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

Review of the paper

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. 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 will be clearer in framing these objectives and recalling the need of a specific analysis and discussion of forcings and boundary conditions 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 will present in the "Model Setup" section 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* Figure 1 *and* Figure 2 *in the present document). We will point out that 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), 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 2*) shows a good agreement with measured values, 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 3*) 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 (*Figure 4*) and net surface heat fluxes (*Figure 5*), as well as the differences in the daily climatological CDFs for the same quantities (*Figure 6, Figure 7*) 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 1*, panels c and f), they will be used to draw some considerations on the effect of surface fluxes and boundary conditions in the basin properties, although again recalling that fully disentangling the role of each factor in general is beyond the scope of this work.*

*The discussion will move along these lines, addressing the reviewers questions raised above. In particular, we anticipate here that the values of theta and S changes, consistent with the trends imposed at the boundary and modulated by the internal dynamics throughout the basin, will be discussed also referring to other climate projections available in the literature for this region. Instead, the weak change in MHWs and CSs seems consistent with the limited change in the shape of the statistical distribution of SST around the extremes (*Figure 6*): in particular, considering that here we take as a threshold the 90th and 10th percentiles of each period in which we compute the extremes, we would expect that major differences in MHWs and CSs would be associated with larger differences in extreme values than around those thresholds, which does not seem to be the case. It should also be pointed out (and we will do it in the revised version) that, concerning the atmospheric forcing, there is increasing evidence of a generalised tendency of GCMs and RCMs to underestimate temperature extremes, mostly due to the neglection of aerosol changes (see for instance Schumacher et al., 2024). In any case, we will also comment more extensively on the implications of defining future MHWs and CSs based on the future statistics instead of the present thresholds.*

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.

-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".

-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 reshape 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."

-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)".

-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. The references have been added to the revised version of the manuscript.

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 5 (in the manuscript) refers to daily-averaged data, and therefore semi-diurnal and diurnal components are lost, and the underestimate in sea surface level variability shown in Figure 5 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.

-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.

-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.

-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 will make this clearer in the revised version, 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, we adjust it in the revised version. The values of the temperature and salinity variations in the Deep Adriatic are consistent with the trends prescribed at the boundary (Figure 1 of this document). We will comment along this line (Figure 1 will actually be included in the revised version) also in the light of the available literature.

-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, we will check for latest literature in this direction, and discuss if possible.

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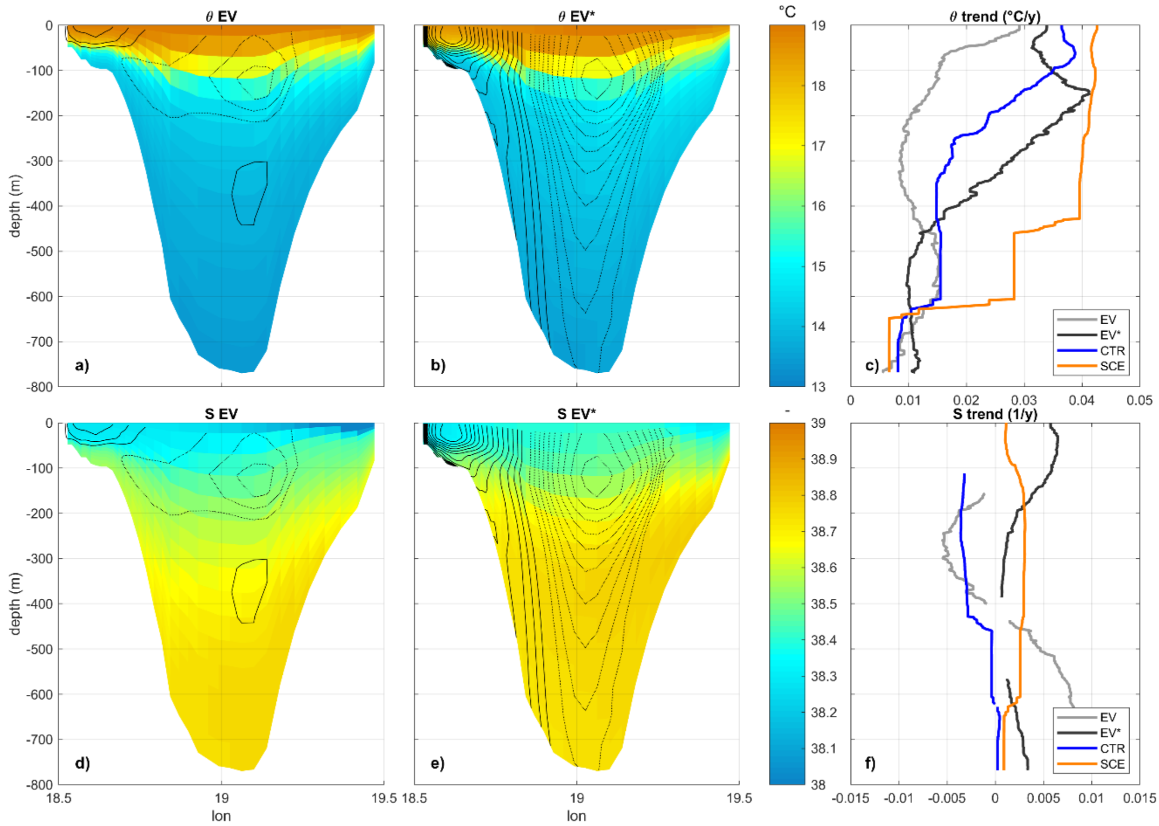

Figure 1: Time-averaged boundary conditions (panels a, b, d, e) and trends (panels c, f). Thick lines and dotted lines represent 0.01 m/s velocity contours in the outflow and inflow direction respectively.

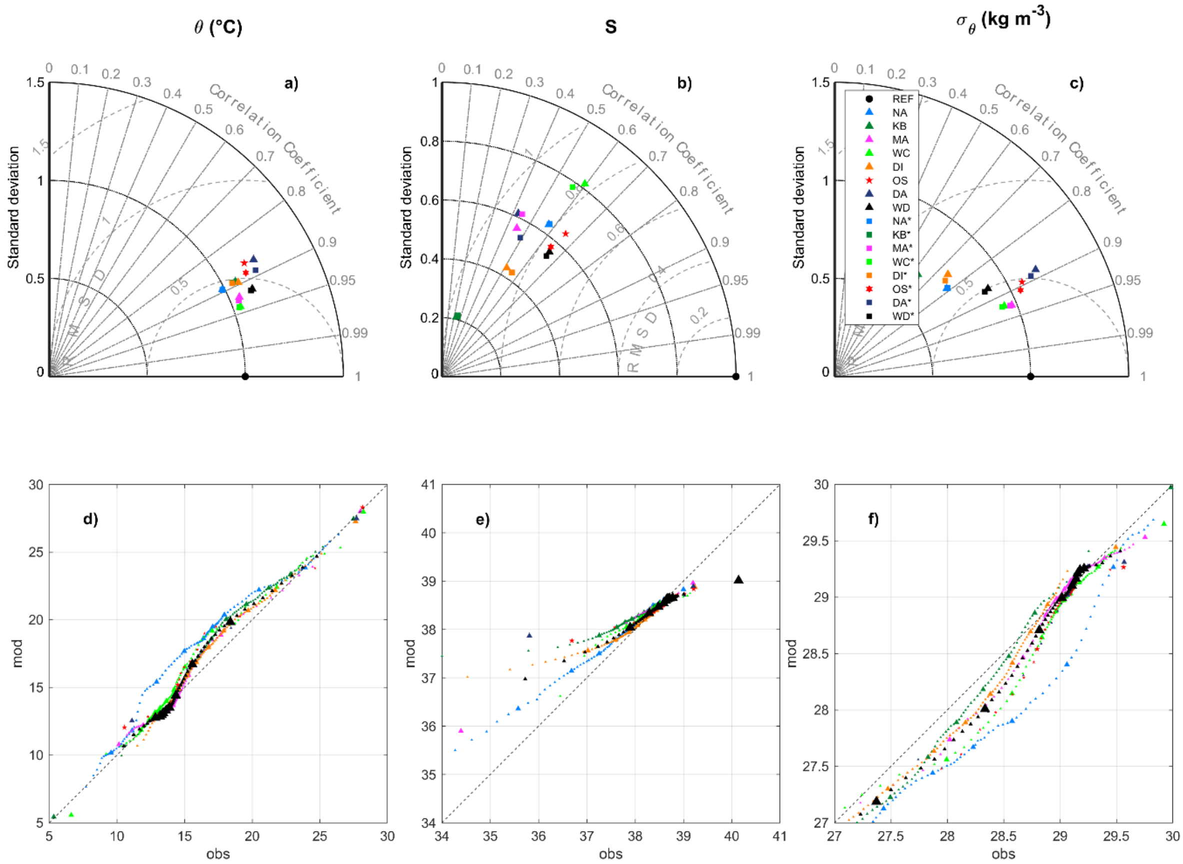

Figure 2: Revised version of Figure 10, including OS (Otranto Strait) as an additional subdomain alongside those defined in Pranić et al. 2021.

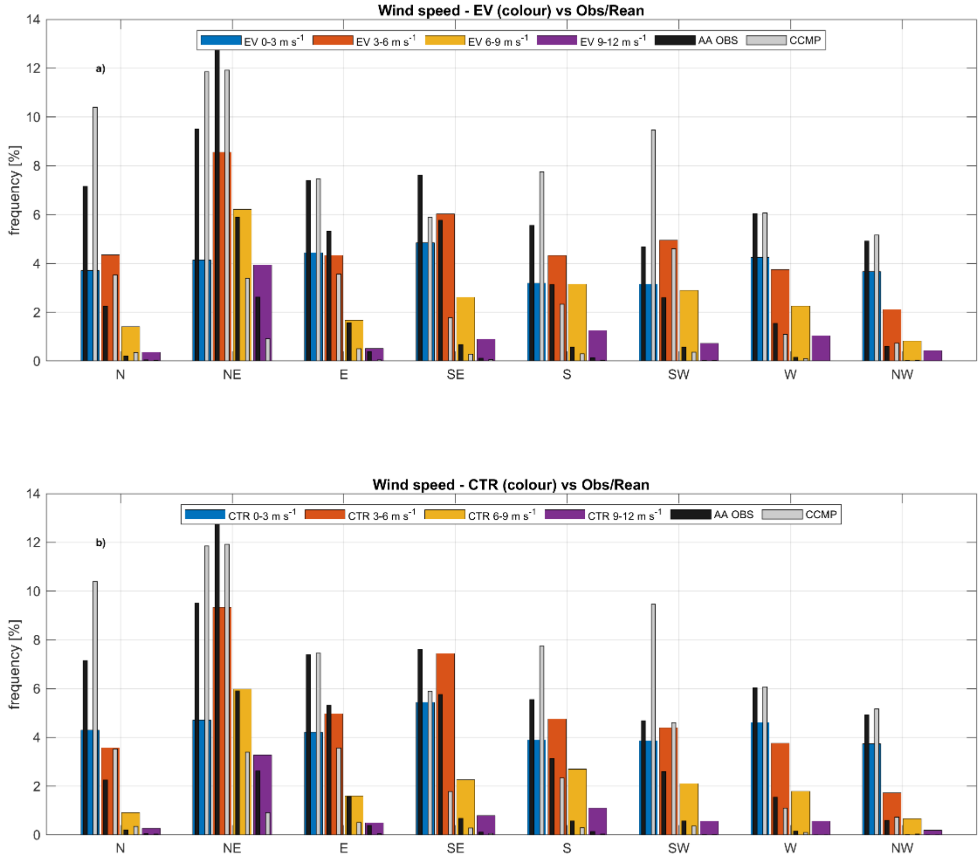

Figure 3: Revised version of Figure 4. Comparison of SMHI-RCA4 fields used: a) in the EV run, and b) in the climate ensemble (coloured bars) against CCMP directional wind statistics and in situ observations at AA.

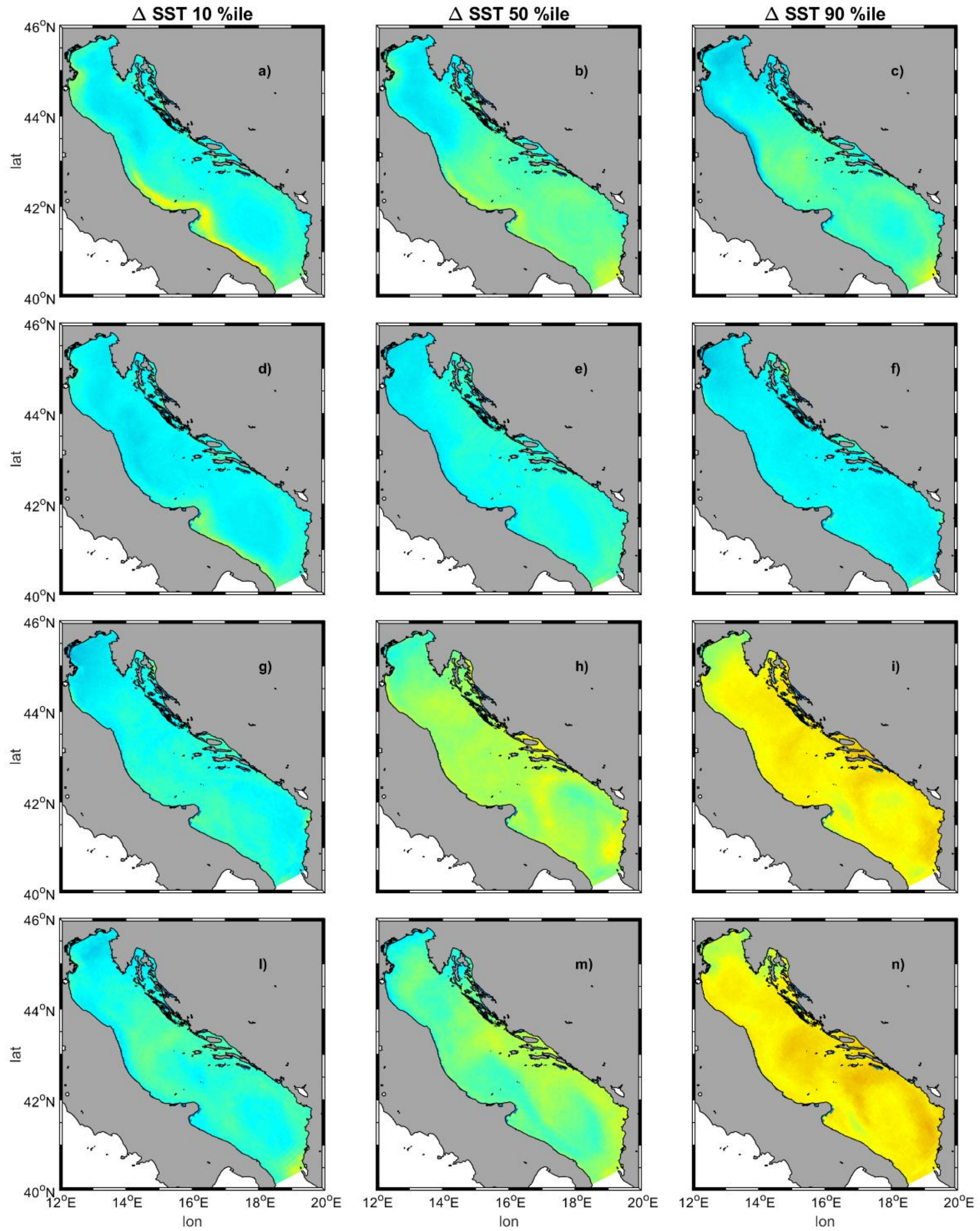

Figure 4: Seasonal variations (SCE-CTR) of the 10, 50, 90 percentile SST

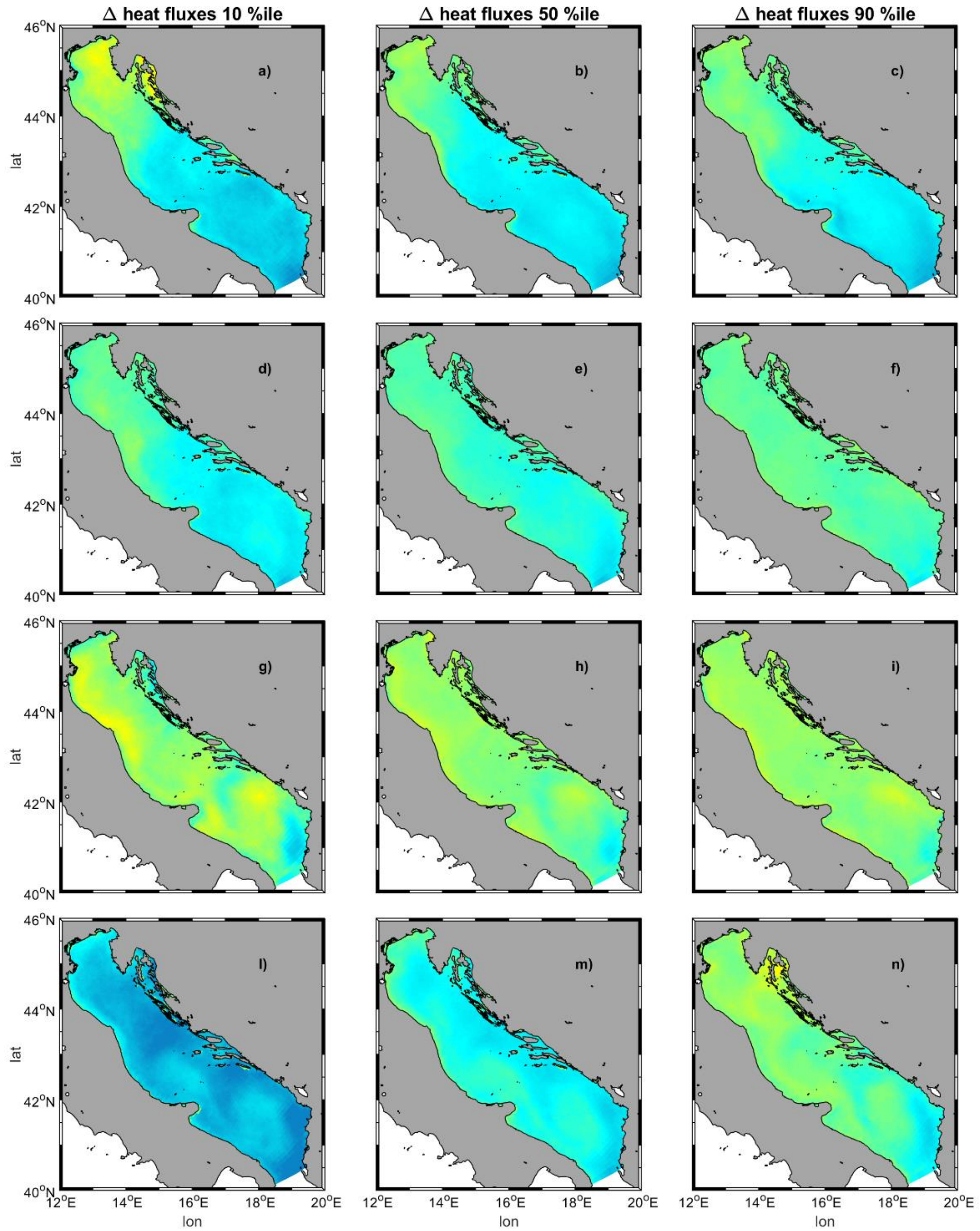

Figure 5: Seasonal variations (SCE-CTR) of the 10, 50, 90 percentile net surface heat fluxes

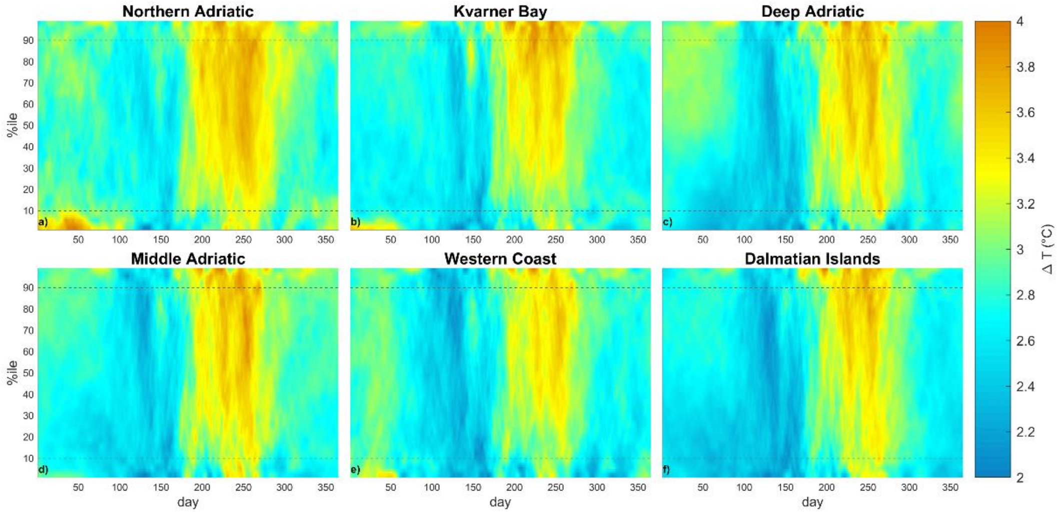

Figure 6: Variations (SCE-CTR) in the daily statistics for SST

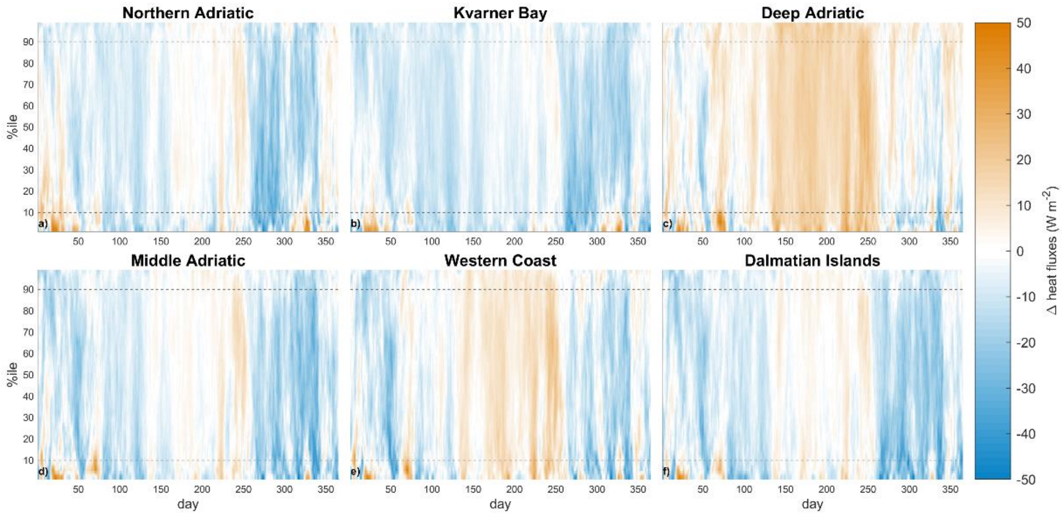

Figure 7: Variations (SCE-CTR) in the daily statistics for SST