*Co-editor decision: Publish subject to minor revisions (review by editor)*

*I thank the authors for doing extra analysis on the relationship between the subtropical jet and cutoff low intensities. Based on the new plot (Fig.S3), I'd argue that STJ intensity is unlikely to play a role in the intensity of COLs. Therefore, I recommend replacing Fig4d with Fig3S and modifying the text around l.260 ( "the subtropical jet likely plays a role in influencing the system intensity"). The relationship between the anomalies is even smaller than the relationship with the polar jet intensity for which it is said that 'no significant relationship' is found.*

*Additionally, I am wondering why the intensity of the polar jet was taken as the average wind speed between 50 and 65 deg S. Given that the average position of the polar jet is ~ 52 deg S (see, e.g., Simpson et al 2020 https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020JD032835). Therefore, the poleward intensity of the jet is prioritised by that index. How sensitive is the jet intensity and relationship with COL's intensity to the choice of latitudes?*

*As the title of the paper highlights the role of the jets in COL's development, I believe that these are important questions to address.*

*Kind regards,*

*Irina*

Dear Dr. Irina Rudeva

Thank you again for your feedback.

There seems to be an issue regarding how we are interpreting the data concerning the relationship between the subtropical jet and COL intensity. While we acknowledge a connection, removing the seasonal cycle from the data seems to weaken this relationship. This is likely because removing the seasonal cycle reduces the magnitude of subtropical jet anomalies compared to the absolute values.

Figure S4 suggests that strong or weak jet values do not directly correspond to strong or weak anomalies. This is because the seasonal cycle subtraction depends on the month. For example, 1998 appears to have the strongest anomaly, but not the strongest actual jet value. Similarly, the period between 1988 and 1989 shows a large actual jet value without the largest anomaly value. This observation could mask the relationship which is evident when examining the actual jet strengths, highlighting the limitations of treating the annual cycle as a fixed and unchanging factor.

This paper (https://doi.org/10.1175/JCLI-3256.1) makes a discussion on this issue, emphasizing that the annual cycle itself can exhibit variations from year to year. Consequently, the variability of the seasonal cycle suggests that simply removing it through averaging might not fully capture the dynamic relationship between subtropical jet and COLs.

Although we think this relationship is clear, we have attempted to strengthen the discussion on this point in the manuscript.

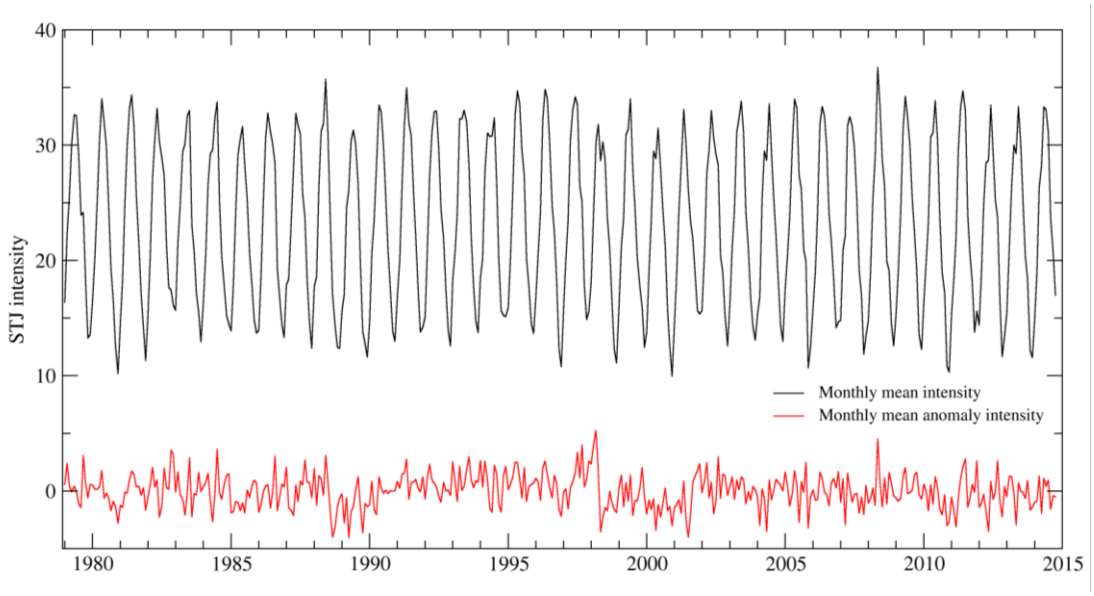


**Figure S4:** Monthly variations of mean intensity (black lines) and mean anomaly intensity (red line) of subtropical jet for the period from 1979 to 2014. Unit is $m.s^{-1}$.

Regarding the second comment, we acknowledge that the criterion used for detecting the polar front jet is somewhat simplified, given the temporal and spatial variability in its positioning. However, we intentionally used a wide latitudinal range to encompass this variability, following previous studies (e.g. Bals-Elsholz et al., 2001), as outlined in the methodology of our manuscript.

The challenge in identifying jet streams and their characteristics is partly due to their diverse structure often characterized by fragmentation and meandering which varies over time. Obviously the criterion used here may not capture the finer spatial and temporal scale variations but should be adequate to characterize the large scale circulation patterns expected to affect COL development.

Thank you once again for your valuable input.

Sincerely

Authors