Earth Syst. Sci. Data Discuss., referee comment RC1
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Comment on essd-2021-194

Anonymous Referee #1

Referee comment on "Next generation of Bluelink ocean reanalysis with multiscale data assimilation: BRAN2020" by Matthew A. Chamberlain et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2021-194-RC1, 2021

The manuscript presents a detailed description and validation of an ocean reanalysis, and it is very appropriate to accompany the dataset itself. The paper is well written, and explains very honestly the strengths and weaknesses of such dataset. I enjoyed reading it. As such, I recommend the paper for publication, but I ask the authors to better discuss, illustrate and explain several issues, summarized below. I think having a dedicated section "Discussion" before the Conclusions is a good option.

General points (for which I suggest including a more detailed discussion).

- Neglecting sea-ice modeling in the reanalysis seems to me a weak point, considering the effects of sea-ice induced circulation around e.g. ACC, which is certainly an area of interest for the authors. A deeper discussion about what they expect to miss, and if there are plans for including a sea-ice model will benefit the paper.

- The validation mostly focus on observation diagnostics. In my opinion a reanalysis is unique in the sense that can capture integrated diagnostics (OHC trends, transports, etc.) which are to some extent unobserved. It seems from the presentation of the work that climate applications are not the focus at all of this dataset. Also, the heterogeneous assimilated data (many datasets switching from delayed to real-time mode) may also compromise the low-frequency variability. Any thoughts about this, to include in the Summary/Discussion?

- The DA system is detailed in a companion paper, not available at the moment (in review for Ocean Modeling). Because of that, some aspects of the DA system are probably not explained, so the reader may have troubles to understand the formulation, if there is some lag between the two publications (see also below specifically).

Minor points

Line 4: it is said up to 2019. It will be useful for the interested readership to specify the plans for update (near real time, once in a while, or never?).

Line 7; "for some variables" please be specific in the abstract.

The spinup period seems short (3 months). Do you have evidence that is enough for stabilizing most low-frequency variability indexes, or was chosen more for practical reasons?

Line 72 vertical resolution seems quite low compared to most other reanalyses. It is also not clear (Line 142) how can the system assimilate both night and day time SST data, since the diurnal cycle will be for sure underestimated. Or maybe I am missing something in the explanation (lines 141-142 are not very clear to me).

Line 74: Forcing fields masked if sea-ice. Not clear how do you use them if sea-ice occurs? Which fluxes would you use instead?

Line 108: it is clear from the text below that the ensemble anomalies in the EnOI are "climatological" (i.e. not flow-dependent). Better to specify here for clarity.

The multiscale Data assimilation formulation seems sub-optimal: no proper scale separation is used, and the use of the same observational data between the two steps implies non-zero cross-covariances between observations and background (in the second step). This seems theoretically sub-optimal and should be mentioned. Another issue is that the time dimension does not change between the two steps. It would be reasonable to assume that longer (broad scale) dynamics is associated to longer time scales, while here the 3-day time scale applies to both. Any thought about this? Again, maybe this is included in the manuscript submitted to Ocean Model., but without it being published it is worth to discuss.

MDT (lines 155-157): I understand this is computed as Mean SSH from a free-running-model run. This means that long-term mean barotropic-dominated transports (e.g. in ACC) in the reanalysis should look very similar to the control experiment, by construction. Some comments about this will be beneficial, as most other reanalyses use other strategies (either an "observed MDT" or one with assimilation of in-situ data)

Table 1. TEM vs TEM2 and SAL vs SAL2: the difference is not explained in the text.

Line 180: Doesn't sound better/Isn't more common to say "super-obbing" with double "b"?

Table 2,3,4. I really like the efforts in quantifying the error growth. Perhaps reporting all those values also in the figure 7,8 (in the profile panel) will help to better see the error growth and the differences with BRAN2016

Also, better to say immediately which observations are used to validate and form the statistics: are those assimilated in BRAN2016 only or also those supplemented in BRAN2020 (like sea mammals, etc.)? Both are possible choices in my opinion, but the interpretation of the results will be different.

Linked to this: lines 265-269 are not very clear to me. I don't understand why differences in skill scores improvements between surface and sub-surface data should lead to the authors' conclusions? It is because of the same atmospheric forcing/ingested surface data? Better to state clearly.

Line 323 typo in "reanalysed"

Line 331: SPINUP-EI is a misleading name. Perhaps change to CTRL or similar