RC1, 'Comment on egusphere-2024-1468', Anonymous Referee #1, 18 Jul 2024.

The manuscript mostly concentrated on the Evaluation Run and its statistical properties by comparing the ocean model output with the available observations (SST, SLP, T). However, the manuscript missing a detailed analysis of the climate runs which is the main promise of the manuscript. The statistical analysis made for the Evaluation Run (Atmospheric forcings, Sea level variability and circulation patterns, Thermohaline properties, Extreme thermal events) should be repeated for all the climate runs.

In this work the model validation was given a large attention as the idea was not only to assess the validity and limitations of the finding on the climate part, but also to provide some guidance on the use of the ensemble dataset associated with the present paper. Nonetheless, we concur on the opportunity of having a broader view on the climate results and we have expand this section with particular reference to sea surface temperature, salinity, and surface heat fluxes statistics (Section 3.2 -now also renamed into "Climate historical runs and projected climate change signal").

In the framework of this expansion it is important to notice (and we made it clearer in the revised version, see for instance lines 99-105 in the track change file) that the methodology for evaluation runs does not fully apply to the historical runs. While the former are driven by a reanalysis, which means that the RCM forcing the ocean model receives as boundary conditions reanalysis fields constrained to the observed atmospheric variability, the latter are driven by GCM fields, which result from a free run incorporating the observed variability "only" in terms of radiative fluxes (consistently with the constraints that can/cannot be controlled in a climate perspective) and generating the internal atmospheric variability as a free evolution under this condition. As a consequence, while atmospheric variability in reanalysis-driven runs is synchronised with the observed variability (and therefore it is licit to perform a direct one-to-one comparison of model and observations – that is, for instance, modelled fields for one day should match observed data for the same day), in GCM-driven runs this is only valid in statistical terms, and the modelled variability is expected to only match the statistical properties of atmospheric processes corresponding to a given radiative forcing regime. This is the reason why some assessment of the performance of the climate runs was actually included in the first version (Figures 13 and 16, now 14 and 21) only in aggregated terms.

On this ground, and with an eye on keeping the manuscript within a reasonable length, in the revised version we will expand along the following lines:

- *Comparison of wind statistics in the climate run in CTR conditions against observations (Figure 5)*
- *Seasonal ensemble variations (percentiles) between SCE and CTR conditions in sea surface temperature (Figure 15), salinity (Figure 16) and net surface heat fluxes (Figure 17) .*
- *Differences in the statistical distribution of sea surface temperature for the different subdomains (Figure 18), particularly in the perspective of supporting the interpretation of the results in terms of thermal extremes (see also comments by Rev. 2 and our response).*

As it is known, the open boundary conditions (OBCs) for the ocean models are critical, especially in the Adriatic Sea, the small differences in the salinity and temperature specified at the OBCs significantly affect the dense water formation and physical properties. In the manuscript, how the OBC data were generated to force the ROMS model is not clear, need to be specified the methods and justified that could be used safely in a climate model.

We thank the reviewer for pointing this out. In the revised version we have expanded the "model setup" Section with a more detailed explanation of how we imposed the boundary conditions and their modulation in the future scenario, and some comments on their suitability for the purposes of this study (see lines 118 to 143 in the track change version). Flanking the description of the methodology adopted we include a new figure (Figure 2) showing potential temperature and salinity distributions along the boundary cross section together with the velocity contours, also considering the EV run. In order to check that the prescribed climatological variations are realistic, we also included a comparison of the average trends, finding that the multidecadal tendency for potential temperature in the climate run in the historical period is well bracketed between the evaluation values (namely, the CMEMS reanalyses used as boundary conditions), while salinity trends (less clear in the reanalysis) appear slightly underestimated in the climate runs. The thermohaline properties appear consistent with typical values from the literature, particularly in terms of Modified Levantine Intermediate Water (MLIW, see for instance Bonaldo et al., 2016, and references therein), now more generally identified as Eastern Intermediate Water (EIW, Schroeder et al., 2024). The known cyclonic flow across the boundary cross section is weaker in EV than in EV*, but is restored internally as geostrophic circulation (Figure 7), restoring the typical climatological values for MLIW around 0.10 m s^{-1} (Orlić et al., 1992; Artegiani et al., 1997). Furthermore, the subdomain-based analysis of the evaluation run in the Results section has been expanded focusing on the surroundings of the model domain boundary (red polygon in Figure 1), and the Taylor diagram (Figure 11) now includes an assessment of the model in that region, showing a good agreement with measurements, with skill metrics comparable with the AdriSC reference (Pranić, et al., 2021).*

RC2: 'Comment on egusphere-2024-1468', Anonymous Referee #2. Citation: https://doi.org/10.5194/egusphere-2024-1468-RC1

<u>Review of the paper</u>

The manuscript deals with the present and the end-of-century, kilometre-scale ensemble modelling approach for the description of ocean processes in the Adriatic Sea using and ensemble of climate runs in a severe RCP8.5 scenario forced by the SMHI-RCA4 Regional Climate Model driven by CMIP5 General Climate Models as well as evaluation runs for the 1987-2010 period.

The text is well written and results are presented clearly.

We thank the reviewer for this positive comment

The authors show that the main behaviour of the model used is ''satisfactory''. However scenario simulations show results that necessitate a deeper investigation of the role of the model set up and of the forcing. The choice of the lateral boundary conditions is also of a crucial importance.

A deeper investigation of the relatively high theta and S changes in the deep Adriatic is recommended. This is also the case of the weak change of MHWs shown.

We do appreciate the reviewer's suggestion. In fact, our positiveness lies mostly in the fact that the model performance against observations is comparable with the one exhibited by a state-of-the-art hindcast (AdriSC, referenced in the manuscript), which is a very good result in a climate model. The purpose of this manuscript and of the associated dataset is to pave the way to a number of studies on the many processes that take place in the Adriatic Sea. In principle the role of different factors (and in particular of atmospheric forcings and boundary conditions) depends on the process to be investigated (for instance, a study on river plume spreading in future conditions would not depend on the same processes and metrics as a study on marine heat waves or on dense water formation), and an extensive discussion on each of these aspects is beyond the scope of this manuscript and probably unfeasible for a single paper (see for instance the comment added in Lines 538-541 of the track change version). In this direction, the scope of the validation presented in the manuscript lies mostly in presenting the potential of the model and its dataset, the possible limitations, and in discussing the results presented in the climate scenarios. In the revised version we strove to better clarify the objectives of this paper and the necessary steps to be undertaken in the future applications.

Nonetheless, we agree with the reviewer that a somewhat deeper (though with an eye on keeping the manuscript to a reasonable size) general discussion on boundary conditions, atmospheric forcings, their role on the results and the implications for other applications would be beneficial for the paper and helpful in the use of the dataset, and we will follow the reviewer's suggestion. In the revised version we included in the "Model Setup" section (Lines 118 to 143 in the track change version) a more detailed explanation of how the boundary conditions were introduced and a check on the trend of the values prescribed in the climate simulations (reflecting the variability of the CMCC-CM profiles) against the ones prescribed in the EV and EV runs (reflecting the two versions of the CMEMS reanalysis), while a quantitative assessment of their quality is introduced in the Results in an updated version of the Taylor diagrams (see Figures 2 and 11 in the revised version). We point out that the thermohaline properties appear consistent with typical values from the literature, particularly in terms of Eastern Intermediate Water - Modified Levantine Intermediate Water (EIW or MLIW, see Schroeder et al., 2024, Bonaldo et al., 2016, and references therein), and the known cyclonic flow across the boundary cross section is weaker in EV than in EV* but in any case is recreated internally as geostrophic circulation (see also response to Rev. 1), restoring the typical climatological values for MLIW around 0.10 m s^{-1} (Orlić et al., 1992; Artegiani et al., 1997). Furthermore, the Taylor diagram (Figure 11) shows a good agreement with measured values also in the surroundings of the boundary, again with skill metrics comparable with the AdriSC reference (Pranić, et al., 2021).*

In addition, also in the direction of expanding the "climate" part of the manuscript as suggested by Rev. 1, we introduced some additional results from the climate runs.

An assessment of the wind regimes in the historical part of the climate runs (Figure 5) shows a good match with observations at sea in the Northern Adriatic also for the GCM-driven simulations, suggesting that overall realistic wind regimes are used also as a forcing for the climate runs.

The variations of ensemble seasonal percentiles between SCE and CTR conditions for sea surface temperature, salinity, and net surface heat fluxes (Figures 15 to 17), as well as the differences in the daily climatological CDFs for SST (Figure 18 – the equivalent for heat fluxes is less informative and has been omitted) in the different subdomains are introduced mainly to *enhance the analysis of the climate variability and the discussion of the results on thermal extremes. Nonetheless, complemented with the trends on the boundary conditions (Figure 2, panels c and f), they have been used to draw some considerations on the effect of local dynamics, river runoff and boundary conditions in the basin properties (see for instance Lines 478-484 in the track change version), although again recalling that fully disentangling the role of each factor in general is beyond the scope of this work.*

Elements along these lines have been introduced throughout the discussion in order to address the reviewers questions raised above, with all the single modifications visible in track change, particularly focusing on temperature changes and thermal extremes (less information could actually be found for salinity, at least for this purpose), the underlying assumptions, and the open challenges.

I recommend major revisions.

Specific comments:

-15. '' with particularly encouraging results''

Could authors explain to what extent results are encouraging?

The overall good capability of the model to reproduce the main features of observed Marine Heat Waves (MHWs) and Cold Spells (CSs), such as timing, intensity, and interannual variability suggests that our dataset could effectively be used for studies involving thermal extremes (e.g. linked to ecological processes, etc.). We rephrase this to make it clearer in the revised version (Lines 23-25 in the track change version).

-45. '' (?Denamiel et al., 2021a) ''

Please correct if needed.

Thanks, fixed

-50. '' ... ranging from the very evolution of the global...''

Could authors verify this sentence?

Thanks, modified into "...from the evolution of the global climate to how this signal propagates through different scales, and how the adopted numerical description impacts the final results". Lines 64-65 in the track change version.

-100. ''Potential temperature (θ), salinity (S), momentum..."

Could authors describe how momentum is used in the boundary condition set up?

In the revised version we will provide more details on the boundary conditions. In particular, we will point out that we imposed Chapman conditions (Chapman, 1985) for free surface, Flather conditions (Flather, 1976) for 2D momentum components, and nudged radiative conditions for 3D momentum components and tracers (potential temperature and salinity).

-105. ''...were modulated accordingly with the anomalies computed from Med-CORDEXderived CMCC-CMprofiles (Scoccimarro et al., 2011) in the norhtheasternmost grid cell of the Ionian Sea.''

Could authors better describe the approach followed?

Thanks, in the revised version we reshaped the description along these lines: "For the EV run, daily reanalysis values were directly interpolated on the model grid points throughout the cross section. For the climate runs, climatological monthly values were first computed from the reanalysis fields with reference to the 1987-2017 period. These values were then perturbed with the anomalies computed, with reference to the same period, from Med-CORDEX derived CMCC-CM profiles (Scoccimarro et al., 2011) in the northeasternmost grid cell of the Ionian Sea." See Lines 128-131 in the track change version.

-185. ''Furthermore, although being the only available option the evaluation of SMHI-RCA4, ERA-INTERIM known to be far from the "perfect boundary conditions" hypothesis, particularly in terms of rainfall-related quantities (Bao and Zhang, 2013).''

This sentence is rather unclear, could authors rephrase it.

We thank the reviewer for pointing this out, actually we were missing a verb! We apologise for that. We add the verb and slightly modify the sentence into "Furthermore, although being the only available option the evaluation of SMHI-RCA4, ERA-INTERIM does not presently represent the state of the art for atmospherical modelling, and is known to be far from the "perfect boundary conditions" hypothesis, particularly in terms of rainfall-related quantities (Bao and Zhang, 2013)". Line 222, track change version.

-210. Whereas the comparisons shown in Fig.2, Fig.3 and 4 show that the model behaviour is rather satisfactory as stated by the authors: ''thus performing significantly better than most of the RCMs available for this geographical area'', it would be interesting to illustrate this by one or two concrete examples.

We thank the Reviewer for helping us noticing that our reference to the RCA4 regional climate model (RCM) skill were probably slightly overenthusiastic. We have refined this phrase removing "thus performing significantly better than most of the RCMs available for this geographical area" from the original sentence. In fact, strictly speaking this actually represents an overstatement since a specific assessment based on a multi-RCM comparison over this particular region and variables lies outside the scope of the study. Nevertheless RCA4 shows overall representative skills for essential climate variables as preliminarily assessed in the context of a previously published article and involving similar geographical domain (Bonaldo et al., 2023) as well as in review articles, including the large CORDEX ensemble (Coppola et al., 2021; Diez-Sierra et al., 2022; Vautard et al., 2021), and specifically over the Adriatic region where Belušić Vozila et al., *(2019) consider wind climate variable specifically. These considerations and the related references have been added to the revised version of the manuscript (lines 252-256 in the track change version).*

Another element driving us towards using this model is that at the time in which climate simulations were gathered, RCA4 was the RCM with the largest number of simulations (corresponding to different driving GCMs) with a sub-daily time frequency for quite a large number of variables, required for driving the ROMS model. As outlined in the methodology section, this approach is appealing within the research framework, as it aims to limit uncertainty sources by focusing on a single RCM setup rather than exploring multiple RCMs driven by different GCMs.

-230. "This northbound improvement of the model skills suggests that internal dynamics partially compensate for the missing variability component in the boundary conditions.'' ; "...the fairly good performance on the Northern Adriatic coast permits a more straightforward use in this region, also in terms of boundary conditions for local applications".

-What is the sampling interval used in Fig.5?

-Are tidal oscillations included in Fig .5?

-The tidal amplitude is known high in the northern Adriatic; could authors discuss the impact of the tidal amplitude of the model performance shown in Fig.5.

-Again authors should further discuss the lateral boundary conditions.

Although tides were included in the EV and EV runs by considering 15 tidal components from the TPXO dataset (Egbert and Erofeeva, 2002), Figure 6 (previously Figure 5) refers to daily-averaged data, and therefore semi-diurnal and diurnal components are lost, and the underestimate in sea surface level variability shown in that Figure does not include tidal variability. In a deeper analysis not included in this manuscript for the sake of synthesis we found that tidal variability resulting from TPXO is generally underestimated. This may contribute to some mismatch in circulation and tracer transport patterns over the short term, but since the result of the validation is considered in aggregated terms, we don't see obvious reasons to expect systematic errors introduced by this factor, whilst it most likely contributes to add some noise around the average skills.*

This is better explained in the revised version, at lines 270-277 in the track change version.

-270. ''... , suggesting that SST does not show any macroscopic sign of a spurious drift related to the model implementation ".

Could authors rephrase this sentence?

Thanks, in this form this sentence aimed at addressing the (undeclared) possibility that a bias in the flux parameterizations at the air-sea interface or numerical issues could, at the multi-decadal time scale, result in an unrealistic temperature drift. In fact, for this sentence to be clear it would call for a better discussion of this possibility, but then it would probably burden the discussion without adding an important contribution (in the end, the drift is not there!). We thus decided to remove this sentence.

-280. '' In turn, while intermediate to high S values are mostly well reproduced, low to mid salinity tends to be overestimated, particularly in the Kvarner Bay and in the Dalmatian Islands.''

Please better discuss and explain the overestimation of the lowest S values.

Thanks, two main aspects can have a role in this result. First, the estimate of submarine freshwater inputs in the karstic northeastern and eastern Adriatic coast is a recognisedly challenging task in the area, and can lead to significant uncertainties. Secondly, the model resolution does not permit a complete description of the complex geomorphology of that coast, and therefore of its small-scale circulation patterns. This is better explained in the revised version, lines 335-341 in the track change document.

-285. '' This suggests that the climate ensemble, whose implementation began before the release of the latest version of MFS (Escudier et al., 2020), should not be considered prone to major elements of obsolescence associated with the use of a previous dataset (Simoncelli et al., 2019)".

Could authors explain how this can be deduced from Fig.10.

Here the idea is that, if the use of the latest version of MFS does not significantly improve the overall skill metrics, it seems reasonable to expect that the use of a previous dataset (the only one available at the time of the ensemble implementation) to compute the climatologies at the boundary should not lead to major shortcomings in the climate runs. We better frame this concept in the revised version (lines 351-354 in the track change document).

-305. '' In the deep Adriatic, an apparent tendency to underestimate average values of θ and S throughout the year is actually the result of some shortcomings in the description of thermohaline properties in the upper layers.''

The sentence needs further explanation of the mentioned shortcomings.

Thanks for this suggestion, in fact the sentence should be adjusted. More precisely, the first part refers to panel c, but we do not have enough elements to actually attribute this result to some processes, or shortcomings taking place in the upper layers. Instead, while there is certainly an overestimate of heating and mixing in the upper layers, the observed underestimate at deeper layers could be inherited from the dataset used for initialization and boundary conditions (the results presented by Pranic et al. 2021, which were based on the same datasets, showed very similar values). We discuss, and hopefully better clarify, this in the revised version at lines 369-384 (track change document), though pointing out that a conclusive interpretation of this mismatch requires a dedicated effort.

-370. Please correct : ''hle200 m''

Thanks, adjusted.

-375. ''Below the upper layer, θ increase varies from + 2.8°C for h=200 m to +1.3°C for h≥800 m, S increase varies from +0.21 to +0.17, and σθ varies between-0.44 and-0.15 kg m−3.''

Authors should discuss the relatively high values of salinity and theta changes in the deep Adriatic (shown in Fig.15c) and present comparison with results from previous work. Also, why two among the vertical profiles of the theta change are truncated at depths less than ~630 m, (Fig. 15c left).

The truncation of the bottom values of the ensemble spread resulted from the graphical setting for the x-axis limits, this has been adjusted in the revised version (Figure 20). The values of the temperature and salinity variations in the Deep Adriatic are consistent with the trends prescribed at the boundary (Figure 2) and a comment along this line has been added in the revised version (track change, lines 478-484).

-385. ''Under this approach, modelled differences between SCE and CTR conditions (expressed as monthly mean cumulative intensity of the events) appear generally minor and in any case only occasionally statistically significant.''

Here also, authors should mention results from previous work, if available.

Thanks, this has been discussed in the light of trends from the recent past in the literature (lines 494-496 of the track change document) and flanked with some further comments on the implications of the choice of the reference thresholds for MHW and CS definition (lines 489-491 in the track change version) and on the effect of changes in the SST statistical distribution (Figure 18, lines 500-502, and 510-512)

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