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

The researchers investigated the seasonal predictability of winter precipitation in the eastern Mediterranean (EM) with respect to the Mediterranean SST and heat uptake in the preceding summer/autumn. Based on both reanalysis data and observations, the authors usie a SOM algorithm to classify SST and heat uptake fields into three groups. of them two show correlation to the following winter EM precipitation. Spatial patterns of lagged correlations between SST and Q_f and subsequent winter precipitation in the EM are explored and compared between the two relevant groups, detecting peaks in August at the Aegean sea. The authors then define an Aegean sea anomaly index (AQA) which, when taken for August months, can act as a precursor with negative correlations to winter precipitation in the EM. Composites of positive and negative AQA-preceded winters are then further analyzed, thoroughly investigating the impact of the AQA on synoptic systems and on the decomposed hydrological balance.

The researchers conclude by proposing a cross-seasonal link between Aegean SSTs and Q_f in August to winter precipitation in the EM. Through the eastward migration of the subtropical jet following negative AQA anomalies, cyclonic activity is reinforced in the EM, allowing for more persistent precipitation.

The study pursues a relevant objective in a novel approach, using reliable datasets and a range of sound methods. The manuscript is comprehensive and sound, well written and nicely structured, albeit slightly dispersed and over-informative at times.

I recommend accepting the paper after some minor concerns are answered and revisions made.

General comments:

1. There is a general feeling that the MS is constructed step by step based on its own statistical results. I recommend restructuring the MS in light of the results and the decisions taken by them, in a way that wouldn't overwhelm the reader. E.g., if the SOM shows similar results for both SST and Q_f, and seeing as Q_f proves to be a better predictor, why not avoid showing and discussing SST throughout? Clearly a lot of effort was put into this work, but not all must be shown to the reader.
2. The SOM algorithm is underutilized here. The 1X3 network inherently looks for zonal variability only, leading to a rough separation that could have been easily obtained with less complex algorithms such as EOF or k-means, simplifying the interpretation of the results. Also, a significant test / standard deviations for the classified patterns is required to show regions of higher and lower confidence.

3. I think conciseness should be sought after in the revised MS. Some information can be removed from the figures and following discussions, increasing the focus on the main points the authors wish to convey.

Minor comments:

Figure 1: why is it important to show the difference between summer and winter precipitation? This panel isn't addressed in the text and the point of it is unclear. Also, the global maps are hard to see. Is it essential to have three of them? Perhaps one is enough to provide the context of remote teleconnections?

L52: amplification of...

L179 and elsewhere: the authors refer to cluster frequency as "explained variance". I find this terminology more suitable for EOF analysis. For clarity and fluidity, consider using cluster frequency (or a similar term) throughout.

L184: what do you mean by "node"? please clarify

Figure 2: I find the "correlation across SOM parameters" puzzling and unnecessary to propagate the reader through MS. Similarly, showing multiple months with mostly weak correlations that fluctuate from month to month does not support the robustness of the analysis. I suggest keeping the only the leading SOM input - Q_f and only the most successful lead-time (August). This will make the message easier to take in.

L200: This is a methodological leap, especially since it is unclear how robust these spatial patterns are in the Aegean. More statistical testing is required to establish the Aegean Sea as an anchor for the SOM separation. E.G., did the results differ significantly when using the Ionian/Ligurian seas instead?

L207: anti-correlated?

Figure 4: would it be possible to denote the corresponding winter precipitation anomalies?

L275: please state that these conclusions relate to the negative AQA phase (correct?)

L280-283: this sentence is unclear. Please rephrase

L283: what do you mean by "geostrophic enhancement"?

L293: I would consider rewriting this section with emphasis on the importance of this work and less speculations and general ideas that do not relate directly to this research.