

Dear editor,

We have addressed your suggestions, particularly focusing on the uncertainties between jet intensity and COLs (number and intensity), recognizing the limitations in these associations and methods employed, which still stand as hypotheses. We have emphasized the need for further investigation to clarify these relationships throughout the manuscript. The response to each of your comments are outlined below.

My main concern is that the relationship shown in Fig.4 of the manuscript might result from the seasonal cycle in the strength of the subtropical jet and the number of shallow COLs (Fig2a,d) that will lead to a spurious correlation between them. Please note that I am not saying that there is no relationship, but such a statement should be better supported. This is important to warrant the high quality of manuscripts published in WCD.

Therefore, I request the following changes to the current version of the manuscript:

(1) replace absolute values in Fig 4 with anomalies;

R. We have addressed your suggestion by replacing Figure 4 with a four-panel figure that includes both raw and anomaly values for easier comparison. Additionally, we have kept the relationship between COL intensity and jet intensity to a supplementary Fig. S3.

(2) Modify the text, especially around lines 255-260, to avoid statements that result from what is likely a spurious relationship, e.g. "a significant negative correlation ... suggests a clear relationship between the intensified subtropical jet and the reduction in shallow COLs". The relationship between the intensity of the STJ and the frequency and/or intensity of COLs should remain a hypothesis throughout the text unless solid proof is found.

R. The text has been revised to acknowledge that the initial correlation between jet intensity and shallow COLs observed in raw counts weakens considerably when the seasonal cycle is removed. We have rephrased the discussion to emphasize that this suggests a potential link, but further investigation is needed to confirm a causal relationship. This emphasis has been carried through to the conclusions.

Furthermore, the statement that 'weaker subtropical jets correspond to weaker eddy-mean flow interactions, thereby facilitating the development of COLs ' seems in contrast with the statement by Nie et al. who said that 'underestimated simulated strength of both the Eurasian midlatitude and East Asian subtropical jets may lead to the weaker local eddy-mean flow interaction responsible for the cut-off low evolution.' I understand that weaker jets lead to weaker COLs and vice versa. This agrees with Fig.S3 but not with Fig. 4b, as the number of shallow COLs drops dramatically with stronger STJ but deep COLs do not increase in frequency. So, even if a weaker jet facilitated the development of COLs, then why did not the number of deep COLs rise with the increase of the STJ wind speed? Please edit this sentence to bring fig 4 and S3 in line with Nie et al.

R. To prevent confusion, we have decided to remove the sentence referencing Nie et al.'s statement, as it appears there might be a misunderstanding in how we are interpreting these relationships.

Finally, on the point made in your last response, extreme wet events have been associated with COLs in many studies (e.g., Risbey et al. 2009, Barnes et al.2023). However, the statement by Rudeva et al. reads: 'the extratropics during 'wet' years are characterised by stronger polar jet and amplified zonal-wave 3'; this does not support the idea that the STJ intensity is related to the COLs intensity (please also note that Rudeva et al is not a peer-reviewed publication).

We have focused on the existing literature in the revised version.