Review of

Revisiting the Moisture Budget of the Mediterranean Region in the ERA5 Reanalysis

Tootoonchi et al.

General

The authors analyze the moisture budget of the Mediterranean region using the ERA5 reanalysis. Their key findings are that moisture budget is not sufficiently closed to allow detection of trends, and the critical contributions of zonally anomalous terms. The writing is generally clear and the analysis sufficiently technically proficient. However, I find the work lacking in several regards. Primarily, the analysis for the most part does do much in terms of relating the various terms (e.g., stationary and transient eddies) to relevant physical processes (e.g., relating stationary eddies to rainy storms associated with the subtropical jet during winter). The authors also do not do a good job of delineating their findings from previous work. Nevertheless, the analysis can potentially help shed light on key processes in the Mediterranean hydrological cycle. I therefore recommend accepting the paper after addressing my major comments, provided below.

Major comments

1. The stated objective of this analysis, to better understand the contributions of time mean and stationary eddies (lines 58–59), is rather incremental. On top of that, the authors do little to delineate their results from previous work. It is therefore not clear what are actual novel findings of the analysis. Further, in several places, it is stated that the results are consistent with previous works, without citing those works. Aside for being bad practice, this further obfuscates the potential novel contributions of the present analysis. In summary, the authors should do a much better job of referencing previous work and clearly stating the novel findings of the present analysis.
2. It is interesting to note that the periods during which global mean P does not equal E go along with rapid global warming (Figure 1). P does not equal E during the rapid global warming from 1979 to the end of the previous century, ending in the 97/8 Niño event. This is followed by a period of weak temperature increase (the so-called 'global warming hiatus') when the $P-E$ residual is small. Then, P deviates from E again as the rate of global warming increases again after 2012. Can you convince the reader that the global residual of $P-E$ is not due to the moisture storage term in the moisture equation $\langle \dot{q} \rangle$ or due to inaccuracies in your methodology? Specifically, one

can use Clausius Clapeyron (CC) to demonstrate the former. Under constant relative humidity H, we would get $\langle \dot{q} \rangle \approx H \langle q \rangle \alpha \dot{T}$ where α is the CC parameter (~7%) and $\dot{T}$ is the rate of global warming. Given that the global mean of precipitable water is about 20mm, this yields for a rate of global warming between 1979—2000 of 0.03K/year $\langle \dot{q} \rangle \approx 10^{-4} mm/day$, justifying the assumption of steady state. It therefore remains to make the case that the residuals are not due to your methodology. For example, due to the use of fewer than available vertical levels, or omitting from the integral near-surface values in regions where surface pressure exceeds 1000hPa. (One way of estimating the integration error may be to compare precipitable water values provided in ERA5 with those derived by integration.) Another potential source of the residual is changes in ice volume. In summary, the authors should convince the reader that the residual of $P - E$ is indeed a feature of the ERA5 reanalysis and not due to their methodology.

3. Given that one potential novelty of this work is to demonstrate the contribution of zonal anomalies in either the humidity or the winds, showing only the immediate Mediterranean region make it difficult to see whether zonally asymmetric terms are related to zonal overturning circulation with links to either the Atlantic or to Asia and Arabia. For example, what is the driver of the negative contribution in the eastern Mediterranean by the dynamic term shown in Fig. 8g? Could this be related to the descending branch of the Indian monsoon (i.e., the so-called Monsoon-Desert mechanism by Rodwell and Hoskins)? Expounding on such processes in the analysis and zonally extending the analysis region may shed light on such potential drivers of the various patterns (e.g., the Indian monsoon, the Persian trough, ventilation of land areas, etc.), which are hardly discussed in the text.

Comments by line number

10—13 This sentence is cryptic

48 I would also add Elbaum et al. (2022, "Uncertainty in projected changes in precipitation minus evaporation: Dominant role of dynamic circulation changes and weak role for thermodynamic changes.")

122 Why is only a subset of the vertical levels used for the vertical integration? Wouldn't this reduce the accuracy of the vertical integration?

185—186 This gives the impression that, as in the global mean, there is some constraint under which we would expect $P - E$ to vanish when averaged regionally, which is not the case.

213 and elsewhere What previous work? Please specifically cite relevant works.

245—247 Not sure I agree with this statement. The terms $\nabla \cdot \langle \bar{u}^* \bar{q}^* \rangle$ and $\nabla \cdot \langle \bar{u}^* [\bar{q}] \rangle$ generally balance out, and so the term $\nabla \cdot \langle [\bar{u}] \bar{q}^* \rangle$ would seem to be of the same order as their residual. More generally, the term $\nabla \cdot \langle \bar{u}^* [\bar{q}] \rangle$ would be related to the zonally asymmetric circulation, whereas the $\nabla \cdot \langle [\bar{u}] \bar{q}^* \rangle$ term would be related to zonally asymmetric temperature variation. If indeed the latter term is not significant, how does this sit with the alleged immense importance of land-ocean contrasts?

255 Why do the transient eddies dominate the sector mean? Please provide an explanation backed by the relevant references. Jet?

Figure 4: the mean and stationary terms in Fig. 4e and 4h are nearly identical. Can you explain why? Please comment on this.

317—321 Note that recently, Adam et al. (2023, "Reduced Tropical Climate Land Area Under Global Warming.") showed that over land areas the subtropics are expanding on both their poleward and equatorward edges, and that this expansion is likely driven by thermodynamic drying (reduced evaporative cooling), rather than a dynamic expansion of the tropical overturning circulation.