Mediterranean Sea heat uptake variability as a precursor to winter precipitation in the Levant
Cohen et al.

Response to Reviewers

We thank the reviewers for their helpful and insightful comments, which improved the analysis and discussion in the manuscript. To address the reviewer's comments, we adapted the main text of the manuscript and added additional sections and figures to the Supplementary Materials (SM) with additional analyses.

Specifically, we added the following findings to the SM:

1. We expanded the SM section S1, elaborating on the SOM analysis and optimization, and added analyses and explanations of the method used to select the final SOM structure. Specifically, we analyzed the amount of variance explained by the different SOM structures, by an individual SOM pattern within each structure, and the Topographic errors that occur for each structure. Using these, we explain our selection of the final SOM structure, aiming to maximize the variance explained by the SOM structure and the leading pattern, and to minimize the amount of topographic errors, for the minimal amount of patterns, consistently yielding 3 very similar leading patterns in both the SOM and EOF analyses .

2. We added a section in the SM (S3) presenting the EOF analysis of ocean heat uptake and SST, and showing their correlations with Levant winter precipitation. The results are similar to those of the SOM analysis, showing that our selected patterns are not sensitive to our choice objective methodology. We decided to focus on the SOM analysis due to its slightly higher correlations (particularly with SST), as a prerequisite for defining AQA, although the EOF analysis can also be used to justify the regions chosen for the definition of AQA.

3. We added a section in the SM (S2) analyzing the relationship between Aegean Sea SST anomalies and Levant winter precipitation, showing that a similar analysis and predictive behavior can be achieved using SST, but that the results tend to be less significant. Given SST's lower predictive skill, we focused on ocean heat uptake in our analysis.

4. We added spatial maps showing the significance of the SOM patterns (added to SM section S1, Figure S4). We analyzed the statistical significance of the correlations between AQA and Levant winter precipitation (SM section S5, Figure S17).

5. To assess the robustness of the Aegean Sea's spatial definition in the calculation of AQA, we changed the area used to define the Aegean Sea by shifting it by ±0.25 degrees north and east, finding that the area defined as the Aegean Sea does not significantly affect the correlations to Levant winter precipitation, which is the main objective of our analysis (added to SM section S5).

Additionally, following the Reviewers' comments, we tried to provide more comprehensive and clearer descriptions of our results and the analyses we performed. Specifically:

1. We elaborated on the optimization process used for the SOM analysis in the methods section, explaining the SOM method (Section 2.3), incorporating the selection method we used for the final SOM structure - minimizing the overall number of patterns, while maximizing the amount of variance explained by the SOM patterns (individually and in total) and minimizing the topographic error.
2. We added additional details on the calculation of correlations, both across SOM patterns and between SOM patterns and Levant land winter precipitation, to the captions of Figures 2 and 3, to make the analysis more straightforward.
3. We revised the manuscript to explicitly emphasize that additional analysis is needed to fully explain the physical mechanism underlying the lagged atmospheric response in the eastern Mediterranean. We addressed this question by analyzing atmospheric and surface data, but were unable to identify a relevant process at this time. We therefore hypothesize that the underlying processes driving the lag are oceanic in nature. Further analysis of the lagged response, therefore, is beyond the scope of the present analysis, and we hope to address it in future work.

Specific responses (black) to the Reviewers' comments (blue) are provided below.

Reviewer 1
The manuscript concludes that the Mediterranean heat uptake, especially in the Aegean basin, can be used as a precursor to predict winter land precipitation in the Levant (eastern Mediterranean). The topic is interesting and the manuscript has the potential to be a useful contribution. But I think a few points need to be addressed before its publication.

We thank the reviewer for the insightful comments.

Firstly, many technical details are missing, which is harmful for a good understanding of the manuscript. For example, it is really difficult to figure out what presented in Figures 2b, 2c, 3b, and 3c (tables showing correlation coefficients).

A more comprehensive description of the selection process for the SOM final structure has been added to the SM, and we also expanded our explanation about the selection process in the main text. Additionally, the captions of Figures 2 and 3 now include a more detailed description of the correlation calculations performed across and between the SOM patterns, as well as between the SOM patterns and Levant winter precipitation.

Secondly, the use of SOM is unusual and not justified in this work. Normally SOM is used with a 2-D grid of different nodes, but there are only three patterns in the manuscript.

We agree that there's some redundancy in using only three SOM patterns. This, however, is an emergent result of the selection process we used, which yielded three patterns. The selection process and rationale for using SOM patterns are now clarified in the SM and main text. Specifically, we examined both EOF and SOM patterns, aiming to maximize the variance explained by the SOM/EOF patterns with the fewest possible patterns. We found three key modes of variability in the Mediterranean, with the SOM patterns showing somewhat higher correlations with winter Levant precipitation, potentially because they are not constrained to be orthogonal to each other (as opposed to EOFs), allowing them to capture more of the variability. Since our goal was to find the modes with the highest predictive power for Levant precipitation, we base our results on the SOM patterns. However, the results are essentially not sensitive to the use of either SOM or EOF patterns.

Finally, the manuscript concludes that AQA (Aegean Sea heat uptake anomaly) is a good indicator for precipitation anomalies in the Levant. But, compared to SST which is a state variable, the heat flux is much more difficult to measure or to be deduced from observation. Its usefulness might be quite limited.

To address our unconventional decision to focus on ocean heat uptake for the definition of AQA, instead of SST (which is a state variable and much easier to measure), we added a section in the supplementary materials analyzing the correlations of monthly Aegean Sea SST anomalies, instead of ocean heat uptake, with Levant winter precipitation. The results are similar to those obtained with ocean heat uptake, but are less statistically significant. We therefore chose to focus on Aegean Sea ocean heat uptake (AQA) for the moisture balance and synoptic analysis, instead of SST. In addition, as we show in the text, ocean heat uptake is proportional to SST tendency, which explains the extended lag of about a month when considering ocean heat uptake relative to SST. This again points to ocean heat uptake as a more potent predictor. To address this point in the manuscript, we added an explanation in the results (Section 3.2) to clarify our decision to focus on ocean heat uptake rather than SST.

Furthermore, it is also a little disappointing that a clear physical mechanism is missing to link the AQA to the precipitation in the target area.

We agree that a clear mechanism accounting for the lagged response is sorely missing. We now acknowledge in the text that a complete physical mechanism is not presented, and that we still lack the explanation for the lagged response between the sea surface pattern and the synoptic conditions.
Having said that, given the seasonal timescale of the regional lagged response, we hypothesize that oceanic processes likely play a leading role, which we hope to explore further in future analysis using ocean reanalysis data and Lagrangian trajectory analysis. Initial promising results, presented in Figure 6, show that winters following negative AQA August anomalies tend to be warm in the north-eastern Mediterranean, possibly contributing to the more unstable

conditions we observe in the synoptic analysis. Nevertheless, additional analysis is needed to reach a definitive conclusion.

We use Figure 1 primarily to provide context for our review of existing work relating Levant precipitation to other regions (e.g., the Indian Ocean) and to show that there is considerable variation in the statistical relations observed in the leading months (Sep–Oct), suggesting nuance in the lagged relations. Indeed, somewhat surprisingly, SST correlations in the Mediterranean Sea are significant but lower than in more remote regions. But as we later show, these become significantly stronger when ocean heat uptake is considered.

Yes, we perform the SOM analysis on anomalies from the seasonal cycle for the entire time series, which is also detrended over the 45 years.

To address the concern of autocorrelations in the sea surface conditions, we calculated the correlation of the detrended anomaly from the seasonal climatology of SST in July and November, and the correlations between them across the Mediterraenan:

correlation between SST detrended anomalies in Jul and Nov
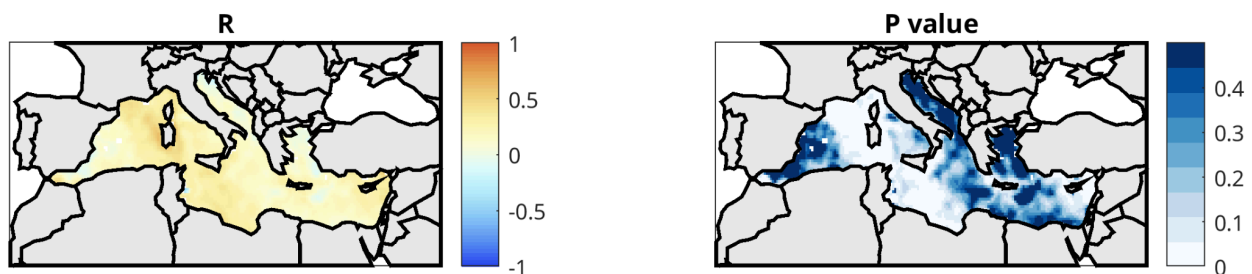
The correlations indicate that in the Aegean Sea, where we focus our analysis, they are relatively weak (below R=0.25). Other regions in the Mediterranean exhibit stronger

autocorrelation between summer and fall SST. Specifically, the western Mediterranean, and the area around Sardinia in particular, shows stronger correlations between summer and fall SST conditions.

3. Figs 2 and 3, Figure Caption, "SST monthly time series". There is confusion for the term "time series". More precisions are needed.

To address this, we changed the phrasing in the captions of Figures 2 and 3, so that the data we used for the SOM analysis will be more clearly stated, eliminating the phrase time-series.

4. Panels 2b, 2c, 3b, 3c. How is calculated the temporal correlation? between what and what?

We now specify in the text that the temporal correlations are performed between the time-series of the amplitude of the SOM patterns through time, and the spatial correlations are calculated across the SOM patterns themselves.

5. Line 207, "AQA is strongly correlated with Qf Pattern 2". AQA is a time series, but the Qf SOM pattern is a geographic structure. How can they be correlated?

To address the reviewer's concern, we explicitly stated that the correlation is between AQA and the temporal amplitude of the second Qf SOM pattern.

Reviewer 2

This paper focuses on an important challenge of improving seasonal prediction of winter precipitation in the Levant region by connecting it with Mediterranean Sea heat uptake patterns. The authors use Self-Organizing Maps (SOM) to identify three main spatiotemporal patterns and develop the Aegean Qf Anomaly (AQA) index representing SOM 2, which correlates strongly with Levant winter precipitation. The research provides valuable insights for seasonal prediction. I also liked that they backed their research with physical interpretation through hydrological decomposition and synoptic analysis.

We thank the reviewer for the productive and insightful comments.

While the overall approach is solid, I have several concerns about methodological choices and explanations that need addressing. Here are my main points:

Major comments

1- SOM vs. EOF choice:

To address the reviewers' comments on our decision to focus on SOM analysis rather than EOF, we added a section in the SM comparing the EOF analysis of Mediterranean SST and ocean heat uptake, which shows that the EOF-based results are very similar, albeit slightly less robust. Our conclusion that only three key modes dominate Mediterranean Sea variability, therefore, is independent of our choice between SOM and EOF analysis. We present the SOM patterns because they present somewhat higher correlations with our target region (the Levant). We hypothesize that the stronger correlations among SOM patterns are due to their not being constrained to be orthogonal (as in EOF), allowing the leading SOM patterns to capture more of the Mediterranean variability.

Note that the optimization was not particularly sensitive to the SOM parameters (shown in Figure S1), so that the number of patterns is essentially the key optimization parameter (as also indicated by the similar emergent patterns in the EOF analysis). In that regard, it makes sense to aim for patterns that capture Levant winter precipitation variability with the fewest degrees of freedom. We now explain this rationale in the revised text and also added more details about the SOM structure selection approach to the SM.

To address the reviewer's comment, we added a note on the sensitivity of the AQA index definition in Section S5 of the SM. Specifically, we adjusted the definition of AQA by adding and subtracting 0.25 degrees north-south and east-west and calculating the correlations of the different regions with Levant winter precipitation. We find that a difference of ±0.25 degrees to the north or to the east in the definition of AQA has no significant effect on its correlations with Levant winter precipitation.

We added a figure to the SM showing the correlation statistics between AQA and Levant winter precipitation. It shows that the correlations in August AQA values are statistically significant at the 95% level.

The Levant land winter precipitation we analyze is calculated as the monthly weighted mean precipitation anomaly from climatology over the Levant domain identified in Figure 1. To ensure the reliability of the precipitation data, we also use in situ observations from meteorological stations around Israel. For the rain gauge data, we calculate the mean of all the stations within the domain.

We agree with this critical point and now seek to clarify it in the Discussion. Specifically, while we show in Figure S15 that NAO and ENSO are not statistically related to AQA, we cannot exclude their potential influence in a more complex way.

To address the reviewer's comment, we adapted the abstract to explicitly state that the correlation is between August AQA and DJF precipitation.

We thank the reviewer for pointing out this issue. To make the manuscript clearer we explicitly stated in the text that positive heat uptake values indicate heat transfer from the atmosphere to the ocean.

To address this comment, we added the variance explained by the 3 SOM patterns to the captions of Figures 2 and 3. Additionally, we added a figure in the SM (Figure S1) showing the

variance explained by the different SOM structures of Qf and explaining its role in selecting the final SOM structure.

- Figure 5: Add statistical significance indicators or confidence bounds to the correlation values.

To avoid saturating the main-text Figure with detail, we added to the caption of Figure 5 a statement pointing to SM Figure S17 that shows the significance parameters (P-values and 95% upper and lower bounds) for the correlation coefficients between AQA and Levant mean anomaly land precipitation.

- Vague phrases like "optimal results" should be clarified - do you mean statistical robustness, strongest correlation, or something else?

In the revised text, we tried to avoid such vague terms. For example, we now describe the selection process for the number of SOM patterns, previously referred to as "optimization," in more detail.

Reviewer 3
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 use 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 Qf 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 Qf 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.

We thank the Reviewer for the useful comments and suggestions, which have been implemented in the revised text and Supplementary Materials (SM), as detailed below.

Thanks for pointing this issue out. As mentioned by Reviewer 1, SST is a state variable that has been used in previous works and is a more intuitive choice as a potential predictor for most readers. This, in part, is the rationale for showing the relation to SST variations in Figure 1. Moreover, ocean heat uptake (Qf) and SST are related such that for a fixed mixed layer depth, the tendency of SST is proportional to Qf. The fact that the lagged correlations for Qf are lagged by an additional month compared to SST (as seen in the comparison between Figures 2 and 3) therefore shows physical consistency which bolsters our results. We estimate that providing results for both SST and Qf variations would facilitate a more intuitive interpretation of our results and make them more comparable to previous work. Thus, while we acknowledge that presenting both SST and Qf results may encumber the presentation, we tried to provide an overall balanced presentation that focuses on Qf while providing relevant context for SST where needed. In the revised text, we try to address the reviewer's concern, and provide more context for the presentation of both SST and Qf.

For the analysis shown in this manuscript, we used both the SOM and EOF methods, focusing on the SOM analysis for the final version and publication, because it yields higher correlations between the emergent patterns and Levant winter precipitation. To ensure the robustness of the results, we added to the EOF analysis of both SST and ocean heat uptake to the supplementary materials, showing that similar results can be achieved using EOF (incorporating more patterns and slightly weaker correlations). Additionally, to better explain the optimization method for the SOM analysis, we elaborate in the supplementary materials section on the SOM analysis and optimization, including a significance test for the SOM patterns and additional details on the optimization process. The optimization process for the final SOM structure consisted of running the SOM analysis algorithm for different SOM configurations (changing the number of columns and rows) and calculating the amount of variance explained by all the patterns, as well as the amount of variance explained by the leading SOM pattern. Additionally, we also calculated the topographical error of the SOM structure. The final optimization method used in our analysis was maximizing the amount of variance explained by the SOM structure and the leading SOM pattern, as well as minimizing topographic error and the overall number of patterns and

repetitions within patterns. This optimization analysis yields the structure of 3 SOM patterns that we use in our analysis.

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.

This comment resonates with comment 1, and we agree with the reviewer that we should aim for a more focused and concise presentation. We tried to implement this guideline throughout the revised text.

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.

Since Eastern Mediterranean precipitation occurs primarily in winter, winter minus summer highlights the Mediterranean regions, particularly the Levant. This is analogous to analyses of monsoonal regions, where summer minus winter precipitation is used to highlight monsoonal precipitation better. We clarify this in the revised text.

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?

Figure 1 is used to facilitate discussion of previous similar works, examining the relationship between Levant winter precipitation and various climate indices worldwide. But the analysis of the specific influence of the Mediterranean remained lacking. We show the lagged responses to September, October, and November to demonstrate that, regardless of the specific details of the correlation for each month, the lagged relations vary significantly across months, pointing to complex dynamic influences (e.g., the tropical Atlantic and the Indian Ocean regions). In the revised text we try to better explain the motivation for Figure 1, while keeping the text as concise as possible.

L52: amplification of…

Corrected.

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.

We calculated the amount of variance explained by incorporating both the pattern frequency, and the amount of variance explained by each pattern. The resulting variance explained is thus

impacted by the pattern frequency, but is not identical to it. We now try to clarify this in the text and figure captions.

To address the comment, we removed the word "node" from the text and explained our meaning of the SOM structure and method in a more straightforward way.

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 - Qf and only the most successful lead-time (August). This will make the message easier to take in.

We acknowledge that this may burden the reader and even obfuscate the results. We nevertheless elect to show the full set of results for completeness and transparency and try to do a better job of pointing the reader to the critical information.

The Aegean Sea was selected because, consistent with the SOM patterns, it proved to be the strongest predictor of Levant winter precipitation. Thus, it was not selected based on SOM separation, but rather on its relation to Levant precipitation. We have extensively examined different regions that indicated potential predictive power (the vicinity of the Gulf of Lion in particular), but found the Aegean Sea to stand out in its statistical relation to Levant winter precipitation. We now state this in the text.

To clarify that the correlation values are negative but significant, we changed the text to state that they are anti-correlated, as suggested by the comment.

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

We have examined this. However, given that the winter precipitation anomalies consist of three-month averages and are related to AQA in a lagged manner, adding these on top of the AQAvariations is not visually particularly revealing. We therefore elect not to display these in this figure.

Yes, the conditions described are relevant for winters preceded by negative AQA values in August. We changed the text accordingly to clarify our meaning.

L280-283: this sentence is unclear. Please rephrase
To better clarify our meaning, we rephrased the sentence, expressing that the moisture balance analysis's results showing the dynamical mean component as important align with the intensified jet we observe.

L283: what do you mean by "geostrophic enhancement"?
We now explain that this refers to increasing horizontal pressure gradients with height, caused by the thermal low.

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.

We tried to make the Discussion less speculative and more focused on the results.