

## Final Response Letter Template (EGU/Copernicus)

**Title:** Author Response to Referee Comment(s) on [egusphere-2025-3810](#)

**Date:** 28 January, 2026

**Authors:** Gennady A. Chepurin, James A. Carton, Luyu Sun, and Stephen G. Penny

**Manuscript Title:** SODA4: a mesoscale ocean/sea ice reanalysis 1980-2024

Dear [Riccardo Farneti](#) and Reviewers,

We would like to thank the reviewers for their constructive comments and suggestions, which have helped improve the manuscript. Below, we provide a point-by-point response to all comments.

### Response to Referee #3

#### Comment 1:

The manuscript describes the configuration of the new ocean reanalysis SODA4, which in comparison to its predecessor SODA3 features higher horizontal and vertical resolution, and which additionally assimilates sea ice data. The reanalysis is evaluated in terms of temperature and salinity changes, as well as transports at a few key locations.

SODA is a widely used product and updates are useful and descriptions of those are necessary.

The presented description/evaluation is very similar to those presented for previous products.

However, I am missing details of a comparison to the previous product SODA3, which would allow the reader to assess the level of advancement. Only the Gulf Stream paths are intercompared for this purpose.

**Response:** [The goals of this paper are somewhat different than the goals outlined by Referee 3. Our goals for this paper are: 1\) to document this mesoscale ocean/sea ice reanalysis, and 2\) to examine its basic performance. We have modelled our goals on those of Lellouche et al. \(2021, <https://doi.org/10.3389/feart.2021.698876>\) in their description of GLORYS12. The difference between that paper presenting GLORYS12 and this study presenting SODA4 is our expanded comparisons to independent observations \(e.g. section transports shown in Tables 2 and 3, Fig. S18 and sea level, Fig. S19\).](#)

[The reviewer asks about our comparison to the widely cited EN4. Our motivation for comparison to EN4 is that EN4 uses a similar profile data set but produces an analysis that explicitly lacks that part of the bias which comes from use of a dynamical forecast model. We think understanding the size and sources of bias is critical. We think the reason why climate documents such as the Gulev et al. \(2021\) IPCC report only rely on products](#)

similar to EN4 and ignore the reanalyses like SODA and ORAS5 is because of their concern about just this issue.

The reviewer asks for additional comparison of SODA4 to the ¼-deg reanalyses such as SODA3 and ORAS5. We do think such a study will be interesting and important. The results of such a study will also be strongly regional. For example, a student, Shaun Eisner, has already produced three such studies for the Arctic alone (e.g. Eisner et al., 2025 <https://doi.org/10.5194/egusphere-2025-5737>). Such comparisons could be carried out in many other regions to examine the impacts of changing horizontal and vertical resolution. Differences would then need to be explained in terms of altered regional physics, modified data assimilation, etc.

**Comment 2:** For temperature and salinity, the manuscript also offers only an intercomparison to the objectively analysed EN4 product instead of an evaluation with the EN4 or other in situ data.

**Response:** We think the reviewer is asking for a comparison to the EN4 or WOD observation sets (the data sets are almost the same). The data assimilation cycling carried out to produce SODA4 automatically produces those data comparison statistics in the form of temperature and salinity analysis increments. Presentation of the analysis increments is included in the discussion of Fig. 6 (lines 161-170), and in supplementary materials (see Figs. S8-S10).

**Comment 3:** Based on only intercomparisons, the evaluation of advancement and quality of the product is difficult.

**Response:** The goal of this paper is not to present a comparison of SODA4 vs SODA3 to address the issue of ‘advancement’. To summarize, the paper goals are: 1) to document this mesoscale ocean/sea ice reanalysis, and 2) to examine its basic performance (and thus quality), similar to what is presented in Lellouche et al. We extend beyond what is included in Lellouche et al to include comparison to independent observations. How much can be learned from SODA4 beyond SODA3 or ORAS5 will become more clear after people have worked with them for a few years.

**Comment 4:** EN4 objectively analysed data is flawed in many ways, as it is also acknowledged in the manuscript. Therefore, the purpose of the intercomparison becomes ambiguous. It may either show the advantage of using a dynamical model for an analysis or an evaluation of the SODA4 analysis. I suggest to include a comparison to the EN4 in situ data and the previous SODA3 product as it has been done before. The purpose of the intercomparison to the EN4 analysis could be clearer. More details are below.

**Response:** The comparison to EN4 is for the purpose of addressing possible forecast model bias (Introduction, Lines 19-21 and in Discussion) while the examination of the analysis increments is for the purpose of evaluating the degree of mean and variable observation minus analysis misfit.

While GMD does not place strict limits on article length we note that the main text is already the length of a typical full model description (6695 words with three tables and 11 multi-panel figures while the Supplement adds an additional 2376 words, seven tables, and 19 multi-panel figures). We think additional comparisons and associated explanations belong in separate papers that can dig into, and interpret, regional differences (such as Eisner et al., 2025 <https://doi.org/10.5194/egusphere-2025-5737>).

## Details

**Comment 5:** l 49-50 I cannot find anything about discharge in this reference. Please clarify or correct.

**Response:** Reference moved to the middle of the sentence to prevent confusion. The sentence now reads: “SODA4 has been developed to address these limitations by increasing model resolution to  $0.1^{\circ} \times 0.1^{\circ} \times 75L$ , upgrading the observation sets to include the 18.6 million profiles contained in the World Ocean Database 2023 (Mishonov et al., 2024), and adding improved estimates of continental discharge”.

**Comment 6:** l 50-51 I suggest to include two sentences about what Fig. 1 shows. Maybe a comparison to observations could be included to provide an idea about the achieved realism.

**Response:** We have added a sentence highlighting one of the impacts of enhanced resolution (L50-51). We don't want to give the impression that the paper will present a more extended comparison of SODA3 and SODA4 (again this is not the purpose of this paper).

**Comment 7:** l 60-62 How about the Southern Ocean <https://doi.org/10.1175/JCLI-D-14-00353.1> describes 1/10 as resolving equator ward of 50N/S

**Response:** Sentence has been expanded to note a similar problem at southern latitudes. The sentence now reads:

“For much of the global domain this resolution satisfies the requirements of Hallberg (2013) to be considered ‘eddy resolving’ but reduces to ‘eddy permitting’ in the Arctic and the deep Southern Ocean due to the decrease in the eddy length-scale”.

**Comment 8:** l 86-89 Why would this be a good idea ? Is there a reason to believe that the evaporated water is brought back by the rivers into the same basin? Why evaporation and not E-P? How does this bring the flux in alignment with the salinity trends The Atlantic is believed to import freshwater, It seems you put a constrain on this which would rather be something the model should deliver you., but I don't understand what has been done. l 88-89 I am missing more details how it was carried out, S1 states the same details about the balancing as provided here.

**Response:** We have modified the main text (L. 89-92) to clarify that our continental discharge adjustments to account for ungauged discharge following the procedure described in Dai and Trenberth (2002), who in turn refer to the work by Fekete et al. (2000)

who use the ECMWF reanalysis to calculate gauged/ungauged ratios. Like Dai and Trenberth, we also find that the Fekete et al ratios give reasonable global continental discharge estimates. The supplemental materials text S1 has also been modified to be consistent with this description. The paragraph in S1 now reads:

To address this problem we first attempt to find alternate sources of monthly discharge for major river systems. Fig. S4 shows the Dai and Trenberth record of monthly discharge for the Ob and Yenisei rivers in comparison to the SODA4 discharge updated based on data from arcticgreatrivers.org (Shiklomanov et al., 2020). We also add monthly discharge from ice sheets such as Greenland which are entirely missing from Dai and Trenberth (Fig. S5). Many gaps remained after this initial exercise. The remaining gaps we fill with climatological monthly values, inflating the implied discharge rates to account for the ~30-40% of the discharge that Dai and Trenberth (2002) suggest is ungauged. The final SODA4 monthly time series of global discharge is shown in Fig. S6. Its time mean,  $1.14 \times 10^6$  m<sup>3</sup>/s, falls at the lower end of the span of recently published estimates: 1.1-1.4x10<sup>6</sup> m<sup>3</sup>/s (Syed et al., 2010; GRDC, 2014; Wilkinson et al., 2014; Müller-Schmied, et al. 2014).

**Comment 9:** l 146 Aren't the Rs the scales and the deltas the distances ?

**Response:** yes, thank you. Fixed

**Comment 10:** l 170-171 How is the heat flux modified, via a relaxation term depending on the sea ice thickness difference to the data?

**Response:** The relaxation term is described in (L177)

“In SODA the net surface heat flux is modified so that ice which is too thin receives less heat and ice that is too thick receives more heat with a relaxation time-scale of 25 days.”

**Comment 11:** Section 3 I think details of the output files would be better presented on the web page that provides the data, best with a doi.

**Response:** This was also suggested by another reviewer. The text has been moved out of the main text and into Supplementary Materials, text S2.

**Comment 12:** Section 4.1 As EN4 may have larger errors relative to than SODA4, it is not clear what this comparison evaluates. Particularly in sparsely observed locations EN4 often falls back to climatology.

**Response:** As discussed in the responses to reviewer 3's comments above, a major motivation for our comparison to EN4 is quantification of forecast model bias. We attempt to emphasize this in our discussion of the comparison to EN4 in 5. Discussion, L289-300.

**Comment 13:** Is the comparison an illustration of what additional information SODA4 offers or and evaluation? It would be good to give the reader some guideline.

**Response:** We attempt to summarize the distinction between SODA4 and the widely cited EN4 and to describe what additional information has been gained from the comparison to SODA4 in L. 293-297:

EN4.2.2 is constructed using an archive of historical temperature and salinity observations that is similar to the World Ocean Database, but without use of a numerical forecast model and thus lacks that potential source of forecast bias. Basin-average comparisons show that temperatures are quite similar and salinities are also similar in the 0-300m layer but less so in the 300-1000m layer. The latter improves after Argo observations become available in the early 2000s except in the Arctic basin which is not sampled by Argo.

**Comment 14:** Figures Why sometimes temperature is K and sometimes in °C? Changes are sometimes in  $W^2/m$  and sometimes in °C/yr

**Response:** The units have been allowed to vary a bit in order to reflect how we think the users will use different results. For example, Fig. 6 shows time mean vertically integrated temperature and salinity increments in units of  $W/m^2$  and mm/dy rather than K/s and  $Kg/(m^3s)$ , which are more common units for heat and water flux. Figs. 8-10 shows temperature deviation, temperature, and temperature trend in K and K/yr because that seems the units most commonly used for such figures.

**Comment 15:** l 250 It says Drake Strait in Fig. S17

**Response:** Changed to 'Drake Passage' throughout the text and supplementary material.

**Comment 16:** l 262-265 Given that a large part of the AMOC variability is related to Ekman transport and resulting in RAPID and SODA4 from ERA5 the agreement between RAPID and SODA4 is surprisingly low. Is the Florida Strait transport or the upper midocean transport responsible for this?

**Response:** We think the reviewer is referring to supplemental materials figure S18 which compares time series of AMOC overturning transport. The main text only presents the time mean). We have added a brief note to the figure S18 legend indicating the SODA4 overturning transport may be impacted by the change in advection scheme prior to 2015 since the calculation involves integration over the full water column.

Here is the revised Legend:

**Fig. S18** Atlantic meridional overturning circulation across 26°N ( $10^6 m^3/s$ ). (blue) SODA4, (black) RAPID time series. The time mean SODA4 overturning transport is:  $16.5 \pm 0.4 \times 10^6 m^3/s$ . The overturning transport may be impacted by the change in advection scheme prior to 2015 (see Fig. S11b).

**Comment 17:** Table 3 How is the overturning defined. Is it at a fixed depth (which?) or the maximum? And is it in density or z-coordinates?

**Response:** A clause has been added to L253-5 giving our definition of the overturning transport

Finally, we compare time mean meridional overturning transports (defined as a maximum of the zonal integrated stream function) across three meridians in the Atlantic.

**Comment 18:** L 282-293 I think for an evaluation of SODA4 a comparison to the actual in situ temperature is required. With the last sentences you seem to evaluate EN4 with SODA4. It's odd to change the reference based on occasion.

**Response:** As discussed at the bottom of the first page of this response to reviewer 3 the temperature and salinity analysis increments are the misfits with respect to the in situ temperature and salinity. They are discussed in the text surrounding Fig. 6 as well as supplemental materials Figs. S8-S10.

**Comment 19:** L 313 SODA4

**Response:** fixed.