

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

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1 | SUMMARY

In their manuscript, Kuhn et al investigate the day-to-day variability of the austral winter jet stream and its relationship with weather patterns, and specifically extreme precipitation over the Australian continent. They use a clustering approach based on daily and vertically averaged wind speed data to define jet configurations for austral winter, and then go on to analyze weather patterns associated with those configurations by compositing precipitation anomalies, as well as weather feature frequencies for the different configurations. The final step of the analysis relates jet configurations to extreme precipitation events over various major Australian cities. The authors find five jet configurations, and clear dominance of one to two configurations for extreme precipitation over the different cities.

I appreciate the manuscripts' aim to investigate austral winter jet variability and its connection to weather patterns and extreme events. The topic is well within the scope of WCD, and not only relevant but also under-covered in the communities' often Northern-Hemisphere-focused contributions. I particularly commend the authors for their analysis of the relationship between extreme precipitation and jet configurations, where the results show some interesting differences in jet configurations between locations, which also point toward different driving mechanisms and roles of the jet stream in the formation of extreme precipitation.

At the same time, I have several major concerns that need to be resolved before I could support publication in WCD. First, spatio-temporal scales, and the relationship between the literature and the authors' own results need more careful treatment. Second, several references do not fit the context in which they are cited. Third, I found it at times unclear how the authors justified the use of the terms "subtropical," "eddy-driven," and "merged" jets, especially the term "merged" is never properly defined. Lastly, some claims are not sufficiently supported by the results, largely because their significance is not assessed. I detail these concerns and suggest improvements in the General Comments below.

In my view, the manuscript in its current form does not allow readers to draw clear conclusions or to place the results in the context of the literature on synoptic-to-large-scale interactions between the jet and surface weather. Still, the core of the paper, especially the identification of different drivers of extreme rainfall across regions, is valuable, and I would be glad to see a clearer, more robust version published after major revisions.

2 | GENERAL COMMENTS

(A) Treatment of spatio-temporal scales,

especially with regard to previous literature. The literature on jet dynamics spans many spatio-temporal scales and this is always a challenge when writing introductions about jet-surface-weather interactions and relating ones own work to literature. I ask the authors to treat this more directly. For example, the manuscript contrasts results based on daily-mean 2D fields with a study of interannual jet variability whose index is based on vertically, seasonally and zonally averaged data (Gillett et al., 2021; l. 256). Although the authors partly step back from this statement later in the paragraph, I do not find the comparison appropriate. The comparison to Suresan et al. (2025) in l. 476 has the same problem, since that jet index is based on monthly-mean positions. Similarly, in l. 444ff, Lehmann and Coumou (2015) (seasonal scale) and Woollings et al.'s jet index (zonally averaged winds) are cited as context for a synoptic-scale analysis. The somewhat inverse problem occurs in the discussion, where the authors compare their Eulerian composite to Beebe and Bates' (1955) jet-streak-centered four-quadrant model.. These publications can still be discussed, but the authors need to state their scope and the scales they address. A clear treatment of scales would also help structure the introduction.

(B) Treatment of literature.

After finding one reference that did not support the point it was cited for, I checked further references and found more such

cases (examples below). I have not checked the entire manuscript, so the list may be incomplete.

line 256: Citation of Gillett et al (2021): They look at inter-annual variability of annual mean jet positions and some intra-annual variability, but all of zonally as well as vertically averaged jet. It is unclear to me how these results relate to the statement in line 2556.

line 541f: I am unsure about how this question relates to Bukenberger et al. (2023), which is not at all concerned with drivers of QG ω forcing, but rather looks at the effects of vertical motion onto the jet, and generally diabatic-adiabatic coupling.

Many of these mismatches appear related to concern (A), so addressing (A) may resolve part of this. Still, given these cases, I cannot be confident that the remaining references are used accurately. I ask the authors to go through them again to ensure that they represent previous work and its relation to their arguments well.

(C) Jet terminology (subtropical, eddy-driven, merged)

Lack of definition and proper discussion in the introduction, as well as unclear usage later on. The term 'merged jet' is used frequently but never defined. Definitions of the term differ between authors, and in my experience the term is mostly used on monthly-to-seasonal scales and is often defined dynamically (by what drives the jet) rather than by jet position. A clear definition is essential to place the statements in the literature. It is also needed to judge whether 'merged jet' denotes an identified dynamical state or merely that a single wind-speed maximum appears in the composites. Maybe the authors also mean 'superposed jet' rather than 'merged'? On the synoptic scale, the concept of superposed jets (as investigated in Winters et al. (2020)) is more commonly used and often defined as characterized by a steep, single-step pole-to-equator tropopause structure. Some relevant references to clarify the term 'merged jet' and its use might be Winters and Martin (2014); Winters et al. (2020); Harnik et al. (2014); Messori et al. (2021).

Furthermore, identifying merged/superposed states, as well as ED and ST jets, requires, to the best of my knowledge, more than the position of high-wind-speed areas. Since the vertical structure is not analyzed, I suggest rephrasing the results section so that references to STJ, EDJ or merged jets reflect what the analysis can establish.

(D) Unclear significance and causality

The authors make various claims about anomalies related to the jet configurations, often framing the jet as causal for surface weather. These claims sit uneasily with the large intra-configuration variability they report themselves. This variability could be shown explicitly, for example with relative measures such as the standard deviation over the mean wind speed. I was unable to gauge whether differences between clusters, and cluster-related anomalies (in precipitation and in the occurrence of weather features), differ significantly from sampling variability, and I ask the authors to test this. For precipitation, simple tests such as Student's t-test are a starting point. For weather-feature frequencies, comparing to climatological frequencies with confidence intervals would be helpful. Especially when it comes to comparing non-EPD to EPD-days for one specific point over Australia, I am interested in the change of variability. EPD days at a single grid point over Australia represent a strong spatial selection. Across all days in a configuration, the jet is probably less tied to that location, and so is jet-related precipitation. An analysis of variability would help to understand how much this contributes to the differences between the left and right columns in Figs. 5–9. I understand that many additional tests can clutter an analysis, but I encourage the authors to test at least the most important results.

The authors often state causality where I see co-occurrence, or sometimes also relationships in which the direction of causality is not clear. Some examples of this are pointed out in the specific comments, but I ask the authors to critically review the framing of their results throughout the manuscript and refrain from stating causality where it is not clearly shown by their own results or can at least be clearly deduced from the literature.

3 | SPECIFIC COMMENTS

Suggestions to change small formulations or words are written in *italic* font. A general point is the **use of tense in referencing**. References are sometimes cited in present tense, sometimes in past. I suggest the authors consolidate this.

3.1 | Abstract

Line 12 ff:

I find this sentence to be rather verbose and not so easy to understand. What does it mean that 'In each of these capital cities, a higher percentage of extreme precipitation events is associated with one or two of the jet configurations'? A higher percentage is associated with one or two than ... with no jet configuration/all others combined?

Line 14 ff:

Does this mean, that typical jet configurations / typical jet intensities are usually not associated with extreme precip?
Generally in the abstract, I would like to see more clarity about whether the aim of this work is to characterize jet configurations, or to link jet configurations to extreme precipitation (which the title suggests, and I feel after reading the paper, is at its heart). I would find it helpful if, before stating the first methodological approach in line 5, the core research question addressed in this paper was clarified.

3.2 | Introduction

L42 ff

In the following paragraph, the scale-topic is particularly pertinent. I would find it helpful if the temporal scale of variability were more clearly delineated, such that it becomes clear to which extent the present literature is complementary versus contradictory evidence with regard to the impact of the jet onto precipitation.

L46:

'to exhibit *fewer* and weaker meridional excursions'

L47:

'noted that the *seasonal mean* STJ exhibits'

L63ff:

you did not define merged jets before. I would suggest this is done, especially if it is used later on. Also: Are you referring to merged or superposed jet regimes? There are sometimes different understanding of the term 'merged jet', so a definition would be particularly relevant here.

L85:

'To our knowledge, no previous work has *characterized* the'

L94:

'be *insufficient* when it comes to'

L105:

'Moreover, we study how jet configurations are reflected in their effects'. This is an example of a type of phrasing the authors often use throughout the manuscript, where they imply a causal effect of the jet structure on the surface weather. However, causality is not analysed in this work. Even the QG-analysis is diagnostic, and does mostly show relations between lifting and sinking motion and the jet. I would therefore suggest the authors use more careful wording (i.e. 'relationship between the jet and surface weather' instead of 'effect of jet on ...' and so forth.

L120:

'role of jet stream': Similar to comment before this one: sounds very causal.

3.3 | Data and Methods

L123:

This dataset is used only for precip, right? If so, maybe state that immediately. For example, start the sentence with 'for analysing precipitation over land, we use ...'. Also, I would appreciate if you briefly mentioned how the analysis comes to be (observations + statistical modelling, if I understand the AGCD website correctly).

L139:

Consider stating which fields (instead of 'meteorological fields')

L141:

'31 km (...)': I'd switch the order here. E.g., 'It is available hourly with a grid spacing of .. , which is roughly 31km at the equator'.

L144:

I wonder why the authors do you not average hourly data? They accumulate total precip. from hourly data as well, if I understand correctly..

L147f:

The deviation from climatology is gridpoint by gridpoint, I assume?

L172ff:

From the context, I guess the Nabla-operator is generally horizontal and in pressure-coordinates. If so, I would suggest mentioning this at first occurrence (e.g. after eq. (2) or (3)).

L184:

I am wondering whether, in the end, we will look at the RHS of eq.2, or whether the authors solve the PDE to get to omega directly. Maybe the authors can make this more explicit.

3.4 | Section 3

193

The position of the EDJ, as named it here, is not really visible, and to my eye, only the STJ is clearly visible as a feature for itself (as far as one can discern whether something is EDJ or STJ just based on the latitudinal position).

L212ff:

Regarding the Davies-Bouldin score, I suggest the authors consider adding a brief explanation on what this score measures to illustrate what they optimize for. If they don't explain the score, I suggest consider replacing the sentence with a more qualitative statement (like 'trying to strike a good balance between ...').

Line 218ff:

I suggest switching the order here. The authors talk about changes in cluster occurrence before describing what characterizes the individual clusters. This made it very hard for me to understand what this means. Therefore, I would first go over the clusters qualitatively. In addition to that, I would consider giving the clusters short names that characterize them. Lastly, I suggest moving the current Fig. 2 to the appendix or supplementary material. It mostly shows that there is not much to be seen in terms of changes (which is nice to keep a note of, but – to me, not at the core of the study). One might even consider removing the discussion of temporal evolution entirely, as it is never picked up after its first mention.

Line 229f:

Questions I have on this part: How different is the intra-cluster variability (e.g. in wind speed, tropopause position) between the different clusters? For some clusters, I would like to better understand how the authors know that something is really the STJ / EDJ?

L284ff

I don't understand quite understand the reasoning here. If precip. is climatologically high, with little variability, this would not necessarily be the case, right?

Fig. 4 and respective analysis

I really like this plot, it's very illustrative already. Some analysis of significance of those anomalies would be helpful (for example a student-t-test to discern where anomalies are unlikely to be randomly different from climatology). I could imagine to only show anomalies in colour where they differ from zero significantly. With significance testing, statements such as the one in L300 ff would be better grounded. Moreover, I would find it helpful to relate the precip. anomaly patterns to the jet, so I would consider adding one or two wind speed contours to show the rough extent of the jet.

3.5 | Section 4**L310**

full stop after 'wet'?

L396ff

How do we know the cluster is really characterized by a merged jet regime? Also, how do we know that wind speeds are slower and the jet broader, rather than the jet just being at different latitudes and thereby smearing out the region where the mean wind speed is large? Again, the argument could be strengthened if we knew more about the variability of the EPDs.

3.6 | Discussion

In the discussion, I like that the authors further narrow down your findings to two jet configurations. I would appreciate if they stated early on, which clusters they consider part of each pattern upfront. (e.g., in line 434ff).

Line 444ff

In this discussion paragraph, I find the citation and spatio-temporal scale issue from my major comments very pertinent. First, with Lehmann and Coumou, who don't look at the jet itself, and then with Woollings et al, who look at diagnostics of the jet that are clearly different from what the authors considered. I would like to see this re-worked.

L475ff

Here, the argument seems to be one of enhanced surface fluxes due to enhanced wind speeds at the surface. First, there is a typo here. The IVT is not along, but perpendicular to the surface pressure gradient. Second, I was wondering about the extent to which this explanation hold up (i.e., do the stronger winds explain the enhanced IVT?). I was also thinking about boundary-layer temperature: Maybe these events coincide with anomalously warm SSTs? Lastly, the usage of Suresan et al. (2025) could be more careful. I see the link that the authors draw to this study, but would like them to be more nuance. Suresan et al base there analysis on a jet index that comes from monthly mean wind data, so their perspective is really large-scale. While I agree some of the basic process understanding can be transferred to the present synoptic-scale study, the different temporal scales should at least be acknowledged.

L478f

Citation?

3.7 | Conclusion

line 538f:

I think one cannot have 'theoretical expectations' from the 4-quadrant model based on geocentric (rather than jet-streak centered) composites. We do not know how longitudinal position, curvature, and orientation vary within the configurations. Comparing results of this study to Beebe and Bates hence seems artificial for me. If one wants to get to the non-object centered limitation of the study, which I think is a good idea, one could simply state that the current approach does not allow for a more in-depth analysis of lifting processes because it is not object-centered.

line 541f:

See general comments: not clear how this question relates to Bukenberger et al. (2023).

3.8 | Figures

Figure 1

I find the orange contours a bit hard to identify over the colormap over Australia. Maybe it's possible to find a map or contourcolour that makes this easier to read?

4 | TECHNICAL COMMENTS

4.1 | Results

L251

'Similar to' ?

Line 266f:

you write 'Overall, this behavior is consistent for all times TF2 and across pressure bins (not shown)'. I think there is a 'during' missing here.

REFERENCES

- Bukenberger, M., Rüdüsühli, S. and Schemm, S. (2023) Jet stream dynamics from a potential vorticity gradient perspective: The method and its application to a kilometre-scale simulation. *Quart. J. Roy. Meteor. Soc.*, **149**, 2409–2432.
- Harnik, N., Galanti, E., Martius, O. and Adam, O. (2014) The anomalous merging of the african and north atlantic jet streams during the northern hemisphere winter of 2010. *J. Climate*, **27**, 7319–7334.
- Messori, G., Harnik, N., Madonna, E., Lachmy, O. and Faranda, D. (2021) A dynamical systems characterization of atmospheric jet regimes. *Earth System Dynamics*, **12**, 233–251. Publisher: Copernicus GmbH.
- Winters, A. C., Keyser, D., Bosart, L. F. and Martin, J. E. (2020) Composite synoptic-scale environments conducive to north american polar–subtropical jet superposition events. *Mon. Wea. Rev.*, **148**, 1987–2008.
- Winters, A. C. and Martin, J. E. (2014) The role of a polar/subtropical jet superposition in the may 2010 nashville flood. *Wea. Forecast.*, **29**, 954–974.