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 reviewers' comments

Dear Pr. I., Andrew Yool,

We would like to thank you for providing us the opportunity to revise our manuscript, and we are extremely grateful to Pr. Antje Voelker, Pr. Allegra N. LeGrande and the anonymous reviewer for their careful reading and comments that helped to improve our manuscript significantly.

We have revised our manuscript and provided a detailed response to each reviewer's comment and request below.

Color code
Reviewer comments
Authors response
The modifications performed in the manuscript appear in red above and in the revised manuscript with Changes Marked.

#3: **Review by Allegra N. LeGrande:**

Water isotope tracers are indeed a useful way to track the water cycle, and this study seeks to provide for high resolution insight into the Mediterranean ocean.
The authors of this study include expert isotope modelers, so the work is on the whole very solid.
I have Mostly few questions about the specifics of implementation and the write up.

1) for someone who is *not* a water isotope modeler, the casual inclusion of shorthand / jargon without explanation needs to be expressly defined. I.E., $\delta^{18}O_{sw}$ or δD (also—shouldn't you write δD_{sw} to be consistent?) or $CaCO_3$ or $\delta^{18}O_c$ all need to be defined – what does the delta mean. What do the subscripts mean. Some of the equation rendering has broken down maybe on the author's side, maybe on the Copernicus side.

We completely agree with Dr. Allegra N. LeGrande on this point, and we regret this lack of information, which is necessary for a better understanding of our manuscript. The same point was raised by Dr. Antje Voelker. We have added all the missing information in the revised version. $\partial^{18}O_{sw}$ stands for seawater, we change this abbreviation to $\partial^{18}O_w$ (use "w" for water). $\partial^{18}O_c$ we use c for calcite, and ∂D_w for deuterium. A table containing all abbreviations used in this manuscript has been added to the revised manuscript (ms)

See new Table 1

2) In the write up of previous work in the med for isotopes, the authors may (not?) be aware that there is almost certainly a mistake in the δD values of Gat as they vary much much less than $\delta^{18}O_{sw}$ – probably the original source should be sought out for that validation.

Thank you for bringing to our attention the discrepancy from the data of Gat et al. (1996). After verification, we have identified a problem with the sources used in the previous version of our manuscript. The shift has been corrected in the new version of our paper, as shown in the figure below (corrected data are plotted in green in panel e). We have also added new data in the western basin from Reverdin et al., (2022).
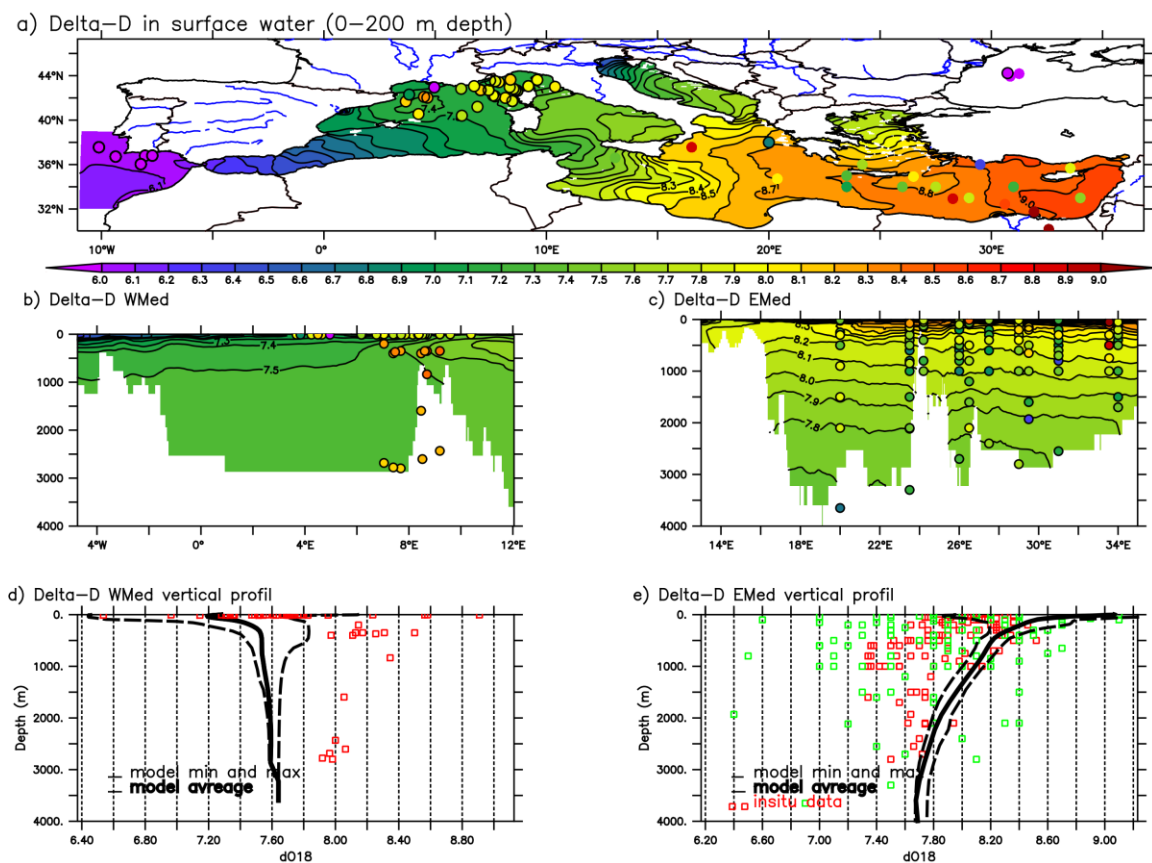

Figure 1 The model outputs against in-situ data for the present-day situation. a) δD_w (in ‰) distribution in the surface water (50 m depth). b) E-W vertical section of δD_w (in ‰) in the western Mediterranean basin d) Zonal mean comparison of δD_w (in ‰) average vertical profiles in the western basin presenting model results against in-situ data. c) and e) the same as b) and d) but for the eastern basin. Colour filled

dots represent in-situ observations from (Gat et al., 1996; Reverdin et al., 2022). Both model and in-situ data use the same colour scale.

3) When it is said that 'we use fluxes' from LMDZiso – that is surface water isotope fluxes? How are fluxes from rivers handled? Do you use observed isotope values or simulated ones? (Do the simulated river values closely approximate the measured ones?) If no measurements are available, what was done instead?

Ideally, the simulation of surface water isotope fluxes should be carried out using the land surface model ORCHIDEE. Isotopes are incorporated into the river discharge of ORCHIDEE, as described by Risi et al. (2016). However, the isotopic version of ORCHIDEE is outdated and cannot be coupled with the current version of LMDZ-iso. A joint project is currently underway to reintroduce water isotopes in the new versions of ORCHIDEE and to couple with LMDZ-iso.

In this scenario, we adopt an alternative solution proposed by Delaygue et al. (2000) to represent the isotopic flux carried by rivers to the ocean: this flux is calculated as $^{18}R_{river} = {}^{18}R_{precipLMDZiso} \times R_{runoff}$, where R is the ratio $^{18}O/O$, R_{runoff} is the same freshwater forcing as that used in the dynamical simulation (Beuvier et al., 2012; Palmiéri et al., 2015), and $^{18}R_{precipLMDZiso}$ is the isotopic ratio in precipitation at the same time and location. Monthly $^{18}R_{precipLMDZiso}$ runoff values of the 33 main river mouths covering the entire Mediterranean draining basin were computed using the climatological mean of the interannual dataset of Ludwig et al. (2009) and the RivDis dataset from Vörösmarty et al. (1996). This alternative approach has shown effectiveness both in the results presented in this paper and globally, as demonstrated by Delaygue et al. (2000). The advantage of this approach lies in its reproducibility across different timescales and locations, as well as its applicability to paleoclimate studies where observed isotope values from rivers are very limited.

Following the reviewer's suggestion, we conducted additional sensitivity simulations to better evaluate the impact of $\partial^{18}O_{river}$ (please see the answer to question 9 below). A new section is added to the appendix to further elucidate this point (see Appendix E).

4) On page 5, they say "it is common to transport the isotopic ratio rather than the individual isotope…" then later "and pseudo-salinity fluxes". I don't know NEMO that well, but I am going to guess they are saying in a round about way that this ocean model has a rigid lid instead of a free surface. They should say either way. Because most isotope models do *not* in fact transport around concentrations of isotopes, they transport around mass. Sure – some models do not actually conserve mass – they are forever having to reimplement water isotopes in their code because they have virtual moisture or salt fluxes. Anyhow, those who *can* do indeed transport around mass not concentration. The per mil isotopic composition is determined on post-processing. Why? This is done so that the isotope / tracer code can have an exact replica of 'water' from the non-tracer code and this tracer can be 1:1 compared throughout the entire model to made sure mass isn't being gained/lost anywhere spuriously. Isotopic composition comes into play because SMOW is defined and fractionation at phase changes is defined. This is, in general, simpler for an ocean model where the mass of water is simply (MO – S), but if

It is important to note that NEMO (and OGCMs in general) have representations of concentration/dilution processes that depend on the context:

- In this study, we used the off-line uncoupled mode of NEMO (pre-calculated dynamics): in this case, we use the linear free surface (fixed volume) with explicit fluxes of evaporation, precipitation, and runoff (calculated according to Delaygue et al. 2000, see our answer to point 3 and 9 of Dr. Allegra N. LeGrande). In offline mode, the model-intrinsic evaporation and precipitation fluxes have to be switched off, since the tracers are already influenced by freshwater fluxes in the forcing.
- It's possible to use the online coupled mode of NEMO to calculate the dynamic variables (circulation fields U, V, and W) in real-time. The sea surface elevation and model layer thicknesses are modified by the freshwater flux (E-P-R), which in turn affects the model volume. It is crucial that the total volume variations precisely follow the E-P forcing used to drive the isotopic module to ensure the perfect conservation of tracer content.

An important issue when modeling isotopes is that of conservation. Since the ocean is not coupled to the atmosphere the tracer cycle is not closed. In consequence, drift occurs. A global correction must be applied based either on the instantaneous or yearly averaged imbalance of surface fluxes for each tracer. The drift due to the linearized free-surface equation and, if relevant, the Asselin filter, are corrected using a specified routine in NEMO. Technical aspects relative to the conservation of tracers in NEMO are not addressed here; they may be found in the NEMO engine webpage (https://www.nemo-ocean.eu/).

The boundary conditions at the ocean-atmosphere interface are provided by an atmospheric GCM with a comprehensive representation of water isotopes (LMDZiso GCM; Risi et al.,2010). They consist of climatological gross fluxes of evaporation and precipitation with their isotopic composition.

The isotopic composition is determined on post-processing because here we transport the isotopic ratio (see equation 1), this allows us to carry a single tracer "^{18}R" instead of two tracers "^{18}O and ^{16}O", which saves computing time on the machine, this point is very important for model performance when using this water isotope package in the coupled model and in very long paleo simulations. It is common practice too to transport the isotopic ratio rather than the individual species. e.g., radiocarbon distribution "$^{14}C/C$" in the Mediterranean Sea (Ayache et al., 2017) and the isotopic composition of water vapor in the advection scheme of LMDZ (Risi et al., 2010b).

Water isotopes behave like conservative tracers in the ocean; they are only modified by fluxes through open boundaries (Craig and Gordon, 1965; Schmidt, 1998; Delaygue et al., 2000; Roche et al., 2004). Isotopic fluxes in and out of the ocean are associated with water transfer at the ocean-atmosphere and land-ocean boundaries.

In numerical modeling, a time step refers to the discrete increment of time over which the model's equations are solved. The choice of time step is crucial as it can impact the accuracy and stability of the model's simulations

In this study the fields of physics variables are read and interpolated at each model time step, i.e., the circulation fields (U, V, W) previously computed by the dynamical model are read daily and interpolated to give values for each 20 min time step. NEMO-related forcings are provided at a day-frequency while isotopic-related fluxes are given on a monthly basis.

We chose a lower frequency of atmospheric forcing compared to NEMO forcing to evaluate model performance in the current climate state against in-situ data randomly observed between 1982 and 2022. Also, the high-frequency coupling could only be performed using an on-line coupled model (which is not currently possible). Consequently, we chose to use the climatological mean of the LMDZ-iso 1990-2020 simulation as boundary conditions. This choice aims to minimize the warming trend and to ensure an average state of precipitation and evaporation, thus reducing high-frequency variability.

Additional details have been incorporated into the revised manuscript to further clarify this aspect:

See section 2.2 lines 140-144

"The physical forcing fields are readed and interpolated at each model time step, i.e., the circulation fields (U, V, W) previously computed by the dynamical model are read daily and interpolated to give values for each 20 min time step. NEMO-related forcings are provided at a day frequency while isotopic-related fluxes are given monthly (see below for the atmospheric forcing)."

And section 2.3 lines 166-171

"The aim is to assess the model's performance in the present climate and against in-situ data observed randomly over the historical period. 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."

6) I'm still confused about the pseudo-salinity tracer. Please explain

The water fluxes from the stand-alone (non-coupled) experiments with LMDZiso are not identical to those constraining NEMO-Med12. Hence $\delta^{18}O_w$ or δD_w computed with the water fluxes obtained with LMDZiso would not be consistent with the salinity predicted by NEMO-MED12. For this reason, we compute a "pseudo salinity" S_w (Delaygue et al., 2000; Roche et al., 2004). This additional passive tracer does not affect the ocean dynamics. Its sole purpose is

to allow a coherent assessment of the relation of the isotopic fields predicted by the model with salinity since they are computed with the same fresh-water forcing.

The evolution equation for S_w is given by:

$$\rho_0 K \bigtriangledown S_w|_{z=\eta} = (\mathcal{E} - \mathcal{P} - \mathcal{R} + |\mathcal{I}|)S_w - ({}^{S_w}\mathcal{I}).$$

with the further assumption that the salinity associated with evaporation, precipitation, and run-off is zero (no effect of freezing/melting on the concentration/dilution of pseudo-salinity in the Mediterranean Sea), the boundary condition for salinity reads.

$$\rho_0 K \bigtriangledown S_w|_{z=\eta} = (\mathcal{E} - \mathcal{P} - \mathcal{R})S_w$$

The basic understanding of these atmospheric fluxes is that evaporation tends to increase the surface salinity, and the 18O/O ratio, in contrast to precipitation and runoff.

Below, we have plotted the anomaly in salinity-pseudo-salinity to assess the correspondence between pseudo-salinity results and standard modeled salinity. The well-known east-west gradient is effectively captured by recalculated pseudo-salinity, showing very similar values to those of standard salinity. Minor deviations are noticed in the Gulf of Lions and the Algerian Basin, attributed to overlooked mesoscale activity impacts in the global LMDZiso simulation. Overall, the pseudo-salinity globally yields values highly comparable to standard simulated salinity.
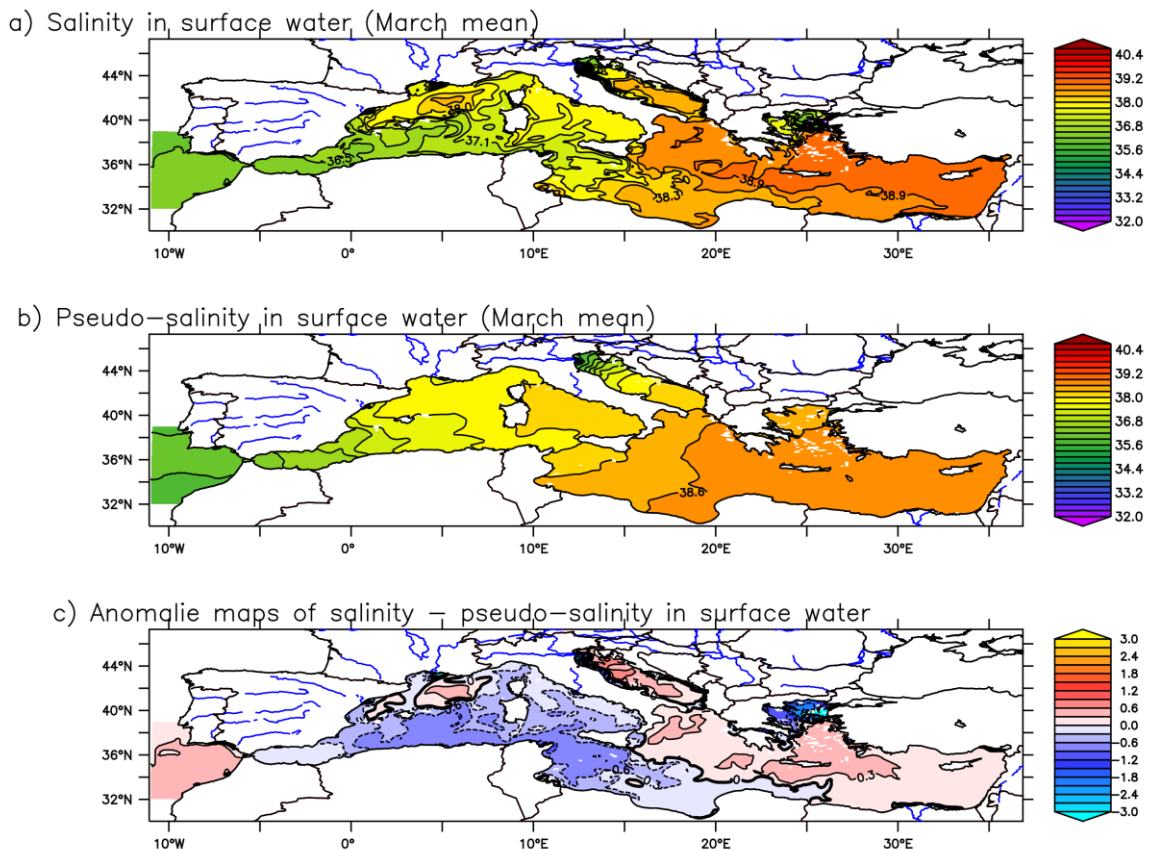

Figure 2 a) Standard simulated salinity from NEMO-MED12 in the surface model. b) Pseudo-salinity simulated in the surface water. c) the anomaly a) - b)

This figure is included in the appendix of the revised manuscript, accompanied by additional details to provide a clearer explanation of the pseudo-salinity concept (see Appendix D).

We agree with the point made. It is evident that the value of 348 ppm used is significantly lower than the current value of 421 ppm. We have used this value because here we evaluate model performance against in-situ data observed at different times between 1982 and 2022.

8) NEMO-MED12 grid is jargon that I don't understand.

The term "NEMO-MED12 grid" refers to the specific configuration of the NEMO model that is used for the Mediterranean Sea (Beuvier et al., 2012). The "MED12" part of "NEMO-MED12" indicates that this configuration of the model has a resolution of 1/12°.
The NEMO-MED12 grid is an extraction from the global ORCA-1/12° grid. This corresponds to a grid cell size between 6 to 7.5km from 46°N to 30°N and represents a grid size of 567×264 points. NEMO-MED12 covers the whole Mediterranean Sea plus a buffer zone including a part of the near Atlantic Ocean, from 30°N to 47°N, and from 11°W to 36°E. The Black Sea is not represented. Clarified in the revised ms (see section 2.1, lines 87-88).

"The NEMO-MED12 grid is an extraction from the global ORCA-1/12° grid. This corresponds to a grid cell size between 6 to 7.5km from 46°N to 30°N and represents a grid size of 567×264 points."

9) Still confused on L165-170 how the isotopic composition for the rivers was determined. It sounds like you are saying that the isotopic composition of river discharge = local grid box precipitation isotopic composition (which would be wrong of course). Can't you use observations *or* use d18Oriver from LMDZiso (or another isotope enabled model). Since you have already established that the Med is an evaporative basin, you might expect that d18Oriver to be a bit enriched compared to d18Oprec… (Places downriver or downhill in a P>E location you would expect d18Oriver to be a bit depleted compared to d18Oprec…) But the Med, and particular places like the Nile, you definitely should expect some evaporation to strip out the light isotopes of the river.

Thank you for your analysis and suggestions regarding the isotopic composition of the runoffs. In addition to our response to question 3, here are some key points to clarify:

- In response to the reviewer's suggestion, we conducted sensitivity simulations to assess the impact of computing the isotopic composition of rivers based on the isotopic composition of precipitation. Two new experiments (EXP1 and EXP2) were conducted using output from an earlier version of LMDZiso coupled to ORCHIDEE-iso (cf. Risi et al., 2016) at a lower resolution of R96x71.
  - EXP1: Employed the approach described in our submitted paper, where $^{18}R_{river} = {}^{18}R_{precipLMDZiso} \times R_{runoff}$
  - EXP2: Integrated the simulated $\delta^{18}O$ of rivers from the older version of LMDZiso at R96x71 resolution, where $^{18}R_{river} = {}^{18}R_{river} \times R_{runoff}$
  - Here, R is the ratio $^{18}O/^{16}O$, $^{18}R_{precip}$ and $^{18}R_{riverLMDZiso}$ are derived from LMDZiso (R96x71, Risi et al., 2016), while R_{runoff} is from the interannual dataset of Ludwig et al. (2009) and the RivDis dataset from Vörösmarty et al. (1996).

- The results of these sensitivity simulations are shown in the figure 9 below. In EXP1, the model reproduces a reasonable east-west gradient similar to our results using a higher version of LMDZiso (R96), as shown in Fig. 2a of the submitted paper. In EXP2, the addition of the $\delta^{18}O$ of rivers simulated by LMDZiso reveals a more enriched $\delta^{18}O_{river}$ compared to $\delta^{18}O_{precip}$, as predicted by the reviewer. Indeed, evaporation can enrich heavier isotopes in remaining water, including rivers, which is particularly evident for the Po river, exhibiting a clear positive anomaly around 0.5‰ near the coast and dispersed over the Adriatic Sea. The impact of other main rivers (e.g., Rhone and Po) remains very close to the coast, rapidly dispersed by circulation.
- However, the impact of the Nile significantly influences the $\delta^{18}O_w$ signal simulated in EXP2, highlighting a well-known issue in ORCHIDEE concerning the simulation of Nile discharge, where ORCHIDEE tends to largely overestimate the discharge, as depicted in figure 9 below.
- Consequently, we opted not to utilize the global version of LMDZiso due to the complex hydrology of the Mediterranean region. Instead, we employed a combination of model outputs and in-situ data to estimate the runoffs entering the Mediterranean Sea. For the isotopic composition, we adopted the same approach used by Delaygue et al. (2000).
- In conclusion, these sensitivity simulations (EXP1 and EXP2) showed an enrichment of $\delta^{18}O$ in the rivers due to evaporation, especially for the Po. The influence of the Nile significantly affects the signals, which has prevented the use of this version of LMDZiso (R71) and we are unable to couple this old version of ORCHIDEE (outdated) with the current version of LMDZiso. Therefore, the approach of Delaygue et al. 2000 was chosen over the data for its reproducibility and usability in paleo simulations.
- A new section is added to the appendix to further elucidate this point (see Appendix E). and we have mentioned this limitation in the conclusion of our paper lines 476-479:

"Here we calculate the isotopic composition of rivers based on the isotopic composition of precipitation, which means that the enriched $\delta^{18}O$ in rivers due to evaporation is not included in our simulation. It is recommended that a future study better represents the $\delta^{18}O_{river}$ (see Appendix E)."
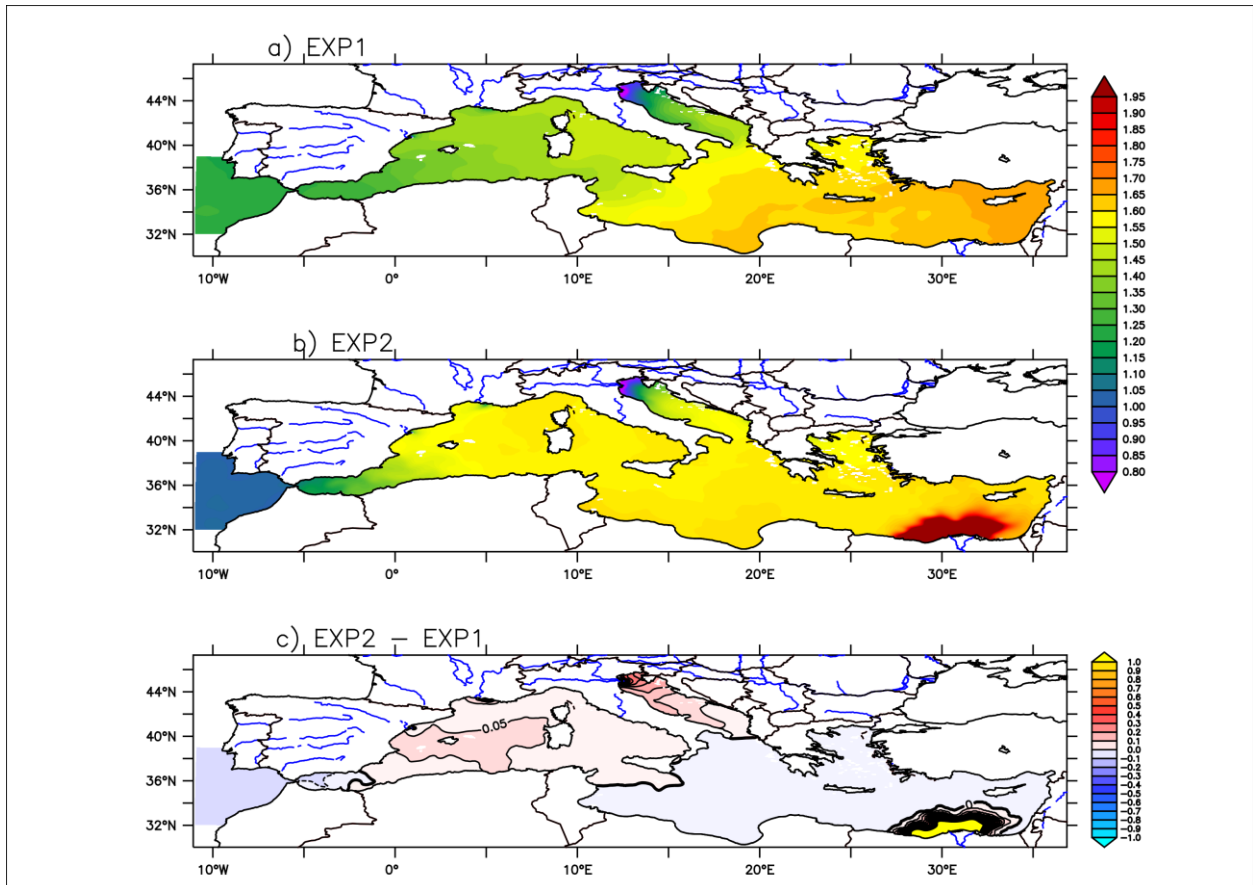
Figure 3 a) EXP1: we use the same approach as described in our submitted paper, i.e.,   18Rriver= 18Rprecip × Rrunoff. b) EXP2: we added the d18Oriver simulated by the old version of LMDZiso at lower resolution R96x71. 18Rriver=18Rriver × Rrunoff. C) the deference EXP2 − EXP1.

10) Can you write up the E-W surface d18Osw context from obs ? Maybe putting observed d18Oriver would make for a better gradient. (The baseline composition is set by your SMOW definition—I'd worry less about that.)

For this study, we've opted not to rely on $\delta^{18}O_{river}$ observations and utilize the framework outlined by Delaygue et al. (2000), but we agree that this is a limitation of this study, and we now stated this limitation in the conclusion of our paper, lines 476-479:

"Here we calculate the isotopic composition of rivers based on the isotopic composition of precipitation, which means that the enriched $\delta^{18}O$ in rivers due to evaporation is not included in our simulation. It is recommended that a future study better represents the $\delta^{18}O_{river}$ (see Appendix E).".

In the future, the ongoing project at IPSL, aimed at updating and integrating various components of the IPSL model (LMDZiso, ORCHIDEEiso, and NEMOiso), will undoubtedly enhance the representation of $\delta^{18}O_{river}$ in future studies.

11) For deriving d18O-S relationships – can you put yours in context of the LMDZiso? Would you expect NEMOiso to differ that much given that you are prescribing your end member from the coupled model? Is this a useful section?

I'm not sure if I've understood this question correctly !

We have prescribed the end members from E and P of LMDZiso and not from the IPSL coupled model, it's important to note a distinction between the global model and NEMO-MED12iso. As explained in the response to question 6, NEMO-MED12 operates as an eddy-permitting model, which is clearly shown in Fig.8a and Fig.8b of simulated sea surface salinity (please refer to answer 6 above).

12) For section 3.3 – can you please check the Gat96 comparison. Does it make sense?

Corrected, see answer 2 above. Again, we apologize for the delay in the Gat et al. (1996) data.

13) For the d18Ocalcite discussion, what is the correlation between d18Oc and temperature temporally and spatially. For interannual variability, does the inclusion of d18Osw confound the correlation. Also—you are presuming surface dwelling foraminifera. Maybe its interesting to look at species specifiic d18Oc.

Calcite $\delta^{18}O_c$ is widely used in paleoclimate research. Understanding its seasonal variability is crucial for reconstructing past climates. The influence of seasonal temperature variability on $\delta^{18}O_c$ (equation 6) is important, particularly in the Mediterranean Sea because of marked seasonal thermal contrast. The $\delta^{18}O_c$ values are determined by both $\delta^{18}O_w$ and the seawater temperature at the calcification depth. For planktonic foraminifera such as *Globigerinoides ruber* and *Globigerina bulloides*, the calcification depth typically ranges from 0 to 100 meters, though variations exist depending on the basin (De Castro Coppa et al., 1980; Grazzini et al., 1986). The season of maximal foraminiferal production can be estimated by data from sediment traps. For instance, *G. ruber* and *G. bulloides* have been associated with calcification seasons in October-November and April-May according to Kallel et al. (1997), while others suggest January-March (Avnaim-Katav et al., 2019) and February-April (Rigual-Hernandez et al., 2012).

In this context, we used our model results to explore the relationship between $\delta^{18}O_c$ and temperature. We employed a paleotemperature equation for inorganic calcite by Kim and O'Neil (1997), which was modified by Bemis et al. (1998), as shown in Fig. 10. Our simulations indicate that the highest $\delta^{18}O_c$ values occur during winter (February, March), while the lowest values are observed during summer/autumn. Although the available observational data do not cover all months of the year, our results align with existing data, highlighting the significant influence of temperature on $\delta^{18}O_c$ in the Mediterranean Sea. Nonetheless, a dedicated study should be conducted to further elucidate the seasonal aspect.

In the revised version of our paper, we have included additional sentences to provide clarity on the seasonality aspect of $\delta^{18}O_c$ (see section 4 lines 427-441).

"Calcite $\delta^{18}O_c$ is widely used in paleoclimate research. Understanding its seasonal variability is crucial for reconstructing past climates. The influence of seasonal temperature variability on $\delta^{18}O_c$ (equation 6) is important, particularly in the Mediterranean Sea because of marked seasonal thermal contrast. The $\delta^{18}O_c$ values are determined by both $\delta^{18}O_w$ and the seawater temperature at the calcification depth. For planktonic foraminifera such as *Globigerinoides ruber* and *Globigerina bulloides*, the calcification depth typically ranges from 0 to 100 meters, though variations exist depending on the basin (De Castro Coppa et al., 1980; Grazzini et al., 1986). The season of maximal foraminiferal production can be estimated by data from sediment traps. For instance, *G. ruber* and *G. bulloides* have been associated with calcification seasons in October-November and April-May according to Kallel et al. (1997), while others suggest January-March (Avnaim-Katav et al., 2019) and February-April (Rigual-Hernandez et al., 2012). In this context, we used our model results to explore the relationship between the $\delta^{18}O_c$ and temperature. We employed a paleotemperature equation for inorganic calcite by Kim and O'Neil (1997), modified by Bemis et al. (1998), as shown in Fig. 10. Our simulations indicate that the highest $\delta^{18}O_c$ values occur during winter (February, March), while the lowest values are observed during summer/autumn. Although the available observational data do not cover all months of the year, our results align with existing data, highlighting the significant influence of temperature on $\delta^{18}O_c$ in the Mediterranean Sea. Nonetheless, a dedicated study should be conducted to further elucidate the seasonal aspect."

For inter-annual variability, the inclusion of $\delta^{18}O_w$ can indeed confound the correlation with $\delta^{18}O_c$. This is because $\delta^{18}O_w$ is influenced by factors such as evaporation, precipitation, and runoff, which can vary on interannual timescales. However, we did not delve into interannual variability in this paper. It should be examined in a separate study. We now discuss this issue in the article: ". The aim is to assess the model's performance in the present climate and against in-situ data observed randomly over the historical period. 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".
See section 2.3 and lines 166-171 in the track changes version.

Regarding the presumption of surface-dwelling foraminifera, it's true that different species of foraminifera calcify at different depths in the water column (e.g Rebotim et al., 2019). Therefore, the $\delta^{18}O_c$ values can vary between species, reflecting the different environmental conditions at their respective depths (Rebotim et al., 2019). In our forthcoming paper, which focuses on paleo events known as sapropels, we are currently implementing a module (developed by A. Mouchet, University of Liege) to facilitate a direct comparison of the model with proxy data (species-dependent). This module operates under the assumption that each planktonic foraminiferal species prefers a specific range of depth and temperature, similar to the approach used by Schmidt (1999). At each time step and geographical location, the possibility of occurrence of a particular foram species is evaluated on the basis of its preferred temperature and depth ranges. This process allows us to determine the mean $\delta^{18}O_c$ along with the mean $\delta^{18}O_w$ and temperature experienced by foraminifera during their life cycle. Currently,

the module considers four planktonic foraminifera species: *G. ruber*, *Neogloboquadrina pachyderma, Neogloboquadrin incompta*, and *G. bulloides* (Schmidt, 1999; Lombard et al., 2011).

Additional details have been incorporated into the revised manuscript to further clarify this aspect (see section 4, line 427-441).

13) There are *some* existing SWING comparisons of different isotopic compositions for different groups. Maybe for your next paper you could pull those in, but for this one, you should at least mention and speculate if it would be useful.

 We have added a sentence in the conclusion to mention the SWING2 project (Risi et al 2012): "It would be interesting to compare how NEMO-MED12 responds to inputs from different isotope-enabled atmospheric GCMs, as documented in SWING2 (Risi et al., 2012). In addition, an intercomparison of results from different coupled models could be valuable as an extension of SWING2."

See section 4 lines 479-481

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