

Review of paper submitted to *Weather Clim. Dynam.*

Winter Jet Stream Clustering over Australia and its Role in Extreme Precipitation Events

by Anna Kuhn, Chenhui Jin, Elizabeth Ritchie, and Michael Sprenger

The paper focuses on exploring the variability of the wintertime jet stream over Australia using a K-means clustering approach and investigates its role in extreme precipitation events. The theme of the manuscript is well-suited for the journal *Weather and Climate Dynamics* (WCD). As the authors note, links between the jet and precipitation events are well studied in the Northern Hemisphere but barely explored for Australia specifically. A strong scientific contribution lies in its novel methodology to cluster the jet stream based on vertically averaged wind speed fields rather than using a zonal-mean jet index. The study is comprehensive, as the analysis of jet stream clusters combines QGw diagnostics, Eulerian weather features associated with Rossby wave breaking, and regional analyses of precipitation extremes. We think that the paper will be suitable for publication in WCD once the following issues have been addressed.

Major comments

1) We suggest restructuring parts of the introduction. It currently reads like a general review about jets and jet variability, and we felt that it might be overly detailed and in parts hard to follow. It is difficult for the reader to follow the red line and to identify the parts that are really central for the research done in this study. This is particularly so because the discussion very often changes between Northern Hemisphere and Southern Hemisphere studies. We recommend that the authors streamline the introduction by following the following principles:

- reduce the number of references and complexity of the text by not mentioning certain aspects / papers that are not essential for this study;
- reduce the switching between the two hemispheres – and, related to the point above, consider whether some aspects of Northern Hemisphere jets might not be relevant for this paper;
- try to better guide the reader, for instance with brief introductory statements about the purpose of the individual paragraphs and try to make sure that these themes then naturally lead to the key objectives of this study;
- group related studies and more explicitly discuss their similarities, differences, and, where relevant, contrasting findings.

2) L57: the authors write “there are conflicting results of the effect of a weaker or stronger STJ on precipitation”. We did not fully understand, what this conflict is. Please provide references to the studies reporting these conflicting results and briefly explain the nature of the discrepancy.

3) L538–539: The authors indicate that the QGw pattern does not align with theoretical expectations. Please elaborate in the discussion section on how this might affect the validity of the results. Similarly, in L447–450, it is mentioned that the results partially

contradict Risbey et al. (2009). Please elaborate on the implications and possible reasons for this.

4) In section 3.1, first paragraph the authors describe the clustering methodology, which we consider to be central to the validity of the results. The authors should show the figure for the Davies-Bouldin score with respect to the number of clusters at least in the supplementary material and explain if the first local minimum at $K=5$ is the absolute minimum; if there is a higher number of clusters with lower score, the authors should state why they didn't chose that number. It is especially important to justify this as the authors themselves state in lines 532-533 that their main limitation is the choice of the number of clusters. In general, it would be important to briefly discuss how the results may depend on the specifics of the clustering.

Minor comments

L149: Please clarify which aspects of the methodology follow Koch et al. (2006) vs. Banderier et al. (2025) mentioned in L102–103. The authors state in section 2.3 that they use the vertically averaged horizontal winds following Koch et al. (2006). However, in section 1, lines 102-103 the authors explain that their study will follow a similar approach to Banderier et al. (2025) which also uses vertically averaged wind fields. Is Koch's method compatible with Banderier's approach? This is not entirely clear to us from reading the manuscript.

Please ensure the manuscript title and the Supplement title are identical.

Lines 330-333: What was the subsample size for the random selection of EDP days and how many subsamples were drawn to reach this conclusion?

It is not entirely clear to us whether the main contribution of the paper is an increased understanding of jet dynamics over Australia (which in itself is a relevant scientific contribution), or whether there is a novel contribution in the methodology that could be of broader interest and applicability. If that is the case, the paper would benefit from clearly stating it in the conclusions.

The methodology is repeated several times over the paper (lines 208-209, lines 229-231). Rather than adding clarity, I consider that this makes the paper harder to read.

This review has been prepared by a group of four young researchers who participated in the EGU Peer Review Training in September 2026 as part of the WCD team. Heini Wernli coordinated this group, but the review is entirely based on the impressions and valuable input of the trainees.