee #1

Referee comment on "NorCPM1 and its contribution to CMIP6 DCPP" by Ingo Bethke et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-91-RC1, 2021

Summary
This manuscript represents a comprehensive overview of NorCPM1 simulations as contributions to CMIP6 and DCPP, with a focus on the evaluation of NorCPM1's capabilities in terms of reanalysis (assimilation) and decadal retrospective forecasts (hindcasts). After an introduction to the topic, the authors present a detailed description of changes applied to NorESM in order to be used as a decadal prediction system, NorCPM1. In this part of the manuscript, the authors' focus lies on the oceanic data assimilation scheme utilizing an EnKF, the inclusion of observations and their uncertainties, the localization technique, the scheme to cope with competing innovations in terms of hydrographic profiles, SST, sea ice observations. The construction of sea ice anomalies in preparation for the assimilation experiments leads the authors to actually run two different assimilations and subsequently, hindcasts. The next two parts of the manuscript are dedicated to the evaluation of the quality of reanalyses and hindcasts. Here, the authors analyse the variability of physical quantities of atmosphere and ocean, of sea ice, and of biogeochemical parameters. The main conclusions from this analysis is that (1) NorCPM1 actually has good decadal prediction skill in terms of surface and lower tropospheric physical states, with less skill in biogeochemistry, (2) external forcing exerts a large impact on the skill of decadal predictions with NorCPM1, (3) initialization contributes to skill in some regions and at some (shorter) lead times.

Comments
This manuscript not only delivers a fine description of NorCPM1, but also potentially reaches out to other efforts in the decadal prediction community due to both its comprehensive description of the data assimilation effort, and its comprehensive analysis of assimilations and hindcasts in atmosphere, ocean, sea ice and biogeochemistry. Although the manuscript is rather long, the authors found a way to structure it nicely by implementing a recurrent scheme of subsections taking care of evaluating variability of physical ocean state, biogeochemistry, sea ice, and atmosphere, and by shortly introducing the intention of each section right at the beginning of each section. Also, the authors honestly report on mishaps during their simulations (2015+2016 GHG, the ozone shift of 23 months, and deviations from protocol: land use SSP370 instead of SSP245), which could not be corrected for, do not really influence the authors' conclusions, but could potentially be important for future users of this simulations.
I have three general remarks/questions for the authors to ponder with:

1) I take the point that the oceanic DA with an elaborated EnKF scheme carries a lot of the interest to the authors. However, it seems that the combination NorESM+CMIP6 external forcing carries most of the skill in decadal predictions. As we know, this can be seen in other ESMs, too. And of course this usually is a good sign: if the combination ESM + external forcing delivers good results, so that assimilation does not have to repair too many "shortcomings". To provoke the authors, albeit in a friendly manner: should the prediction community rather invest in better models than in sophisticated assimilation?

2) The authors rely on the ocean-atmosphere coupling to transfer observational information to the atmosphere. I am totally fine with this, especially in the view of multi-annual predictions. Nevertheless, what do the authors think about having an atmospheric assimilation as well, at least for the large scale atmospheric state? I would also like the authors to be very clear from the beginning that the EnKF is applied for oceanic/sea ice DA and IS NOT applied in atmospheric assimilation here. This point could be made in l.94 "NorCPM1 further stands out in that it uses an EnKF based anomaly DA scheme..."

3) The authors include a lot of figures, and I like this very much. However, the quality of the figure annotations (labels) is sometimes rather poor. I would like to ask the authors to re-assess the annotations, this would greatly help the reader to quickly connect with the figures.
   3a) figures with maps are at the limit in terms of crowded information, but that is still okay.
   3b) although the maps themselves are in hires, their annotations sometimes look very lowres, e.g. as in Fig. 3
   3c) please put the annotations outside the maps according to rows and columns, similar to Fig.3 (Figs. 7,9-12,15-17,21-24,D3-5)
   3d) huge difference in font size, Fig. 14
   3e) Would be good to have all the a,b,c... at a rather similar position throughout all figures. Now they are sometimes in the upper left, lower left, or somewhere within the figure.

And one minor remark:

l.98
"see Section 2.1.1 for details"
Is this reference meant for the description of the DA? Then section 2.2 (or subsections) would fit better.

I would like to thank the authors for this comprehensive, already well written manuscript, which I enjoyed reading very much!