

General Comment

The authors have largely revised the manuscript to offer a more concise and straightforward analysis of their dataset. I appreciate the detailed and extensive responses they provided. I believe the removal of PPfod was a good decision, as it improves readability and lightens the manuscript. Overall, I find the revised version much clearer, with a fluid introduction and well-identified objectives. The manuscript is pleasant to read. The detailed description of the PP model in Appendix A is an excellent addition; it is not only useful for this manuscript but will also be valuable for users of the related CMEMS products. The inclusion of PAR is another positive improvement, clarifying the modulation of both Chla and PP. Finally, I liked the addition of limitations and next steps in the conclusion.

One main methodological concern remains. I continue to struggle with the interpretation of how correlated the univariate EOF modes are. To me, mode ordering is not inherently comparable across variables, and Sections 3.2.1 and 3.2.2 appear more as descriptions of statistical diagnostics. From this perspective, I find the addition of the MEOF method adequate, as it allows, more rigorously than intuitively, to highlight the structural coupling between variables. While I do agree with the authors' description and interpretation of the MEOF modes and their coupling, I still fail to see how they support or correlate with the univariate EOFs. Most of my specific comments below support this observation. I think that a manuscript rearrangement, such as inverting the subsection ordering within Section 3 (MEOF before EOF), would be sufficient to highlight the statistical derivation of the basin's modes.

Overall, I do think that the analysis of the modes/regimes provided by the authors (a spring-summer maximum production period and a spring bloom-like phenology) is adequate. I maintain that the analysis would be a valuable addition to this edition of the *OSR* but recommend strengthening the methodological derivation of the main modes of the basin before publication. As such, I think that the manuscript would require minor modifications.

Specific comments

line 150 *However, these areas represent a small fraction of the Mediterranean basin.* Small in surface area yes but are they small in terms of productivity ?

Furthermore, their small surface area might result in them having a smaller weight in the statistical methods (EOF/MEOF). However, in the end, they might have their own modes of variability, decoupled from the entire basin modes. This observation is reinforced by looking at Fig. 3e, where PP is masked in some coastal areas.

The author raises the inclusion of mesoscale structure to contextualize the choice of spatial dimension. I agree that the 4×4 km size is well suited to capture mesoscale structure in the Mediterranean Sea, but I wonder how the one-month period compares to the temporal scale of mesoscale structure (e.g., movement, dissipation, etc.).

line 174: Are PSC considered in the MEOF, or only Chl, SST, MLD, and PAR?

Figures: I found the usage of the same jet palette for all figures confusing, especially in the interpretation of the spatial variability of the modes. I suggest keeping the jet palette for the mean states (Figures 1 and 2) and using a diverging colormap centered on 0 (e.g., red-white at 0 and blue) for the mode variability (Figures 3, 5, 7, and 8).

Figure 3 caption: I feel that a reminder of what positive/neutral/negative variability means would help better correlate the visible spatial patterns with the identified modes. For instance, in Figure 3, it is stated that small variability (~ 0) indicates areas in opposition of phase with the mode and are removed (e.g., white areas). Meanwhile, in Figures 7a–b, the MLD and Chl variability are described as negative, revealing that they are *in anti-phase with the seasonal solar-thermal forcing embedded in the temporal amplitude* (i.e., the 1st MEOF mode).

Section 3.2.1 / 3.2.2 titles : Line 253 / 352 The first mode is presented as the classical annual cycle of the basin, and the second mode as deep water formation and spring bloom variability. I wonder what makes the latter, especially the second modes for Chl and PP, not equally specific as an "annual" or "seasonal" cycle dominant in a specific region (e.g., the Gulf of Lion area). I acknowledge that this mode also presents stronger interannual variability, as well depicted by the authors, particularly for MLD.

Section 3.2.1 / 3.2.2 : Line 253 / 352 *Annual variability (1st mode) and Deep-water formation and spring bloom variability (2nd mode)*. I find the usage of the word "mode" to characterize the two main regimes of the Mediterranean Sea confusing and premature at this stage. These sections appear to be more of a presentation of statistical diagnostics, with the modes (or regimes) of the basin being highlighted later with the MEOF.

From line 256: Based on this observation : *The variability of these areas is low as compared to the rest of the fields and the uncertainty associated with satellite retrievals in shallow waters are known to be substantially larger than open waters (Volpe et al., 2012; Volpe et al., 2019; Brando et al., 2024)*. I would expect all these regions to be masked consistently through the study at least for the related variables.

line 270: *The combination of the spatial pattern and of the temporal evolution points to winter deep-water formation in the Gulf of Lion, whose interannual variability is further isolated in the second EOF mode.*

This comment, which points to the first mode of the MLD, also refers to the 2nd mode of the basin. Is the first mode of MLD also linked to winter deep water formation, or just the second one

line 334: *It is worth mentioning that nearly the total variability of light (98%) and temperature (97%) is linked to the annual cycle while there is still a substantial part of variability to be explained for MLD, CHL and PP.*

I still fail to see why the 2nd modes of PAR and SST are worth considering in the second mode of the basin, as they are statistically weak. For SST, I find it interesting that this mode aligns with Volpe (2012). For PAR (from line 380), the authors suggest a possible physical interpretation related to the modulation of cloud coverage, which I personally find interesting.

However, I think the possibility that this mode is just noise remains high. I also fail to see why this second mode, interpreted as cloud presence, would be related to deep water formation and spring bloom (the 2nd mode of the basin).

line 440: I fail to see how the MEOF 2nd mode captures the interannual modulation of the MLD, which was raised beforehand as the main difference between the MLD 1st and 2nd modes.

technical corrections

Line 8, 86: There is an inconsistency between PSC which refers to PHYtoplankton Size Class and Phytoplankton Size Structure (line 8-38 and 86).

Line 7-8: I would recommend specifying surface phytoplankton biomass and integrated primary production.

Line 25 and 36-37. From the GOOS, phytoplankton biomass and diversity are the *EOV* (or *subvariable of the EOV*) while productivity is a *derived product* and physical drivers are *supporting variables*.

(<https://goosocean.org/what-we-do/framework/essential-ocean-variables/>)

line 87: I suggest mentioning that CHL comes from satellites (e.g. . *CHL fields are derived from a multi-sensor, gap-free Level-4 satellite product specifically developed for the Mediterranean Sea ...*) as it is for SST later on.

line 174: multi variate EOF (MEOF)

Figure A1, A2: the x and y labels might be reversed.