Manuscript " Modelling the water isotopes distribution in the Mediterranean Sea using a high-resolution oceanic model (NEMO-MED12-watiso-v1.0): Evaluation of model results against in-situ observations"

Mohamed Ayache[1], Jean-Claude Dutay[1], Anne Mouchet[2], Kazuyo Tachikawa[3], Camille Risi[4], and Gilles Ramstein[1]

[1]Laboratoire des Sciences du Climat et de l'Environnement, CEA-CNRS-Université Paris Saclay, 91191, Gif-sur-Yvette, France
[2]Freshwater and OCeanic science Unit of reSearch (FOCUS), Université de Liège, B-4000 Liège
[3]Aix Marseille Univ, CNRS, IRD, INRAE, Coll France, CEREGE, 13545, Aix-en-Provence, France
[4]Laboratoire de Météorologie Dynamique, IPSL, CNRS, Sorbonne Université, Paris, France

Correspondence: Mohamed Ayache (mohamed.ayache@lsce.ipsl.fr)

Reply to editor comments

Dear Mr. Yool,

We would like to express our gratitude for the time you have dedicated to reading and evaluating our paper at each stage since its submission. Your comments have significantly enhanced the quality of our paper. We have incorporated your comments into our revisions, as detailed below.

Color code

Editor comments

Authors response

The modifications performed in the manuscript appear in red in the Revised Manuscript with Changes Marked.

In your response to the first comment (actually a set of questions) from referee #2, you provide an extensive answer to these points. However, the resulting text modifications are quite short in contrast – for instance, you mention LDMZ several times in your response, but this does not appear to occur in your modified text. If I have overlooked this, please let me know, but if relevant information has not been included in the manuscript can you please address this.

We would like to thank you for your comment. We have clarified and better presented our experimental protocol in the revised version of the paper as requested by reviewer #2. Section 2 has been largely rewritten in the revised version of the paper.

We have incorporated the following sentences on the dynamic forging (see lines 150-165 in the track changes version):

The simulation was conducted over 30 years following a 44-year spin-up period (1958–1980 repeated twice), ensuring model stability for over 75 years. The years of hydrodynamic forcing were randomly selected from precalculated circulation fields spanning 1958 to 2013 (Beuvier et al., 2012a). The objective of this method is to minimize the impact of extreme variability effects, such as the Eastern Mediterranean Transient (EMT) or the Western Mediterranean Transition (WMT), on the simulated circulation (Roether et al., 2006; Schroeder et al., 2008). The spin-up strategy was adapted from previous passive tracer simulations, such as neodymium and tritium studies (Ayache et al., 2015a, 2016).

and these sentences, on atmospheric forcing (see lines 170-175 in the track changes version).

The aim is to assess the model's performance in the present climate and against in-situ data observed between 1982 and 2022. Therefore, we have opted to use the climatological mean of the LMDZ-iso 1990-2020 simulation as boundary conditions. This choice was made to minimize the warming trend during this period and to ensure that the precipitation and evaporation simulated by the LMDZ-iso model for the current climate situation are as close to the average state as possible, with minimal impact from inter-annual variability.

On the same point, there are a few minor issues with the added text:
- "a random draw" – what is meant by this? It's not clear to me at all

The term 'random draw' refers to the process of selecting years randomly from precalculated circulation fields of the Mediterranean Sea spanning 1958 to 2013 (Beuvier et al., 2012a). This approach is employed to simulate a consistent and random representation of present-day circulation. By incorporating these random selections into the historical period (1958–2013), the goal is to minimize the impact of intense variability events like the EMT or the WMT on the circulation simulations.

We have clarified in the revised manuscript, as shown below (see lines 150-165 in the track changes version):

The simulation was conducted over 30 years following a 44-year spin-up period (1958–1980 repeated twice), ensuring model stability for over 75 years. The years of hydrodynamic forcing were randomly selected from precalculated circulation fields spanning 1958 to 2013 (Beuvier et al., 2012a). The objective of this method is to minimize the impact of extreme variability effects, such as the Eastern Mediterranean Transient (EMT) or the Western Mediterranean Transition (WMT), on the simulated circulation (Roether et al., 2006; Schroeder et al., 2008). The spin-up strategy was adapted from previous passive tracer simulations, such as neodymium and tritium studies (Ayache et al., 2015a, 2016).

- "(for more than 75 of run)" – simulation years one assumes?

Corrected

- "The spin-up strategy was adapted FROM our previous passive tracer simulations"

Changed

In your response to point #4 from referee #3, you again provide an extensive and detailed response to the issues raised, but again it is unclear if or how the manuscript has been revised to account for this. Please make this clearer.

Agreed,

These sentences was added to the revised manuscript

(see lines 127-131 in the track changes version).

The isotopic composition is determined on post-processing because here we transport the isotopic ratio (see equation 1), which allows us to carry a single tracer "^{18}R" instead of two tracers "18O and 16O". This reduces the computation time on the machine, which is a crucial factor in the performance of the model, especially in a very long palaeo-simulation. It is a common practice to transport the isotopic ratio rather than the individual species. For example, radiocarbon distribution (14C/C) in the Mediterranean Sea (Ayache et al., 2017) and $^{18}O/^{16}O$ of precipitation (Risi et al., 2010b).

and these sentences (see lines 209-214 in the track changes version).

In our study, we utilized the off-line uncoupled mode of NEMO, which employs pre-calculated dynamics. This mode operates with a fixed volume and explicit fluxes of evaporation, precipitation, and runoff. Alternatively, the online coupled mode of NEMO can be employed to compute dynamic variables (such as circulation fields U, V, and W) in real time. The sea surface elevation and model layer thicknesses are adjusted by the freshwater flux (E-P-R), consequently affecting the model volume. It is essential to ensure that total volume variations accurately correspond to the E-P forcing used to drive the isotopic module, thus maintaining the perfect conservation of tracer content.

Your response to point #7 from referee #3 on the value of pCO2 used is both ambiguous and unclear on whether it has led to any manuscript amendment for clarification. I appreciate that the 348 ppm value is that from around year 1982, but go on to confuse things by referring to comparison time points up to year 2022. Can you please make clear – and do so in your manuscript – why the value was chosen, and whether it's constant regardless of simulation time, which it what's implied in your response.

We fully agree with this remark. The LMDZ-iso Atmospheric simulation was conducted following the Atmospheric Model Intercomparison Project (AMIP) protocol, as presented in Risi et al. (2010b), utilizing prescribed monthly and interannually varying SST and sea ice, in addition to a constant CO2 value of 348 ppm for the present-day situation. The same values have been employed in this study because their impact is constrained on our results, given that the model has been evaluated against in-situ data sampled primarily in the 1980s (i.e. Pierre et al. 1999). While in-situ data from more recent periods 1998-2022 from Reverdin et al. (2022) are more limited and only localized in the northwestern Mediterranean Sea.

This point is clarified in the revise manuscript.

(see lines 166-170 in the track changes version)

The LMDZ-iso Atmospheric simulation was conducted following the Atmospheric Model Intercomparison Project (AMIP) protocol, as presented in Risi et al. (2010b), utilising prescribed monthly and interannually varying SST and sea ice, in addition to a constant CO2 value of 348 ppm for the present-day situation. The impact of these low pCO2 values in comparison to the current value of 421 ppm is constrained by the fact that the model has been evaluated against in-situ data sampled primarily in the 1980s.

Figure 1 uses a strange plotting style with large circular patches. As these patches overlap, it tends to have the result that the order in which the data is plotted matters, such that southernly data overlies more northerly data given a strange tessellating pattern. Is there any way that the panels could be replotted in a style that doesn't involve overlapping like this? It doesn't appear to be a problem in later figures.

Thank you for pointing this out. We've looked at the problem of judging between boxes in the current version of Figure 1 and found that the problem is due to saturation in the scale used to plot this figure. We've adjusted both the scale and the colour palette used (see the new version of Figure 1). We hope that these changes will resolve the problem that appeared in the previous version.

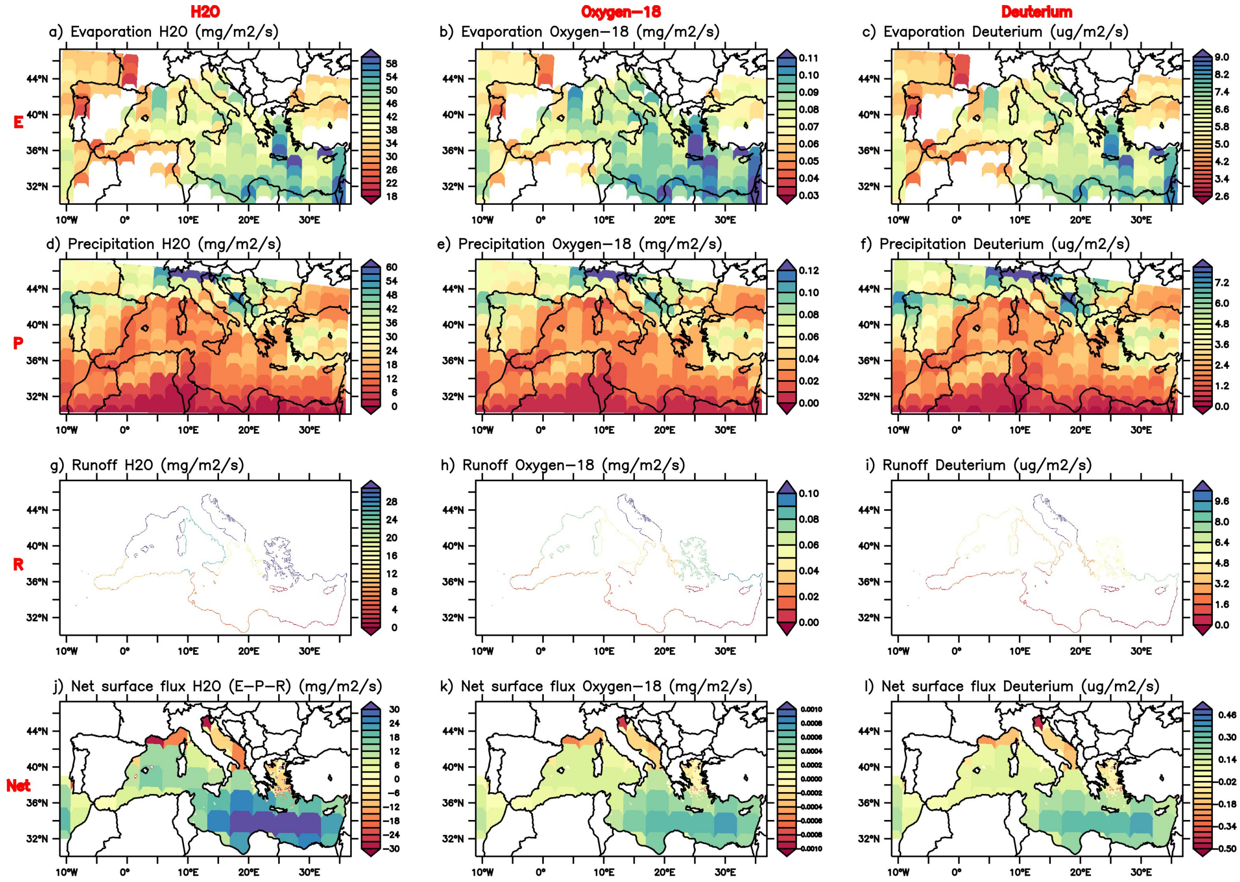

Figure 1 Boundary conditions and input (evaporation and precipitation) maps applied to NEMO that originate from the LMDZ-iso atmospheric model (Risi et al., 2010a). a) Evaporation, b) Precipitation, {c) River runoff, J) Net surface flux (E - P - R) for H_2O, (b, e, h, k) the same but for $\delta^{18}O_w$, (c, f, i, l) for δD_w. The isotopic composition of river runoff is not available from the LMDZ-iso model: this flux is computed as $^{18}RP \times R$ where R is prepared from the data of Ludwig et al., (2009) and Vorosmartyet al., (1996) and ^{18}RP is the isotopic ratio in precipitations at the same time and location

Finally, regarding the use of colour in the figures, this was noted by both the referees and by our production team. The change between revisions seems to have been to move from one rainbow palette to a more garish one, and the revised colour map is arguably inferior to that at the previous revision as it “wraps-around” such that the final colour is very similar to the first colour. Please consider checking the colour scales of your figures using our recommended colour blindness simulator …

https://www.color-blindness.com/coblis-color-blindness-simulator/

You may also wish to consult the ColorBrewer website for colour maps that are appropriate for colour blindness …

https://colorbrewer2.org/#type=sequential&scheme=BuGn&n=3

Please advise me if you have any technical challenges on this point. However, from the changes between manuscript revisions, it appears that the software you use is flexible on this point.

On more specific details, I note that Figure 1 has transitioned from a colourblind-friendly palette, Viridis / Parula, to one that makes use of red-green, while Figure 6 appears to show the old colour bar for a figure using the new colour scale.

We appreciate the feedback regarding the use of color in the figures. We understand the importance of ensuring that our visualizations are accessible to all readers. In response to your suggestions, we have replaced the colour palette with a new one, as shown in the revised version of our manuscript. We hope that this new palette will offer improved clarity. Furthermore, we have rectified the inconsistencies noted in Figures 1 and 6 to maintain coherence throughout the manuscript.