

Public justification (visible to the public if the article is accepted and published):

I thank the authors for their revision of the manuscript.

I think the relationship in the remaining Fig.4 between the intensity of the jet and the number of COLs comes primarily from the location of the jets as well as identification methods for COLs and jets rather than the jets' intensity. In particular, the 'intensity' of the STJ describes its presence or absence in the first place: a 'weak' STJ is found in summer when there is no defined jet between 25-35degS; whereas a 'strong' STJ stands for winter. Again, should the jet's intensity be replaced with anomalies in Fig.4, I reckon the relationship would be lost. In summer, there are many more shallow COLs on the equatorward side of the polar front jet than in winter, when the presence of the STJ reduces the number of shallow COLs in some parts of the southern hemisphere (e.g., there will be fewer persistent vortices north of the 40S due the presence of the STJ). On the other hand, deeper COLs in winter may be explained by stronger du/dy on the poleward side of STJ contributing to stronger (more cyclonic) relative vorticity, i.e., stronger COL's intensity.

I welcome authors to comment on this relationship. I notice that the number of COLs, especially shallow COLs, also has a strong seasonal cycle, therefore, it needs to be removed similar to the season cyclone in COL's intensity.

Authors' response:

Dear Editor

While it is true that the relationship between the COLs and jet intensity in Fig.4 may be influenced by factors such as the location of the jets and the methods used to identify COLs and jets, it is important to consider the broader context. The intensity of the subtropical and polar jets influences the distribution and characteristics of COLs throughout the seasons.

However, replacing jet intensity with anomalies in Fig.4 overlooks this relationship, potentially obscuring important insights. Our findings are supported by recent research, particularly a study authored by the editor, which demonstrates the presence of low-pressure anomalies associated with COLs during 'wet' years in Australia. These anomalies coincide with periods characterized by a stronger polar jet and amplified zonal-wave-3, which corroborates our findings.

<http://www.bom.gov.au/research/publications/researchreports/BRR-053.pdf>

We maintain confidence in the validity of the connection between COLs and jet intensity. We are somewhat unclear about what we are being asked to do, is anything further required to be modified in the paper or is a response to the editors comment enough. We could add a further comment in the paper concerning the uncertainties in the COL/jet relationship in view of the de-trending along the lines of our response above but it would be useful if we had specific guidance on what is required for the paper to be accepted.

Thank you for your understanding.

Authors