

## ***Interactive comment on “A New Index For The Wintertime Southern Hemispheric Split Jet” by Stella Babian et al.***

**Anonymous Referee #1**

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This manuscript introduces a new index for the Southern Hemisphere (SH) winter split jet. The split jet is an important feature of SH winter climate which requires greater understanding due to its importance in understanding a globally-relevant part of the climate. Winter (July-September) monthly zonal wind data from a reanalysis dataset (ERA-Interim) is used to define an index based on principal component analysis. The rationale is that there is some disagreement between previous locally-defined split jet indices in terms of conclusions on links between split jet variability and large-scale modes of climate variability. The creation of a new hemispheric-scale index is motivated by the need to understand these disagreements.

With regard to the manuscript itself. I found it difficult to follow and have a number of concerns about the methodology and rationale which are crucial the main conclu-

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sions. These are listed below and represent major comments that would need to be addressed for publication in ACP.

Major comments: I'll start with a recommendation in light of the below comments. My feeling is that this work would be easier to interpret and present clearly if the methodology and questions were reversed. By reversed I mean that the present approach of developing an EOF-based index that correlates with previous split jet indices could be reversed and instead the EOF analysis could be used to investigate in detail the linkages between existing split jet indices and large-scale patterns of atmospheric circulation variability. This could help to solve a number of issues listed below relating to the methodology and interpretation. In fact, in light of the major points below I find it difficult to see another option for raising the manuscript to the necessary standard. The main major points are as follows:

1. There are a number of concerns about the EOF/PC analysis and I remain to be convinced that this is an appropriate tool for developing an index of the split jet (but see above note about reversing the analysis / questions).
  - a. The rationale for the precise combination of PCs used for the PSI index is not explained and seems arbitrary (equation 5 on lin 25 of page 8). Why was this specific linear combination chosen?
  - b. The actual EOF patterns are not shown, which makes it very difficult to judge the relevance of these patterns for the split jet region and possible variability with other aspects of the SH climate system.
  - c. The vertical pressure level seems to be chosen on the basis of highest correlation with previous split jet indices rather than a specific dynamical reason. It is important to explain why 150 hPa is more appropriate than the lower 200/300 hPa levels used previously.
- d. I'm not an expert on EOF analysis, but it seems problematic to me to just perform

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this on winter months since presumably large jumps can occur between the end of one winter and the beginning of another that could introduce artefacts into the analysis of month-to-month variability. An explanation of why this approach is acceptable or what was done to avoid this is important.

2. The use of a combination of PC2 and PC3 used in the PSI index are potentially problematic in discussion of links with ENSO (section 4.2). The context for this is that in tropospheric geopotential height, the 2nd and 3rd PCs are associated with two variants of the so-called Pacific South American (PSA) pattern (e.g. see Cai and Watterson, 2002), which are often interpreted as representing Rossby wave trains from the tropics (e.g. Karoly, 1989).

3. The relatively low correlation between the PSI index and previous split jet indices is a concern (Table 2). Correlations of around 0.7 only explain 50% of the variance and to me this indicates that the new index could be mixing up a number of other correlated dynamical features that exist on the hemispheric scale. In particular see the above point about PSA-1 and PSA-2. It seems possible that the PSI captures some sort of PSA-related patterns, which we know are highly correlated with the split jet region (Figure 1 of Cai and Watterson).

4. Statistical significance should be included on the composite anomaly maps shown in Figures 5 and 6, otherwise it is difficult to judge which aspects of the maps to focus on.

5. The manuscript needs to be reviewed by a fluent English speaker (or alternatively one of the many available English review services) since it is not currently of a publishable standard.

Specific comments

Page 1, line 1: I suggest mentioning explicitly that the split jet is evident in mid-latitude westerly/zonal winds.

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Page 1, line 9: Similarly, it should be mentioned here that the principle component is defined from the zonal wind field.

Page 1, line 15: “relation” -> relationship.

Page 1, line 19: “an SH” -> a SH.

Page 2, line 2: “minimum wind speeds in the westerlies of the upper level flow” -> weak upper-level westerly winds

Page 2, line 9: Mention somewhere here that the AAO is now more commonly referred to as the Southern Annular Mode (SAM), since many (most?) readers will be most familiar with the SAM terminology.

Page 2, line 26: The meaning of “which confines with the relationships between” is not clear to me. This needs to be re-worded.

Page 2, lines 34-35: I disagree that inconsistent relationships indicates deficiencies. I would prefer to say that these differences need to be understood and motivate the development of an alternative index to help in this understanding. Also, the concept of a more hemispheric definition than the previous regionally-defined indices represents a key novelty in the authors' proposed index.

Page 3, lines 1-5: I'm not sure what the main point being made here is. This should either be made clearer or possibly the paragraph could be removed. Also, it important to note that the impact of stratospheric ozone depletion in the lower-troposphere is highly seasonal (mainly summer) and not significant in austral winter.

Page 3, line 10: Note that the AAO/SAM is not specifically a low-frequency mode of variability, although it will contain a component of externally-driven low-frequency variability and trends.

Page 4, line 1: “dissolved” -> resolved.

Page 4, Section 2.2: The use of different terminology in the title (EOF analysis) and

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main text (principal component analysis) is confusing and should be clarified.

Page 4, line 12: I'm not an expert on EOF analysis, but it seems odd to me to just perform this on winter months since presumably large jumps can occur between the end of one winter and the beginning of another that could introduce artefacts into the analysis of month-to-month variability. An explanation of why this approach is acceptable is important.

Page 4, line 18: I don't understand what is being described in the sentence starting "Afterward, the monthly ...". Maybe this needs clarification and/or re-wording.

Page 4, line 19: "the relationship" -> the linear relationship

Page 6, line 23: "isobar" -> "pressure level"

Page 8, line 1: It seems odd to mention the PSI here before it is defined in the next section. I would prefer to keep the focus on the individual PCs in this section.

Page 8, lines 18-19. I would remove "This gives rise to the assumption" and simply state that splits are less correlated to PC1 than PC2 and 3.

Page 8, line 22: Further explanation of what is meant by a "dynamical PC based index" is needed.

Page 8, line 23: It's not clear how this particular linear combination was decided on. This needs to be explained.

Page 8, line 29: How well correlated are the various indices with each other? A correlation of around 0.7 between the PSI and these indices explains only half of the variance and therefore could potentially capture quite different aspects of variability.

Page 9, first paragraph of section 4.1. It is important to add statistical significance to the composite difference plots. Also, it would be useful for the reader to see what these composites look like for the split jet indices rather than not show them.

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Page 10, main text discussion on individual PC2 and PC3 composites: Given that these show rather different composite patterns, it would be useful to give an explanation of how the combination of the two provides an appropriate PSI.

Page 12, Section 4.2: As mentioned above, I doesn't seem appropriate to categorise the AAO/SAM as a low-frequency climate mode due to the importance of jet vacillations in its existence.

Page14, discussion of ENSO links in main text: In the lower troposphere the 2nd EOF of monthly geopotential height often shows a pattern referred to as the Pacific South American Pattern (PSA). I would encourage the authors to comment on how this might relate to their results and also PC2 in zonal winds at 150 hPa.

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