Mediterranean Sea heat uptake variability as a precursor to winter precipitation in the Levant

Cohen et al.

Response to Reviewer 3:

We thank the reviewer for his helpful comments and his insistence on simplifying and improving the presentation of the results. Specifically, we accept the Reviewer's suggestion of change the focus in the manuscript to the EOF analysis and results, instead of the previous focus on the SOM analysis. We therefore moved the EOF-based and results from the Supplementary Materials to the main text, and moved the SOM-based results to the Supplementary Materials instead. This change has no implications for the main results and conclusions, while significantly improving the clarity of the presented results. Furthermore, to address the additional comments raised by the reviewer about the SOM analysis and its parameter selection, we amended the explanation about the SOM analysis and results in the supplementary materials. Specifically, we discuss the limitations of SOM analysis and the fact that it is not designed to maximize the amount of variance explained, and to emphasize that we tried to find the minimal number of patterns beyond which repetition in the patterns' characteristics emerges.

A comment-by-comment (blue) response (black) is provided below:

The authors elaborate on the SOM configuration and EOF analysis. However, this analysis raises several additional issues:

1. The approach of the authors towards the SOM analysis gives the impression that it is being mistreated: SOMs do not have a "leading pattern" and are not constructed to minimize the explained variance, nor do they have temporal "amplitudes". Rather, SOM nodes represent cluster centroids and each sample (in this case, monthly anomaly) is assigned to a single pattern, unlike EOF where each sample is constructed by different amplitudes of the EOF modes. SOM nodes are not directional vectors that explain temporal variations either, unlike EOFs. The authors seem to be referring to node frequency as amplitude, in which case it is unclear over which period this frequency is calculated.

As the Reviewer suggested, we now base our results on the EOF analysis, and use the SOM analysis as support for the robustness of the results. There is therefore no need to consider the emergent SOM patterns as 'leading patterns'. Instead, the emergent three

SOM patterns nearly match the first 3 EOF patterns, indicating that our results are not sensitive to the choice of methodology. We explain that SOM pattern loading can be regarded as analogous to the principal component of the EOF, as evidenced by the similar correlation results for the EOF and SOM patterns.

2. The supplementary material does not explain the choice of 1X3 SOM configuration using a relevant measure: this should include an elbow-method analysis or minimizing the SOM quantification error. If anything, results shown in the supplementary should motivate the authors to enhance the SOM to at least a 2X3 configuration, drastically raising the total explained variance (though this is not a traditional requirement from a SOM analysis) and obtaining refined spatial patterns. E.g., the key area of the Aegean sits in the low-confidence area of node 3 – suggesting that the association between the AQA and the SOM nodes is weaker than it seems from the composite maps. This may improve with a refined SOM application.

Given that the focus has shifted to the three leading EOF patterns, there is no need to justify the choice of 3X1 SOM structure. We nevertheless mention in the Supplementary Materials that pattern redundancy appears for structures larger than 3X1.

3. Maximizing the variance explained by the "leading" SOM pattern counteracts the primary purpose of the SOM – building clusters with minimal internal variability.

We now avoid using this terminology.

4. Topographic errors in 1D and 2D SOMs are incomparable – the topographic error is asking how many of 2nd-winning neurons are not neighbors of the 1st winning neurons. E.g., in the selected 1X3 SOM most neurons are neighbors by construction, and so the TE of this configuration is not comparable to the 2D configurations and does justify the choice. TEs are used to measure the continuity of the SOM space – e.g., are there nonneighboring neurons that are very similar to each other. For selecting SOM size, the quantification error is more relevant, and even that is only borderline comparable between 1D and 2D SOMs, as 1D networks inherently emphasize one dimension of variability – more suitable for analyzing temporal variability of a local timeseries, etc.

Given the focus on EOF patterns, the discussion of topographic error in the SOM patterns is no longer relevant.

5. Moreover, if the conclusion is that EOFs can readily produce similar correlations and seeing that the SOM algorithm is underutilized and treated as an EOF analysis throughout, I recommend switching to the EOF results for clarity. In my view, SOM is meant to enter

where EOF falls short of capturing the dominant patterns of the system, or if higher precision is sought after. However, this SOM configuration essentially converge to the EOF results – rendering its inclusion redundant.

Changing to EOF analysis will highlight the importance of the dynamical pathway described here rather than focusing on the clustering approach – which involve several subjective choices that are not justified well by the authors. I believe that EOF serves a more objective, reproduceable, and physically interpretable approach for the purpose of this MS.

We thank the Reviewer for insisting on this and have accepted the suggestion.

Minor comments:

1. The global maps in figure 1 are redundant for the purpose of this MS. If a case is to be made concerning them, it can surely be made using a single map.

We accept the Reviewer's suggestion. We now present only one global correlation map in Figure 1.

2. There is no justification to display and discuss two SOM networks with highly similar results (e.g., SST and Qf). Choose one, and state that similar results are obtained if the other is used. This is not very surprising seeing as the two fields are highly correlated.

Given that the tendency of SST is related to Q_f, the relation of these two fields, as well as their relation to Levant precipitation, is nuanced. We elect to show both fields because most previous work has focused on SST, whereas our results indicate that Q_f is likely a more relevant field. To address the Reviewer's consent, we have now joined the presentation of the SST and Q_f results into a single figure, which allowed clarifying the text discussing the SST and Q_f patterns in Section 3.1.

3. Most references do not include a doi, making the review process unnecessarily tedious, and are not in line with the WCD format requisites.

DOI added to references.

I recommend accepting the MS once the issues with the SOM analysis are resolved – either changing entirely to EOF framework or enhancing the SOM analysis to justify its use.

As suggested, we now base our analysis on the EOF framework.